IPCC Working Group I Fourth Assessment Report

Expert Review Comments on First-Order Draft

Chapter 2

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Please note that under IPCC procedures authors are required to take account of all substantive review comments in both review rounds. Thus responses to individual comments may be influenced by comments from other reviewers.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1	A	0:0	0:0	The partition of CO2/CH4 results between chp2 & chp7 makes it much less clear than in IPCC201. Links between the two parts should be clearly indicated [Philippe Bousquet]	Accepted, will be improved upon
2-2	A	0:0	0:0	Overall, this chapter is well written and well referenced. However, I do have a number of specific criticisms. [Patrick Hamill]	Noted
2-3	A	0:0	0:	I would like to compliment the authors on an excellent draft. You have succeeded in covering a broad span of material extremely well, and provide many significant advances on the TAR. The coordination across chemistry, aerosols, and radiative forcing has worked better in this draft in many respects as compared to the TAR. I hope that my comments may help the authors improve the readability of what is already a very fine chapter. [Susan Solomon]	Noted
2-4	A	0:0	0:0	If you reference the comments I made on the zeroth-order draft, you will note that I recommended a couple of simple conceptual figures to show the relationship between accumulating emissions, RF at a point in time, and surface temperature change. You could do this for a long and short-lived source, etc. I still think this is a good idea. This draft is an improvement in terms of the caveats for RF, but as noted in (almost all) of my comments, I think you can still do better. Nonetheless, as a former lead author on one of these documents, I know how difficult it can be to pull everything together, respond reviewers, etc. --so I commend you for your efforts thus far. Please take my comments as constructive criticism. [Ian Waitz]	Noted, figures will not be added but text will be clairified
2-5	A	0:0		I am sorry to say I found a large number of ambigous, unclear, unnecessary repetitive and sometimes conflicting statements in this chapter. I will try to give some examples below. [Florens De Wit]	Noted, we don't agree
2-6	A	0:0		In this chapter it is only weakly acknowledged that radiative forcing is limited as a metric for climate change impact. Let me state a few critical points: Not nearly all relevant processes in climate are radiative. In fact the processes responsible for the actual meteorological observations - i.e. Atmospheric flow, convection, advection on small scales – are hard, if not impossible to represent in a global average radiative forcing (RF) and a single Climate Sensitivity (CS). Climate sensitivities are different for different forcing mechanisms. This seriously reduces the usefullness of RF as a metric for climate change impact estimation, as it was intended	Alreay discussed sufficiently in chapter –see section 2.8

No.	Batch	Page:line From	Page:line To	Comment	Notes
				(I suppose) to be a simple metric. Different CS for different mechanism also could point to a underlying CS function that is nonlinearly dependent on the magnitude of forcing. Another interpretation of the different CS is that effects might have different spacial and temporal distributions, making their impact different, when their absolute magnitude is similar. The question remains whether a single CS is sufficient to model the impact of a forcing over time, while both forcing and CS might quite possibly change over time. All in all the use of a single forcing and a single sensitivity to represent the impact of forcings that might differ in space and time and will impact on the processes in the climate system in a nonlinear way over time as well, seems a lot less usefull as is suggested. Unfortunately this would imply that the entire chapter needs to be rewritten and all mention of radiative forcing in the rest of the report would need reconsideration as well. [Florens De Wit]	
2-7	A	0:0		Overall, this chapter provides an excellent summary of knowledge of radiative forcings. However, the treatment of AEROSOL FORCINGS is problematic in some respects. (i) There is a tendency to present detailed results from numerous studies rather than a synthesis of current knowledge. (ii) There is a tendency to claim that uncertainties in aerosol forcings have been substantially narrowed since the TAR. Regarding (i), it would be better to delete much of the detail, referring instead to the several assessment/review papers that have emerged recently (e.g. Kinne et al., 2005; Yu et al., 2005; Bates et al., 2005.) Regarding (ii), reduced uncertainties is dubious and not supported by the cited documents. In particular, the AeroCom project has revealed that the ranges among current models for global-mean aerosol component mass and optical depth span factors of 2 to11 (Kinne et al., 2005, Table 4). Estimates of the semi-direct and cloud-lifetime effects of aerosols have been developed since the TAR, and these would add to the overall magnitude and uncertainty of aerosol forcings; however, these effects have been artificially removed from the category of "forcing" and put into the category of "feedback". This is a dubious decision, as discussed by Steve Schwartz in his comments on this chapter. While it provides a great wealth of well-organized detail, the chapter lacks an adequate synthesis of knowledge with respect to aerosol forcing. An appropriate synthesis would run as follows: "Forward calculations of aerosol radiative forcing continue (as in the TAR) to have enormous uncertainties and to dominate the overall uncertainty in total anthropogenic forcing. In particular, forward calculations of aerosol forcings do not provide adequately constrained climate model inputs for the purpose of determining climate sensitivity from the temperature record (Chap. 8) or projecting future climate (Chap. 10). For both of	Noted, in the SOD it is less focus on the reduced uncertainties and the LOSU is changed to low for the direct aerosol effect. Further, the best estimate is changed and the uncertainty range increased. However, we still describe that there has been substantial progress in aerosol science: Number of multicomponent models increased. Documentation of differences to observations harmonised. Positive and negative forcing is not independent because aerosol species are dispersed by characteristic transport efficiency in models. Total aerosol effects are better constrained through satellite observed optical thickness. Closure studies, Radiation forcing efficiency from satellites and models. Not talking about the future of aerosol forcing in this chapter.

No.	Batch	Page:line From	To	Comment	Notes
				these purposes, therefore, many modeling groups since the TAR have developed and implemented inverse calculations of aerosol forcing, as discussed in Chap. 9. While this has been a valuable development, it is important to point out that inverse (unlike forward) calculations do not provide information on individual aerosol forcings and, moreover, are only valid under the assumptions that observed climate changes are accurate, that climate models accurately simulate the relationship between forcing and transient climate response, and that no other factors such as larger than expected natural variability are the actual cause of the observed climate change. Thus, continued efforts to improve the forward calculations of aerosol forcing are warranted despite the evident difficulty. Substantial improvements over the next several years may very well occur, owing to a new generation of aerosol-sensing satellites, the data from which is just beginning to submit to integrated analysis with respect to this question. We caution, however, that improved observational knowledge will not necessarily result in reduced uncertainties, since it is also possible that new complexities will be discovered and that these will have the effect of increasing uncertainty estimates. Over the course of this century, aerosol forcings are expected to diminish in importance with respect to the increasingly dominant, positive forcings from ever-accumulating anthropogenic greenhouse gases." [Theodore Anderson]	The cloud albedo effect and any indirect effect is stressed in the current representation. There is a coordination with chapter 7 with regards to aerosol-cloud interactions.
2-8	A	0:0		Pointing out that the rate of growth of methane has declined in recent years is an example of a balanced approach (less greenhouse warming). Other example include cooling effect of aerosols, net deforestation leading to increased albedo. [Richard Anthes]	No comment required
2-9	A	0:0		This is a very well written chapter. What I particularly liked about it is that it is often quite tutorial. As a result I learned a lot when reading the chapter. I haven't have time to write comments on the whole chapter so, as you will see below, my comments just stop at some stage. [Greg Bodeker]	NCR
2-10	A	0:0		Some language expressing the authors' opinions is used throughout. This leads to ambiguity. For example, some uncertainties are said to be 'high', calculations are said to be 'convenient', or demonstrations are said to be 'clear'-- these are relative terms and may not be agreed by all readers. Other specific examples will follow. In general I urge more quantification of such relative effects, or removal of these words, which are sometimes unnecessary. [Tami Bond]	Will stick to IPCC pre defined language as much as possible and aviod sujectiveness
2-11	A	0:0		Writing needs to be read by another party for purposes of proof-reading. The document contains run-on sentences, unclear phrasing, and insufficient use of punctuation. I will	It will be read by many parties

No.	Batch	Page:line From	Page:line To	Comment	Notes
				note some comments below but cannot identify all such instances. [Tami Bond]	
2-12	A	0:0		To enhance readability by non-experts, I strongly suggest minimizing the use of acronyms. Where an acronym is used only two or three times, it can be spelled out each time. This increases the reader's comprehension and does not take much extra space. [Tami Bond]	Accepted. Will reduce acronyms
2-13	A	0:0		Differentiation between forcing and feedbacks should occur earlier in the chapter-- near the beginning. A clear definition used for purposes of this report should be given, if possible. This would inform later discussions on whether individual mechanisms were forcing or feedbacks. [Tami Bond]	Accepted
2-2667	B	0:0		First of all I commend the authors for a well written and comprehensive chapter. I have a couple of comments on the chapter itself and a couple of other comments on the consistency with chapter 7. [Olivier Boucher]	Accepted
2-2668	B	0:0		Overall comment: I am not sure the authors have sticked to a consistent methodology to assess the uncertainties. I appreciate that uncertainties cannot always be assessed and cannot always mean the same. However a bit more care should be given to this if in the end the different RFs are to be summed up.For instance, it seems that the uncertainty for the aerosol RF is the s.d. from a range of model results (eg AEROCOM). Why is it then argued (page 63, line 6) that this represents a 90% confidence interval? [Olivier Boucher]	Accepted, will br claified
2-2669	B	0:0		Overall comment: I am a bit dubious that the semi-direct and second indirect effects can be called feedbacks. I appreciate why the authors do not treat them as a RF; however I would like to make the point that what is not a RF is not necessarily a feedback. A feedback should involve a loop which is closed (larger T, larger water vapour mixing ratio, larger GH effect, larger T). There is no such loop for the second aerosol indirect effect. So it is an indirect effect, not a feedback. More effort could have gone into trying to incorporate it into the climate efficacy concept. For instance the cloud lifetime effect has virtually been written off (neither accounted for as a forcing, nor accounted for in the efficacy of the cloud albedo effect). [Olivier Boucher]	Accepted. Will define better and upfront. Will coordinate with Chap. 7.
2-2670	B	0:0		Consistency with chapter 7: there are 3 items to be discussed with chapter 7: trends in OH where there seems to be some repetition and similar material in the two chapters, the contribution of deforestation to present-day CO2 concentrations (what would be present-day CO2 if there hadn't been any deforestation since let's say 1750? it would be nice to have a number), chapters 2 and 7 seem to have slightly different appreciation on the trend	Accepted airborne fraction discussion shifted to chapter 7 + deforestation and landuse changes impacts on CO2 added

No.	Batch	Page:line From	Page:line To	Comment	Notes
				in the airborne fraction (no trend in chapter 2, positive trend in chapter 7 as stated page 3, line 34.). The discussion of the aerosol indirect effect is roughly consistent between the two chapters (apart from my concern about the cloud lifetime effect). [Olivier Boucher]	
2-14	A	0:0		TSU NOTE: Please see supplementary review material [Philippe Bousquet]	Noted
2-15	A	0:0		Ch 2 would therefore benefit from a much closer analysis of aerosol and ozone temporal changes. In particular, it needs to consider: (a) whether there are robust arguments and physical principles to support the assertion that we understand the temporal pattern of aerosol RF; (b) whether the aerosol RF could have a very different temporal pattern within the envelope of the inter-model uncertainty range (the error bar in your excellent bar charts). For example, how certain are we that the forcing levelled off since 1980? What is the hard evidence? I would say, given the huge number of genuine uncertainties listed in section 2.4.6 you are not in a position to say that you understand the temporal pattern in forcing and T response; (c) adding further figures to supplement fig 2.9.3. Currently I think 2.9.3 is inadequate to underpin what is done in ch9. As a minimum it is necessary to add an envelope of uncertainty around each forcing curve. At present, we have an excellent forcing bar chart with large uncertainty ranges for aerosol RF and a graph of temporal forcing that gives the impression that these estimates are error-free. [Kenneth Carslaw]	Noted, Temporal changes of aerosol forcing are not well known. The uncertainties involved in the temporal changes is very limited available in the published literature. A better description of the uncertainties in the temporal changes in the aerosol forcing is given.
2-16	A	0:0		My comments are mainly on aerosol part of this chapter, especially on the aerosol direct forcing, because this is the area I am more familiar with. Aerosol forcing has very different spatial and temporal scale than LLGHG. Adding the RF(LLGHG) and RF(aerosol) together to have a total "anthropogenic forcing" is misleading and could conceal the roles of aerosols on climate, especially on regional climate. [Mian Chin]	Noted, Maps of forcing and even surface forcing are there. In the chapter it is given emphasis to the efficacy for the comparison of the global mean climate response to various RF. The point will be made in terms of the forcing.
2-17	A	0:0		Aerosol comprises multiple components which are known to have different physical and optical properties thus different forcing efficiency. The RF of aerosols should be assessed at component level. Again, a "net" aerosol forcing value is misleading such that the positive and negative forcings from different types (e.g., BC and sulfate) cancel each other to give an apparent small forcing value. This treatment has direct policy implications and should be carefully dealt with. [Mian Chin]	Noted, a total direct aerosol RF is given with change in the best estimate and with a larger uncertainty. Further, in SOD it is now focused more on new available studies of the direct aerosol RF which is based on observations, where the assessment at a component level, though desirable, is very difficult and not robust yet.
2-18	A	0:0		The terms used in this report should be clearly defined at the beginning of this report, i.e.,	Accepted, a better discussion of RF and

No.	Batch	Page:line From	Page:line To	Comment	Notes
				"radiative forcing", "radiative effects", or "radiative perturbation". The concepts of these phrases, especially "radiative forcing", are used inconsistently throughout the chapter. Examples: • At the tropopause or at the top of the atmosphere? The term "radiative forcing", or RF, is defined on page 2-7 as "the change in net irradiance at the tropopause…" and yet later on in the text all the aerosol RF are presented as the values at the top of the atmosphere. • Anthropogenic only or not? On page 2-26 the aerosol "forcing" is differentiated from "effect" as the "forcing" is the radiative effects from anthropogenic components only while the "effect" is the sum of radiative effects from both anthropogenic and natural aerosols. However, the effects of natural sulfate (from DMS and volcanoes), organic carbon (from vegetation and natural wildfires), and black carbon (from natural wildfires) components are all included in the term "RF". [Mian Chin]	radiative effect as well as difference between RF at TOA and tropopause is now given. The use of RF is only used for anthropogenic activity.
2-19	A	0:0		Another inconsistency is related to how the aerosol forcing components are separated. They should be either separated by chemical composition (e.g., sulfate, organic carbon, black carbon, dust), or by origin (e.g., fuel combustion, biomass burning, land change desertification). However, the current classification is confusing and not consistent. For example, "sulfate aerosol" (page 2-29) refers to a chemical compound that contains SO4= originating from anthropogenic, biogenic, and volcanic sources; "OC from fossil fuel" (page 2-30) and "BC from fossil fuel" (page 2-31) are the chemical species of OC and BC from one particular source. In contrast, "biomass burning aerosols" (page 2-32) are not a particular chemical species but a mixture of them (mostly OC, BC, and some sulfate) from fires with both natural and anthropogenic origins. [Mian Chin]	Rejected, Citing of different categories has been done to make different results comparable. The split of carbonaceous aerosols is problematic. SO4 from biomass burning is in SO4 (to our knowledge). Inconsistency is resolved partially in total aerosol forcing !
2-20	A	0:0		Aerosol surface forcing is largely overlooked in this assessment. Because the surface forcing is directly linked to the surface energy budget and temperature change, it should be included in this report. [Mian Chin]	Accepted, a figure with the surface forcing for aerosols will be included, or alternatively, will be emphasized in the text.
2-21	A	0:0		I strongly disagree with the conclusion that the level of scientific understanding is "medium" for aerosol direct RF. The current issues include (a) all the forcings values are still model-based, and there are very large discrepancies among the models in terms of emission, composition, component mass, scattering and absorbing aerosol optical depths, vertical distributions, and mixing states, (b) global aerosol optical depth retrievals from satellites with enough accuracy have just recently become available but uncertainty level is still high, (c) the radiative fluxes derived from satellite data have a large range of values, (d) we don't even know the composition of OC and the formation mechanism of the secondary organic aerosols, let alone their physical and optical properties and	Accepted, LOSU changed to low and the uncertainty range is increased.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				radiative forcin. The understanding level is at the best still "low" if not "very low". More models and longer observation records since TAR are necessary, but not sufficient, for a higher level of scientific understanding. [Mian Chin]	
2-22	A	0:0		In fact, there are several places in the document where the statements directly contradict with the conclusion of the understanding level being "medium": • Page 2-4, line 45-46, "a combined RF is given as -0.2 ± 0.2". This is 100% or a factor of 2 uncertainty (and the uncertainty is actually underestimated), which is very large. • Same page, line 54, "the RF of separate aerosol species is less certain than the combined RF". • Page 2-29, line 13-14, "Their (anthropogenic emissions and the resulting burdens) computation remains uncertain". • The statement on page 2-26, line 39-40, "model studies…that appear consistent with observational estimates" contradicts with the statement on page 2-36, line 10-11, "models appear to under estimate the aerosol forcing by 20-50%". • Page 2-35, line 3-5, "the associated uncertainties for each individual component suggested that…the overall uncertainty of aerosol forcing would be very large through uncertainty propagations". [Mian Chin]	Accepted, LOSU changed. New observational based studies give the total direct aerosol RF further strengthen the view that the uncertainty in the total direct RF is smaller than the combined RF for individual aerosol components
2-23	A	0:0		Since the conclusions and evaluations given in this report will provide important guidance to not only future scientific research directions and priorities but also the policy making processes and fundings, we should be very careful and very thorough in making those assessments and summaries that will not mislead the scientists, government officials, and the public. [Mian Chin]	Noted, LOSU changed and uncertainty range increased.
2-24	A	0:0		general comment: congratulations to the authors for this chapter. It is very complete, with a lot of relevant reminders and new additions of information. Sometimes it is getting complicated when too much details are provided. [Cathy Clerbaux]	Thanks
2-25	A	0:0		These comments are mostly minor. Although I think some mention of water vapor is important. I think the writers have done a very good job, and should all be proud to be associated with the final report. [Michael Coffey]	Accepted. Thanks
2-26	A	0:0		The title of Chapter 2 is "Changes in atmospheric constituents and in radiative forcing". These two aspects of the chapter do not seem to have received equal weighting in the chapter. For example, in the Executive Summary, atmospheric constituent changes are summarized but it is the radiative forcing implications that are emphasized. In the main	Accepted more discussion of gases added and more graphs of timeseries ..statement included on the underpinning importance of high

No.	Batch	Page:line From	Page:line To	Comment	Notes
				body of the chapter the CFCs, HCFCs and HFCs do not receive much attention (in contrast to CO2, CH4 and N2O) and their long term changes are not shown in graphical form. Instead reference is made to the IPCC-SROC report. That report is unlikely to receive the attention given to the entire IPCC report. In addition it should be noted that the HFCs in particular are typically not discussed in any detail in the quadrennial UNEP/WMO ozone reports. I recommend adding some graphs of the ODP gases and the HFCs [Derek Cunnold]	quality measurements
2-27	A	0:0		Use of acronyms is excessive and detracts from readability [Robert E. Dickinson]	Accepted, will reduce
2-28	A	0:0		In places reviews observed trends without making use of similar materials elsewhere or more appropriate for chapter 3 [Robert E. Dickinson]	Rejected. We will take chapter 3 material on cosmic rays and contrails
2-29	A	0:0		1. Pleased to see dropping of terminology of 1st and 2nd indirect effects in favour of albedo and lifetime effects, although I fear we know nothing about true lifetime effects and little more about LWP and cloud fraction response to aerosol increases. Nevertheless, I don't have a simple, alternative expression for what I would consider cloud response to aerosol in the coupled system with feedbacks. What folllows is a number of general comments, a list of references, and then specific suggestions for changes. [Graham Feingold]	Noted
2-30	A	0:0		2. 2.4.6.1.1 The tone of this section is problematic. Although new surface based remote sensors have been used to address aerosol-cloud interactions, it has been shown, by some of the same authors who applied them, that they may not be adequate for addressing the albedo effect quantitatively. E.g. Feingold et al. (2003) showed that the albedo effect could be detected at a ground-based continental site, but Feingold et al. (2003) pointed to the difficulties of quantitative assessment using aerosol optical depth (or extinction) as a proxy for CCN. There is little reference to regional and global satellite studies that have attempted to assess the albedo effect (e.g., Nakajima et al. 2001; Breon et al., 2002) and the discussions of how well they can do this due to numerous issues raised by Feingold (2003) and Rosenfeld and Feingold (2003). Specific suggestions for changes on this issue (and other general issues) are made below. [Graham Feingold]	Taken into account; as changes are made throughout 2.4.6
2-31	A	0:0		3. It seems clear to many that the satellite measurements of the albedo effect are not simply an albedo effect, according to the strict definition of Twomey that albedo changes are assessed for clouds of similar LWP. None of the satellite studies stratify by LWP and therefore the response of albedo (or drop size) is an ambiguous mix of change due to aerosol and change due to change in LWP, and other bulk cloud properties like cloud	Taken into account

No.	Batch	Page:line From	Page:line To	Comment	Notes
				fraction (Schwartz et al. PNAS, Feingold 2003, recent work by Penner and coauthors). This is mentioned on Pg 2-38 but I felt treatment was too brief. [Graham Feingold]	
2-32	A	0:0		4. Are the GCM studies addressing the albedo effect indeed addressing a pure albedo effect? i.e., are they stratifying their output for clouds of similar LWP? (I'm not familiar enough with the literature to answer this.) If not, don't they fall into the same category as cloud lifetime studies in the sense that the clouds are viewed as dynamical entities with changing water content? [Graham Feingold]	Noted; GCMs results correspond to albedo only, when so reported.
2-33	A	0:0		5. There are a few references to the importance of aerosol composition in determining the albedo effect. Many of these studies have looked at limited effects (e.g. surface tension) in isolation, and often in an equilibrium sense, whereas more recent studies have considered the various composition effects in unison, and in a kinetic framework (Ervens et al. 2005). The latter study suggests a significantly weaker sensitivity to composition than the earlier studies had suggested over broad parameter space. [Graham Feingold]	Taken into account; reference included; not all previous studies were equilibrium ones.
2-34	A	0:0		6. There have been attempts to rank the relative importance of aerosol size distribution and composition effects for the albedo effect. A study by Feingold (2003) showed that size distribution parameters were significantly more important than composition in determining the albedo response. [Graham Feingold]	Taken into account. This could depend on locations where measurements are taken.
2-35	A	0:0		7. There should be more explicit mention of the fact that clouds are so poorly resolved by GCMs and that many of the parameters that drive the albedo response are not resolved (e.g., convection, updraught velocity) [Graham Feingold]	Taken into account
2-36	A	0:0		8. Regarding the cloud lifetime effect. The report steers clear of attempting an RF estimate of the cloud lifetime effect – and perhaps wisely. New results have shown that even the sign of the response of LWP to changes in aerosol is in question (for stratocumulus: Jiang et al. 2002 – already referred to, Ackerman et al. 2004; Lu and Seinfeld 2005; and for cumulus clouds, Xue and Feingold 2005; Jiang and Feingold 2005). The Han et al. (2002) study is another example of this. Mention of this would seem appropriate. [Graham Feingold]	Noted; discussion moved to Ch. 7
2-37	A	0:0		9. Regarding the usage of efficacy, I wondered why the sign of all the values are positive in Fig 2.8.1. Shouldn't the normalisation by CO2 forcing also reflect the change in sign? E.g., shouldn't the cloud albedo efficacy be negative? [Graham Feingold]	Incorrect interpreation> Howeer, section reworded for clarity

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-38	A	0:0		References: Ackerman, A. S., M. P. Kirkpatrick, D. E. Stevens, and O. B. Toon, The impact of humidity above stratiform clouds on indirect aerosol climate forcing, Nature, 432,1014-1017, 2004. Breon, F.-M., D. Tanre, and S. Generoso, Aerosol effect on cloud droplet size monitored from satellite, Science, 295, 834--838, 2002. Ervens, B., G. Feingold, and S. M. Kreidenweis, 2005: The influence of water-soluble organic carbon on cloud drop number concentration. J. Geophys. Res., 110, D18211, doi:10.1029/2004JD005634. Feingold, G., 2003: Modeling of the first indirect effect: Analysis of measurement requirements. Geophys. Res. Lett., 30, No. 19, 1997, doi:10.1029/2003GL017967. Feingold, G., W. L. Eberhard. D. E. Veron, and M. Previdi, 2003: First measurements of the Twomey aerosol indirect effect using ground-based remote sensors. Geophys. Res. Lett., 30, No. 6, 1287, doi:10.1029/2002GL016633. Jiang, H., G. Feingold, and W. R. Cotton, 2002: A modeling study of entrainment of cloud condensation nuclei into the marine boundary layer during ASTEX. J. Geophys. Res., 107, D24, 4813, doi:10.1029/2001JD001502. Jiang, H., and G. Feingold, 2005: The effect of aerosol on warm convective clouds: Aerosol-cloud-surface flux feedbacks in a new coupled large eddy model. J. Geophys. Res., in press. Lu, M.-L., and J. H. Seinfeld, 2005: Study of the aerosol indirect effect by large eddy simulation of marine stratocumulus. J. Atmos. Sci, 62, No. 11, 3909-3932. Nakajima, T., A. Higurashi, K. Kawamoto, and J. E. Penner, A possible correlation between satellite-derived cloud and aerosol microphysical parameters, Geophys. Res. Lett., 28, 1171--1174, 2001. Rosenfeld, D., and G. Feingold, 2003: Explanation of the discrepancies among satellite observations of the aerosol indirect effects. Geophys. Res. Lett., 30, No. 14, 1776, doi:10.1029/2003GL017684. Schwartz, S. E., Harshvardhan, and C. M. Benkovitz, Influence of anthropogenic aerosol on cloud optical properties and albedo shown by satellite measurements and chemical transport modeling. Proc. Natl. Acad. Sci., 99, 1784-1789, 2002. Xue, H., and G. Feingold, 2005: Large eddy simulations of trade-wind cumuli: Investigation of aerosol indirect effects. J. Atmos. Sci., in press. [Graham Feingold]	Noted
2-39	A	0:0		The whole chapter is well documented and illustrated. [Savitri GARIVAIT]	Thanks
2-40	A	0:0		I find that the chapter still needs some editing in particular with regards to coordination of different sections within. For example, with regards to aerosols , the reader is left with	Noted and Accepted. Observations will be carefully considered

No.	Batch	Page:line From	Page:line To	Comment	Notes
				the idea that aerosol modeling seem to have constrained the sign and improved the uncertainty of the magnitude of the RF. However, in many instances in the chapter, there is mentioning to the fact that comparison with observations offer a somewhat favorable result but very dependent on observation and instrument conditions. [Santiago Gassó]	
2-41	A	0:0		Because "Observations of the climate system are the foundation of our science" (From Chapter 1 page 1_2 line 26).. this Chapter should change place and follow the Chapters 3 and 4 on "Observations" and be renumbered accordingly [Vincent Gray]	Rejected.
2-42	A	0:0		A major failing of this entire Chapter is the inadequate treatment of "emissions" and the relationship between emissions and atmospheric concentrations.There is no section dealing with emissions and how they are measured. We do not have a Table of sources of methane emissions and we do not have a discussion of the relationship between emissions and concentrations. You have failed to warn governments that the relationship between carbon dioxide emissions and their concentrations is so uncertain that measures to reduce emissions may have little effect on concentrations [Vincent Gray]	Taken into account. The section on CO2 contains a treatment of emissions and linkages to atmospheric concentrations including graphs which will be improved. The section on methane does not describe emissionos and linkages but this will be added in the next draft
2-43	A	0:0		This chapter is also well constructed, and many aspects related to anthropogenic-induced changes in atmospheric constituent and possible effects on climate change have been covered, and equally distributed and discussed in the chapter. The main recent advances and some uncertainties of key components that determine climate changes have been well documented since the TAR. [Xueliang Guo]	thanks
2-44	A	0:0		General comments: (Mostly Ch 2 and needs to be reflected in Ch 1). TSU NOTE: Comment received too long - has been cut and pasted into a review supplementary document. [John Hallett]	Noted'
2-45	A	0:0		There are significant holes in Ch 2 from earlier work which need to be remedied. Input of IN into models is quite inadequate and most times misleading. The whole issue of secondary ice needs to be addressed globally - suggestions and references attached. [John Hallett]	Noted
2-46	A	0:0		I would like to compliment the authors for a first draft which already provides a comprehensive assessment of the recent litterature. [Didier Hauglustaine]	THanks
2-47	A	0:0		This chapter was supposed to be "Changes in atmospheric composition and RF" but is in fact "Past changes in atmospheric composition and RF". Was this chapter supposed to cover only changes since pre-industrial times or to provide also a estimates of future RF?	Rejected. Chapter 10 will cover future emissions. We are not repeating SRES or what was in TAR.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Since the future changes in atmospheric composition are not covered by Chapter 7 either, the whole AR4 misses these estimates and a assessment of the recent literature on the future forcings (in particular the ACCENT exercise in the case of tropospheric ozone). [Didier Hauglustaine]	
2-48	A	0:0		TSU NOTE: Please see supplementary review material [Katherine Hayhoe]	Noted
2-49	A	0:0		It seems confusing to me to keep comparing the values of radiative forcing calculated to those from TAR when I think that the caculations are being done for a slightly different period, up to the present day. It would be a good idea to show how much of the change in forcing from TAR due to each mechanism has come from a change in the concentration since the TAR, and how much is due to a fundamental change in our understanding, or revision of previous data. [Eleanor Highwood]	Partially accepted. Table 2.9.1 alrady does this. Will clarify text though
2-50	A	0:0		Throughout this chapter, there is frequent use of the word "necessarily" to indicate that something actually happens as a result of some prevalent conditions. By doing a word search on the document, one can find several locations where that word is used in this way. I suggest that the word "necessarily" be removed where its use does not add anything to the meaning of the sentence it is found in. Alternatively, where emphasis is needed to show that an event would most certainly occur, the word "necessarily" could be replaced by another more applicable word such as "invariably". In my experience, "necessarily" is generally used in the negative context, such as in "not necessarily". [Charles Ichoku]	Noted. It is necessary to consider each "necessarily" necessarily on merit.
2-51	A	0:0		According to the aim of the United Nations Framework Convention on Climate Change, regional climate changes are the very target of the convention; otherwise, the convention can be "Convention on Global Temperature Increase" or "Convention on Stabilization of Greenhouse Gases." In this regard, ideas such as "Regional Climate Change Potential" of Pielke et al. (2002) looks very important, and should be used in near future even if it is not possible right now. Suggestions along this line will show the direction of future climate researches, and worth to note somewhere in the report. [Kiminori Itoh]	Rejected.We may consider other metrics in Future assessmnet reports. Not the place to suggest future climate science research directions.
2-52	A	0:0		This chapter stresses the importance of radiative forcings, in particular TOA radiative forcings. However, according to the proposal by the report of National Research Council "Radiative forcing of climate change - expanding the concept and addressing uncertainties," (cited as Jacob et al. 2005 in References section) radiative forcings and non-radiative forcings are as important as TOA radiative forcings. This change in concept seems so large that it could be regarded as even a paradigm change. Suggestions on this point will give readers a good perspective of future climate researches, and hence, worth	Not clear what the question is. Do you mean "surface" forcing? If so, yes, this is an important diagnostic, but we partially disagree that this is as important as RF in terms of the global-mean syrface temeprature change. Or, do you mean "non-radiative" forcing?

No.	Batch	Page:line From	Page:line To	Comment	Notes
				to note. [Kiminori Itoh]	Even NAS study has declared that there do not exist metrics to quantify these.
2-53	A	0:0		According to the aim of the United Nations Framework Convention on Climate Change, regional climate changes are the very target of the convention; otherwise, the convention can be "Convention on Global Temperature Increase" or "Convention on Stabilization of Greenhouse Gases." In this regard, ideas such as "Regional Climate Change Potential" of Pielke et al. (2002) looks very important, and should be used in near future even if it is not possible right now. Suggestions along this line will show the direction of future climate researches, and worth to note somewhere in the report. [Kiminori Itoh]	Rejected.We may consider other metrics in Future assessmnet reports
2-54	A	0:0		The authors have worked hard to put together a well-balanced draft. I enjoyed reading it from the first line to the end [Fortunat Joos]	Thanks
2-55	A	0:0		I was surprised to see the over-emphasis on Concept of 'RF (Radiative forcing)' in Chapter 2. I strongly feel that the executive summary on individual important gas needed to be rewritten; in the first place, changes in actual concentration based on concrete observation activities be described clearly, then its impact on radiative forcing be mentioned, if needed. [kyung-ryul Kim]	Partially accepted. However, concentration changes will continue to be linked with RF
2-56	A	0:0		The importance recent developments on isotopic approach on closing global budget and identifying processes involved should be more emphasized. Further description on brief historical review and recent developments, in particular, on N2O, is strongly recommended. [kyung-ryul Kim]	TBD
2-57	A	0:0		Figure captions and table headlines should make an understanding of the respective figure or table possible without reading the text or a list of acronyms; acronyms should be explained in the figure captions or in footnotes of the tables. [Ralf Koppmann]	Accepted
2-58	A	0:0		The chapter is well written, and the different topics are well balanced; I do not see a possibility to shorten the text without losing information. [Ralf Koppmann]	Noted
2-59	A	0:0		Sometimes sentences are too long and too difficult to read, sentences extending over three lines or more should be split into more sentences. [Ralf Koppmann]	Accepted
2-60	A	0:0		I do not overlook all of the peer reviewed literature of all specific topics of this chapter, but as far as I evaluated the citations in a random survey, I think the most recent and appropriate literature has been cited.	Noted - thanks

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Ralf Koppmann]	
2-61	A	0:0		In Chapter 7 there appears to have been a deliberate policy in descriptions of the carbon cycle to use units of Pg-C in place of the hitherto more usual Gt-C. I think that this usage (Pg-C) has merit and is more consistent with units used for CH_4 (eg Tg-CH_4 rather than Mt-CH_4). It is unfortunate, therefore, that Gt-C is used in this Chapter 2. These usages should be harmonised in the two chapters (and perhaps other chapters?). Also at first usage of Pg-C in each chapter, the identification of Pg-C with Gt-C and with 10^15g-C should be made for the reader's edification. Comments made in this cell are also made for Chapter 7. [Keith Lassey]	Rejected. Gt-C used . Accepted that units will be spelt out on first use.
2-62	A	0:0		TSU NOTE: Please see supplementary review material [Keith Lassey]	Noted
2-63	A	0:0		This is a very thorough and good summary of the current state of aerosol retrievals, modeling and our understanding of aerosol effects. Congratulations to the authors! [Istvan Laszlo]	Thank you
2-64	A	0:0		General: Please indicate if the reported mixing ratios are by volume ppb(v) or by mass (ppb(m). [Caroline Leck]	Rejected. Molar fraction used – text clarified
2-65	A	0:0		Please replace "species" with constituents or compounds. Its use is in partucular abundent in "section 2.4 Aerosols". Species belong to the world of animals. [Caroline Leck]	Accepted
2-66	A	0:0		Additional general comment: I miss a discussion or even mention of the shipping issue. There are now a number of papers that deal with the emissions and impacts of shipping that I thought could be usefully referred to. But, I recognize that you have to draw the line somewhere.... [David Lee]	Rejected. Only aviation sector discussed. Ship-effects included in aerosaol effects
2-67	A	0:0		Whole chapter: I need a glossary: Confusion between RF, Direct Radiative Effect, and all radiative related terms. I assume all estimates should done at the tropopause level, but throughout the chapter, this is very inconsistent. [Robert Levy]	Terms will be clarified
2-68	A	0:0		Sorry some of these statements are out of order: I cannot insert rows: I mainly concentrated on the aerosol sections, because my own level of scientific understanding for aerosol is "higher" than for the other forcing agents. I, however, wanted to strongly suggest that the level of scientific understanding of aerosols is at best "low." Yes, we have lots more observations over a longer time scale, but many studies seem to be inconsistent and models and observations have not been throughtly combined. Satellite measurements have not been synthesized with in-column and surface measurements. Most of the	Accepted. Aerosol LOSU now "low"

No.	Batch	Page:line From	Page:line To	Comment	Notes
				estimates described in this document are model based, with insubstantial statements saying that models are consistent with observations. Uncertainties are too large to be suggesting that the problem is "solved." [Robert Levy]	
2-69	A	0:0		I thought of a good analogy with baking (as I am baking a birthday cake while typing up this review). There are many steps to baking a pie. Pre-heating the oven (setting up the problem) Getting the ingredients together (the observations). Finding the correct pan to bake the cake (creating the model). Baking the cake (a long time of observation/model synthesis). Eating the cake (producing lots of numbers). Digesting the cake (asking if the results of the study make sense and were they useful?). I believe that we are still in the collecting stage, and we are just beginning to bake the cake. Forunately, I think the oven has been properly pre-heated. [Robert Levy]	Noted. Those who take cakes that the Parsee man makes, makes dreadful mistakes. The heat is on, though!
2-70	A	0:0		GENERAL COMMENTS This Chapter is the culmination of a very impressive amount of scientific work and achievement: by the Chapter 2 authors, and by the expanding Radiative Forcing research community. Overall, the Draft Report reveals a vibrant, dedicated, and talented group working in atmospheric physics and chemistry. I have been pleased to see the enhanced focus on quantifying the forces and feedbacks now operating within our inexorably changing climate system. There are some places in the text where it seems like growth in the "Radiative Forcing" climate community appears to have produced some non-convergent focus on less crucial aspects of the anthropogenic forcing of the climate system. My sense is that this newly intensified focus on the "lesser forcings" in the climate system has created a tendency to include everything possible in this Chapter's focus, thus leaving the impression that important strides have been achieved since the FAR. This may, indeed be the case, but is appears that we now may be adding many less-central processes with more uncertain complexity that may contribute relatively little to the already well identifies larger contributions to RF. Candidates for possible oversell of significance might be: anthropogenic water vapor; indirect cloud-aerosol- radiative interactions, and thus influence of stratospheric gas-phase chemistry on ozone's influence on RF. (In fairness, it is important for me to note that what has been really interesting in my own research may be quantitatively negligible, relative to the growth in the suite of long-lived greenhouse gases. [Jerry Mahlman]	Noted – thanks. Will reduce water vapour discssion
2-71	A	0:0		May I complement the authors on their careful attention to uncertainties in this chapter. I found section 2.9.2 particularly helpful in understanding the considerations that had gone into determining confidence levels. I would just make one editorial suggestion, however - which would be that when the first use of the LOSU term appears in the Exec Summary	Accepted. Foot note will be added

No.	Batch	Page:line From	Page:line To	Comment	Notes
				you include a footnote saying something like: The factors determining the different levels of scientific understanding used here are summarized in section 2.9.2. [Martin Manning]	
2-72	A	0:0		There still seem to me to be some boundary problems in the coverage of the carbon cycle across chapters 2, 5 and 7 and the cross-referencing between chapters is not right in some places. Clearly some people have worked very hard on the carbon cycle material and a lot of progress has been made since the ZOD but I feel that the coverage could be made more complete and easier to follow if some structural adjustments were considered by the authors of all 3 chapters. At present chapter 7 seems to be left to do too much and this defeats the aim of emphasizing the broad importance of the carbon cycle by having it covered in all three chapters from different perspectives. The original plan, in the "Notes to LAs" distributed at LA1, was that Ch02 would cover the relationship between (observed) changes in atmospheric composition and emissions. At the moment both Ch02 and Ch07 seem to be doing this and we have two rather different presentations of fossil fuel emissions and CO2 increases in both text and figures, and both sets of figures could do with improvement. The best way of doing that would probably be to pool our resources and make cleaner decisions on what goes where. Similarly the "Notes to LAs" planned for air-sea fluxes and their changes over time inferred from observations to be covered in Ch05. The boundary issues in this case may be more arbitrary and perhaps require a pragmatic rather than theoretical approach. But I miss seeing a map of delta-pCO2 or inferred air-sea fluxes in Ch05 that would illuminate say the TRANSCOM3 results on the latitudinal distribution of ocean fluxes in Ch07. The information in Ch05 on change over time in the ocean uptake fraction is left dangling at present and begs the obvious question: Is this consistent (at least to an order of magnitude) with model results? Can that question be picked up in Ch07? For pragmatic reasons of length and structure I would argue that the material on carbonate chemistry and pH (acidification) would fit better in Ch05 - if necessary with changes to the contributing author list. That still leaves the bulk of material on the carbon cycle in Ch07 but it could then become more tightly focused around: the big picture of different reservoirs and time scales; the terrestrial carbon cycle which is so heavily influenced by climate variability and change; and all the inverse and coupled modelling work. [Martin Manning]	Accepted ..extensive discussions at LA3 have helped improve chapter cross referencing on carbon cycle issues. The second order draft will take these into account
2-73	A	0:0		This chapter nicely reflects the maturing of our understanding and representation of radiative processes for climate change purposes. I think it is much easier for non-specialists to understand the issues now. [Michael Manton]	Noted -thanks
2-74	A	0:0		Although the Chapter 2 Figures are generally much better than those of Chapter 1, they	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				should still be improved. [Lourdes Maurice]	
2-75	A	0:0		The large number of "submitted" references used in Chapter 2 is an area of concern. These are not yet peer reviewed. Prior to publication, recommend an assessment of the status of various references and urge primary reliance on those that have been accepted for publication. [Lourdes Maurice]	Accepted. We will
2-76	A	0:0		The whole chapter makes a great read already. Congrats for all the work. [Malte Meinshausen]	Noted -thanks
2-77	A	0:0		Changes in RF are sometimes discussed without clarifying whether the changes are simply updated estimates since TAR due to improved understanding, or real atmopheric changes due to increasing concentrations etc. Recommend being more careful on this point to avoid confusion. [Dylan Millet]	Accepted. wIll clarify
2-78	A	0:0		Several times Ammann et al. (2003) is written incorrectly as Amman et al. (2003) [Raimund Muscheler]	Accepted
2-79	A	0:0		Throughout the chapter, the authors refer to the level of scientific understanding. Of the three ways in which uncertainty is presented (likelihood scale, confidence level, level of scientific understanding), I find the latter to be the most confusing, and the place where additional detail could, in some places, be most useful. Where the level of understanding is, for example, established but incomplete, it would be very helpful to state *how* it is incomplete. By highlighting what questions do remain in the level of scientific understanding, it will better help the reader to understand the value of what is known. By contrasdt, with confidence levels or likelihood levels, it seems realtively clear that these are limited by available observation. Less additional information is need for these, except to explain, where appropriate, what limitations in data or theory might contribute to medium confidence estimates, i.e. what limits the ability to make estimates with higher confidence. [Anthony Patt]	Accepted. Will be improved for SOD
2-80	A	0:0		In the executive summary of this chapter it is sait towards the end that the total global mean surface forcing is most likely negative. It is very difficult for me to understand how this could be true, knowing that the temperatures at the surface over the globe increases. Should we not rather have a cooling at the surface if the total forcings would be negative at the surface? [Rolf Philipona]	Noted. This chapter is concerned with essentially the radiative forcing (see definition). This refers to the *instantaneous* change in the net iradiance at the surface, NOT the changes one would expect to see after the climate (surface-troposphere) system has started to respond e.g., with

No.	Batch	Page:line From	To	Comment	Notes
					surface temperatures changing.
2-81	A	0:0		Also, even though chapter 3 of this report is mentioning the increase of water vapor in the atmosphere there is nothing sait about the radiative forcing of this increasing water vapor. I know that this is not man made, it is a feedback and therefore natural, but in this report volcanos are also treated, which are natural. [Rolf Philipona]	Rejected. While stratospheric aerosols from volcanic eruptions are regarded as a "natural" forcing agent, water vapor is considered a "feedback" variable in climate change assessment. The possibility of water vapor being a "forcing" in the troposphere is discussed in 2.3.8 (to be moved to 2.5), while "forcing" by strtatospheric water vapor from methane oxidation is discussed in 2.3.8.
2-82	A	0:0		With my group I am measuring the radiation budget at 10 stations in the central Alps covering an area of about 200 by 200 km2. From 1995 to 2002 we observed very strong changes of radiation fluxes. The total incoming radiative flux, net shortwave plus downward longwave that gets absorbed at the surface, shows high correlation with the increasing temperature. But while shortwave radiation decreases longwave radiation strongly increases. However, we see even higher correlation between longwave downward radiation from the clear sky and increasing temperature and the humidity. This indicates that anthropogenic greenhouse gases and water vapor increases. Our investigations show that 70% of the increase is due to water vapor and the rest is due to anthropogenic greenhouse gases. [Rolf Philipona]	Noted. These are important observations of the climate system. However, they are not indicative of RF since atmospheric temperature, water vapor and cloud variations/ change affect the measurements. Note that changes in tmeprature and hydrologic variables are regarded as "feedbacks" in the climate system. See Response to Comment 2-81.
2-83	A	0:0		Now, measuring these forcings at the surface, which perfectly correlate with the increasing temperature and increasing humidity makes it very hard to believe that radiative forcing at the surface should be negative even on the average over the globe. Please have a look at the three following papers. I am prepared to write a synthesis of our findings, which could be added in Chaptre 2.9. [Rolf Philipona]	Noted. See Response to Comments 2-80, 2-81, 2-82.
2-84	A	0:0		Philipona, R., B. Dürr, C. Marty, A. Ohmura, and M. Wild, 2004: Radiative forcing - measured at Earth's suface - corroborate the increasing greenhouse effect. Geophys. Res. Lett., 31, L15712, doi:10.1029/2003GL018765 [Rolf Philipona]	See above.
2-85	A	0:0		Philipona, R., and B. Dürr, 2004: Greenhouse forcing outweighs decreasing solar radiation driving rapid temperatur rise over land. Geophys. Res. Lett., 31, L22208, doi:10.1029/2004GL020937. [Rolf Philipona]	See above

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-86	A	0:0		Philipona, R., B. Dürr, A. Ohmura, and C. Ruckstuhl, 2005: Anthropogenic greenhouse forcing and strong water vapor feedback increase temperature in Europe. Geophys. Res. Lett., 32, L19809, doi:10.1029/2005GL023624 [Rolf Philipona]	See above
2-87	A	0:0		Some minor changes [Rolf Philipona]	??
2-88	A	0:0		Whenever possible, identify TOA forcing from surface forcing. [A. R. Ravishankara]	Accepted
2-89	A	0:0		This is a very comprehensive chapter that covers a lot of territory. Congratulations to the authors for pulling all this together. All my comments are made in the vein that they will make the chapter better. [A. R. Ravishankara]	Thank you
2-90	A	0:0		More substantive references to all the work above can be provided on request. [Alan Rodger]	?
2-91	A	0:0		While I am a little disappointed that the "cloud-lifetime" effect has not been given stronger coverage in this chapter, I think I do understand the authors' rationale for leaving it out of the main results. However, there are some inconsistencies in the way this effect is referred to, and I mention these in my specific points. [Leon Rotstayn]	Noted. Will be clarifed. Chapter 7 will cover most of this
2-92	A	0:0		TSU NOTE: Please see supplementary review material [Stephen Schwartz]	Noted
2-93	A	0:0		These are comments on Chapter 2 only. Comments on other sections will be provided later. Quotations from original in boldface type [Stephen E Schwartz]	Noted
2-94	A	0:0		There is no comment for the lifetime even if Table 2.10.1 showing the value of lifetime. It is necessary to write the recent development of lifetime. The method compared between the compound (X) OH rate constant and 1,1,1-trichlorometane's rate constant have large uncertainty. Because behavior of compound (X) in atmosphere is not equal to the 1,1,1-trichlorometane. [Akira Sekiya]	Comment will be added
2-95	A	0:0		Comments is necessary for the lifetime of CO2.?Many people use different lifetime of CO2. It leads the different prediction conclusions. Please write the lifetime of CO2 is not clear because special behavior in atmosphere. It should use carbon cycle model equation	Accepted: material on lifetime of CO2 added to section 2.3.1

No.	Batch	Page:line From	Page:line To	Comment	Notes
				to know the remaining CO2 value in atmosphere.?The equation include the all the effect and CO2 behavior in atmosphere. [Akira Sekiya]	
2-96	A	0:0		GWP is the most important evaluation for the calculation of global warming amounts in Kyoto Protocol. Recently there are lots of developments for the replacement of GWP such as Indirect GWP, Net GWP, CWP, TWPG so on. If IPCC report is according to science base, should be writing and list up the repots, and presentations. Please comments that current GWP (100y) value is the political value, and wide range analyses are necessary to consider the future environment. [Akira Sekiya]	Noted
2-97	A	0:0		Very nice chapter, congratulations to the author team. [Steven Sherwood]	Thank-you
2-98	A	0:0		This chapter has made great strides since the zeroth draft, and as far as I can tell is "on track". My major comments are (a) that the wording in the Executive summary is way too loose and (b) that the accountability of some of the values in some of the summary figures is way too slack for such a high profile publication. [Keith Shine]	Accepted. Will reword ES and add text to section 2.9.2
2-99	A	0:0		This chapter is well written. [Ramachandran Srikanthan]	Thank-you
2-100	A	0:0		Chapter 2 is generally well and concisely written. Compared to earlier IPCC assessments the chapter extends the topics significantly and it introduces some new concepts. In some parts the document needs extension and some corrections are highly recommended (see below). [Johannes Staehelin]	Noted –thank-you
2-101	A	0:0		Read Chapter 2 in entirety. Well-constructed, good summaries, informative and a good review with reasonable summaries, interpretation, recommendations for how to interpret models and observations. Most of detailed comments from this Reviewer pertain to Secs 2.3, 2.8, 2.9. [Anne Thompson]	Thank you
2-102	A	0:0		Clear descriptions about what are shortages or are still unknown in climate change science would be helpful for planners of science program. [Yukitomo Tsutsumi]	rejected- this is not the job of this chapter
2-103	A	0:0		Significant progress on the radiative forcing estimates. Very interesting chapter [Philippe Tulkens]	Thank you
2-104	A	0:0		Throughout the chapter, uncertainties are quantified in terms of "(…) a factor of X uncertainty". This way of quantifying uncertainty should be defined in an explicit and	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				unequivocal way. [Philippe Tulkens]	
2-105	A	0:0		This FAR chapter covers all or part of the topics covered in four chapters of the TAR (Chapters 3, 4, 5 and 6). This represents about a two-fold compression of detail in research areas that are growing rapidly. I believe that this degree of compression is excessive, particularly in some areas (see comments below). Given the role of the IPCC in establishing and justifying research directions at the policy level, this compression, if sustained, will damage the field and threaten some of the observational underpinnings upon which the credibility of anthropogenic climate change is ultimately based. I therefore urge, in the strongest possible terms, that this chapter be expanded as discussed in the following comments. [Ray Weiss]	Partially accepted, will expand obs
2-106	A	0:0		The balance of this chapter is strongly skewed toward changes in radiative forcing (RF), and away from changes in atmospheric constituents. Even the brief overview of changes in atmospheric constiuents is expressed in terms of RF, and often skips over the discussions and figures of changes in actual atmospheric abundances that were presented in previous Assessments. This emphasis on RF, an interpreted quantity, not (in most cases) a directly measured one, is so strong that I find the chapter title misleading. As it is presently written, a more appropriate title for this chapter would be simply "Changes in Radiative Forcing". I do not wish to attribute any intentional bias, but I cannot help but note that this RF emphasis coincides with the research interests of the two Coordinating Lead Authors. This is not surprising, as it is expected that chapters will reflect the expertise of their authors. Increasing the number of Coordinating Lead Authors is one way to resolve the current strong imbalance. [Ray Weiss]	Partially accepted, will expand obs
2-107	A	0:0		This chapter covers major forcing mechanisms and agents, which should and can be evaluated with a reasonable confidence. Most of the sections in this chapter are concisely written, include appropriate figures and tables, and give assessment of results from important recent studies. The main contents are well structured, except for 3 or 4 subsections that are not so well placed in my opinion (see specific comments). Some issues to which I have given specific comments may be considered during revision. Many technical improvements are necessary. One of the technical improvements that I would like to mention at this place is to give the axises of graphs corresponding titles, not just units or nothing. [Xiaobin Xu]	Accepted. Figures will be improved
2-109	A	0:0		TSU NOTE: Please see supplementary review material [Xuepeng Zhao]	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-110	A	0:23		In relation to the preceding comments regarding chapter 2, start page 25, start line 6, my general comment is that section 2.4 has a major absence concerning the important advances in laboratory studies that have led to improved parametrizations within global models, i.e., policy-relevant findings. [Scot Martin]	Noted. Space limitations mean we cannot go into this much detail
2-111	A	1:1	1:60	By all rights this should be the strongest chapter of the IPCC Assessment Report. Unfortunately, it needs considerable improvement to reach that goal. The objective of the Report should be to present a strong and clear documentation of climate change, and to avoid becoming a punching bag for climate change critics and skeptics. It makes good sense to start with the strongest points that are available to make, i.e., the Chemically and Radiatively Important Gases, especially since the observational record is so well documented, accurate, and compelling. Accordingly, Sections 2.2 and 2.3 should be reversed. The inevitable uncertainties and ambiguities start to mount rapidly when the discussion turns to RF results. [Andrew Lacis]	Rejected. But sprit of comment is accpeted. Will beef up trace gas discussions
2-112	A	1:1	95:16	My general comment to Chapter 2: It has a very good reference list. However, Chapter lacks a critical analysis of most papers and is written as ''This author said this. Another author said that". I believe that the readers of this Report will be better served if the Report tells them who is right and who is wrong, or at least provides expert recommendations regarding what data/conclusions should be used and what are their limitations. Politeness and political correctness of this Report should not substitute a crtical analysis and expert recommendations. [Mikhail Danilin]	Partially accepted. We alrady assess much more than a typical review
2-113	A	1:3	1:14	The list of authors should be given with the full name of the people, without their affiliation.,as in Chapter 1 [Philippe Tulkens]	Editorial matter
2-114	A	1:8		Affiliations should be left off since not all are included and none are included in Chapters 1 or 3 [Michael Coffey]	Editorial matter
2-115	A	1:12		"Robert Sausen" add in brackets: "(DLR) [Hartmut Grassl]	Editorial matter
2-116	A	1:13		comment: to which organization belong "Gera Stenchikov" and "Christiane Textor" ? [Hartmut Grassl]	Editorial matter
2-117	A	1:13		Change "Gera" to "Georgiy L." [Alan Robock]	Accepted
2-118	A	1:24	1:31	These two sentences seem a little inconsistent. It GWP remains the recommended metric for comparing potential climate impacts of the emissions of different forcing	Accepted. Text will be clarified

No.	Batch	Page:line From	Page:line To	Comment	Notes
				agents, then why do you use RF estimates for each emissions source? (See my comment on chapt 1 for the confusion this has caused in the aviation community): "The Global Warming Potential (GWP) remains the recommended metric for comparing the potential climate impact of the emissions of different forcing agents. There are well-documented shortcomings, particularly in using GWP to assess the impact of short-lived species. RF is estimated for the main forcing agents and, for the first time, an estimate of RF associated with each principal emission source is also made." Maybe I am just unclear here about what distiguishes a forcing agent from a principal emissions source. I will read on... (however, if you put in some examples in parentheses behind each of the principal emisisons source (e.g. x, y, z) and different forcing agents (e.g. a, b, c) maybe it would be clearer. [Ian Waitz]	
2-119	A	2:0	24:0	Page 2-24: Aerosol semi-indirect effect should be mentioned after summarizing the direct and indirect effects of aerosols. [John Seinfeld]	Ok
2-120	A	2:0	28:0	Page 2-28: What are the assumptions for aerosol size distribution and aerosol water uptake in AEROCOM experiments? With aerosol emissions prescribed, these assumptions are the key factors that lead to the differences in AEROCOM forcing estimates. [John Seinfeld]	Noted, The actual modelled aerosol size distribution in the AeroCom experiments is not sufficiently documented. First it is (in contrast to TAR) now a dynamic property in almost all aerosol models and thus difficult to summarize. Second, only rough diagnostics are available which allow no straightforward interpretation. Size assumptions at source are described along with the AeroCom source documentation (see AeroCom webpage) . Important differences in aerosol water uptake are documented in Textor et al 2005. A refernce to the AeroCom source description has been added.
2-121	A	2:0	33:0	Section 2.4.5.5 on Page 2-33: Liao and Seinfeld [2005] estimated an anthropogenic nitrate forcing of -0.16 W m-2. This study should be mentioned here because it has the most comprehensive simulation of preindustrial and present-day nitrate aerosol. [John Seinfeld]	Accepted
2-122	A	2:1	4:1	Replace "greenhouse gas" by "LLGHG".	Not sure what this refers to

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[John Seinfeld]	
2-123	A	2:1	29:3	Lines 1-3 on Page 2-29: The sentence, "Although black carbon is less soluble than particulate organic matter, and wet removal is parameterized taking into account this difference, it appears that residence times of soot in some models are slightly longer then those of organic matter", is confusing. [John Seinfeld]	Accepted, sentence changed
2-124	A	2:1		perhaps consider "Effectiveness" for "Utility" , utility implys use [Michael Coffey]	Not sure what this refers to
2-125	A	2:6	100:6	Line 6 on Page 2-100: It is noted that "# BCPOM forcing: total aerosol forcing - sulfate forcing". Is this correct? [John Seinfeld]	Ok. Cautious remark needed
2-126	A	2:14		Insert a whole Section here entitled "the Relationship between emissions and atmospheric concentrations" [Vincent Gray]	Rejected. Emission discussion will be included in section 2.3
2-127	A	2:17	6:19	Lines 17-19 on page 2-6: The statement, "The Southern Hemisphere net RF is very likely larger than the Northern Hemisphere one, due to the globally very inhomogeneous aerosol RF that is more concentrated in the Northern Hemisphere", is confusing. Brief explanations are needed. [John Seinfeld]	Ok. Will revise.
2-128	A	2:17	38:20	Ch 2 p 2-38 lines 17 -20. Unclear as to evidence for MORE ice with smoke. Simple physics might suggest other wise; but combustion could also loft mineral particulates form soils; also organic ice nuclei, but thee are volatile and may not persist for significant times. [John Hallett]	Cannot link this comment with the text on the page and line stated. Still, will examine the text and revise if needed.
2-129	A	2:17	81:18	Lines 17-18 on Page 2-81: This reference should be updated as follows: Liao, H. and J. H. Seinfeld (2005), Global impacts of gas-phase chemistry-aerosol interactions on direct radiative forcing by anthropogenic aerosols and ozone, J. Geophys. Res., 110, D18208, doi:10.1029/2005JD005907. [John Seinfeld]	Accepted
2-130	A	2:19	39:27	Ch 2 p 2-39 lines 19 - 27. I think that there is evidence for more Large nuclei and/or UGN from combustion which might work the other way round (More PPT) under some conditions. [John Hallett]	Noted.
2-131	A	2:24	34:31	Lines 24-31 on Page 2-34: What are the typical values of dust single-scattering albedo from satellite and AEROCOM measurements? [John Seinfeld]	Accepted, however there are no AeroCom measurements as such. The mineral dust section however reports

No.	Batch	Page:line From	Page:line To	Comment	Notes
					now such values.
2-132	A	2:26	6:27	Lines 26-27 on Page 2-6: Describe clearly whether the total RF is positive at TOA or at tropopause. [John Seinfeld]	Accepted, clarification is now attempted. RF is defined w.r.t. tropopause.
2-133	A	2:29	3:30	Lines 29-30 on Page 2-3: It is described that the global mean RF for combined net total of all anthropogenic effects is estimated to be 1.5?1.0 Wm-2. Is this instantaneous or adjusted forcing? Is the RF estimated at tropopause or at TOA? Are aerosol indirect effects and semi-indirect effect accounted for when calculating this net total forcing? [John Seinfeld]	Noted. reworded
2-134	A	2:29	7:31	Give the definitions of "bottom-up" and "top-down" approaches. [John Seinfeld]	Accepted. described
2-135	A	2:32	35:34	Lines 32-34 on Page 2-35: "In their model, a doubling of SO2 emissions over present day conditions corresponds to 45% more sulphate, 14% more ammonium and 44% less nitrate." should be replaced by "In their model, a 50% increase in sulfur emissions over present day conditions corresponds to 45% more sulfate, 14% more ammonium, and 44% less nitrate (Liao et al., 2003)." [John Seinfeld]	Accepted
2-136	A	2:34	35:34	Line 34 on Page 2-35: "Chuang et al., 2002" should be "Chung and Seinfeld, 2002". [John Seinfeld]	Accepted
2-137	A	2:47	40:48	Lines 47-48 on Page 2-40 and lines 1-9 on Page 2-41 should be moved to Section 2.4.6.2.2. Note that surface layer indirect aerosol radiative forcing is mentioned in this paragraph but not shown in Table 2.4.6 and Figure 2.4.4. [John Seinfeld]	Noted; Cloud lifetime and semi-direct effects moved to CH. 7. Table 2.4.6 and Figure 2.4.4 include forcing due to albedo effect only
2-138	A	2:48	20:49	Lines 48-49 on Page 2-20: Since this paragraph is about the ozone forcing since preindustrial times, the study of Gauss et al. [2003] shouldn't be cited here. Gauss et al. [2003] shows ozone forcing resulted from the changes in ozone over 2000-2100. [John Seinfeld]	Noted – text reworded
2-139	A	2:53	57:	Lines 53-57: In AEROCOM calculations, what value is used for dust single-scattering albedo and what fraction of dust is assumed to be anthropogenic? [John Seinfeld]	Noted, a description of the zero anthropogenic AEROCOM dust fraction is included.
2-140	A	3:0		A mega-point for CHAPTER 2: There is now a pressure to involve the broadest possible contributions from the climate-science community to participate in the IPCC assessments. Therefore, there appears to be a huge pressure to include the recent contributions of legions of scientist-participants. Note that Chapter 2 contains 23 dense pages of literature references. It would be an informative exercise to evaluate what fraction of those copious references actually contribute to important new quantitative conclusions for the FAR I	Noted – thank you

No.	Batch	Page:line From	Page:line To	Comment	Notes
				have been impressed with the discipline and conciseness of the Executive Summary. It gets immediately to the essence of Chapter 2, and it develops quite early a strong sense of the compelling reasons for the world to take the mega-challenge of human-induced climate warming very seriously. I support the continuation of using the Global Warming Potential as the recommended metric for intercomparision of a surprisingly wide spectrum of forcing agents on the climate system. GWP is very useful, simply because it isolates very clearly what is quantitatively small, but accurate, and what is potentially quantitatively significant, but uncertain. [Jerry Mahlman]	
2-141	A	3:0		The regionality of aerosol forcing has to be stressed. This needs to be a separate bullet! The global average does not tell the story. The NH forcing must be much larger than in the SH. Also, the way the summary is written, it really does not do justice to the differences in the surface forcing that must be large AND important. If they want to add a clause like "the inhomogeneity in aerosol forcing does not lead to inhomogeneity in the TOA forcing, especially as a global average" may be useful. [A. R. Ravishankara]	Partially accepted. Seperate bullet on pattern wil be considered and could be added
2-142	A	3:0		The executive summary must address the issue of absorbing aerosols and changes in the temperature structure of the atmosphere. [A. R. Ravishankara]	Rejected. Beyond scope – that is a response.
2-143	A	3:0		The concept of efficacy is brought up in this chapter and is prominently displayed in the figure that will likely become the poster-child of forcing for the next decade. But it is not mentioned in the executive summary! This situation has to be rectified. [A. R. Ravishankara]	Rejected. Effiacy adequately mentioned at start of ES
2-144	A	3:1	3:5	The Executive Summary should begin with the strongest points by first describing the observational record of the radiatively important greenhouse gases, and by noting that the radiative effects of these gases can be accurately calculated. Radiative Forcing (RF), which is presented as a be-all end-all for describing human and natural influences on climate and should be moved further down the line. After all, RF is really a only specialized model diagnostic which requires caveats and a careful explanation. Note that RF itself does not serve as a model input parameter in any reasonable climate model. Furthermore, since RF approaches zero as the climate approaches equilibrium, RF becomes a moving target. The specific meaning of RF may be clear to the expert from the context of how the term is being used, but it should be recognized that "RF" is frequently used in a far more general sense. Therefore, the specific context needs to be carefully spelled out because the numerical value attached to any particular RF quantity may be compromised by so many contributing factors and conditions. [Andrew Lacis]	Rejected. However, PDF Forcing discussion will be moved below CO2 forcing discussion

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-145	A	3:1	6:35	Something should be said about the contribution of ocean, sea in the radiation forcing. Can we talk about RF without addressing the role of ocean and sea or in general hydrosphere? If this issue is addressed further in the text, it should be mentioned in the executive summary. Otherwise, I think it is relevant to include it. [G. H. Sabin GUENDEHOU]	Rejected. Do not really unerstand comment. Forcing estimates are considered over oceans, hydrosphere when global estimates are computed/determined.
2-146	A	3:1		Executive Summary: Early on there should be a bullet that distinguishes forward from inverse calculations and explains that this chapter is concerned with the former: "Radiative forcings can, in principle, be determined by forward calculations (based on the concentrations and properties of the forcing agents) or by inverse calculations (based on deducing what forcing is needed to explain an observed temperature change.) This chapter is concerned exclusively with forward calculations." [Theodore Anderson]	Partially accepted. Will make clear we use a bottom up approach
2-147	A	3:1		Radiative forcing is NOT a comparative estimator of the global mean surface temperature changes. It is an estimator of the change(s) in one or another component of radiative fluxes that result in change(s) in global mean surface temperature. Forcing should not be confused with response. Executive Summary. Attention should be paid to indenting of paragraphs. Not clear the rhyme or reason for the indents of the paragraphs. My guess is that the paras starting with bold lettering should not be indented, e.g., page 2-3, line 24. Please check. Similarly page 2-5 lines 10-17 would seem to need to be indented. yes? [Stephen E Schwartz]	Partially Rejected. Text has been reworded to clairfy our meaning . Indents will be used appropriately
2-148	A	3:1		Section: Executive summary. The executive summary gives clear messages and interestingly points the changes in radiative forcing (RF) since the TAR. The style of the text though is somewhat dry and its layout (sentences in bold and explanation in an indented paragraph) is different from the other chapters' executive summary [Philippe Tulkens]	WE like this ES layout
2-149	A	3:1		Section: Executive summary. The executive summary should reflect the current state of knowledge on RF for combined aerosols species (cfr. Section 2.4.5.7). The detailed section explains why it is still early to give estimates on RF for combined aerosols species but the summary does not mention this. [Philippe Tulkens]	Rejected, new studies based on observational data compute the total anthropogic direct aerosol RF
2-150	A	3:3	3:4	This description of radiative forcing as "a comparative estimator of the global mean surface temperature changes for the range of human and natural influences on climate" is reasonable, but it should be accompanied or preceded by a statement that the forcing concept allows a separation of cause (energy balance change) and effect (surface temperature change) in the analysis of the global warming problem. That is, we can hope	Rejected. These have been clarified in earlier IPCCs. Excessive detail not necessary here

No.	Batch	Page:line From	Page:line To	Comment	Notes
				to quantify forcings even if we have extremely imperfect knowledge of climate response (which necessarily involves such complications as thermal lag, feedbacks, unforced variability, and non-linearities.) [Theodore Anderson]	
2-151	A	3:3	3:4	This statement is not clear. I would suggest leading off with 'The concept of radiative forcing…' [Greg Bodeker]	Accepted
2-152	A	3:3	3:3	RF is not an 'estimator' (nor a predictor) of temperature. (To a linear approximation, temperature is the time history of RF, convolved with climate response function. Mathematically, T is a functional of RF, but defining functionals is probably not the way to go in AR4). 'estimator' has a specific meaning that does not apply here. Also 'comparative' seems inappropriate, since the use of RF does not neccessarily imply that anything is being 'compared' with anything else propose: 'RF works well as a measure of the drivers of climate change for the range of ....' [Ian Enting]	Partailly accepted. Forcing concept is reworded
2-153	A	3:3	3:3	"GLOBAL MEAN radiative forcing works well as a comparitive estimator of EQUILIBRIUM global mean surface ….". A similar comment applies elsewhere (e.g. on line 11). [Keith Shine]	Accepted
2-154	A	3:3	3:4	For absorbing aerosols, "radiative forcing" defined in this report should FAIL to predict even the sign of the consequent surface temperature change. [Hongbin Yu]	Rejected. See Section 2.8.5
2-155	A	3:3		The terms comparative estimator will not be widely understood, especially by those reading only the Executive Summary [Michael Coffey]	Text has been reworded for clarity
2-156	A	3:3		Most would agree that human activity has increased the action of the greenhose effect. Increasing greenhouse gases has definitely increased the radiative forcing (RF). However, the ultimate outcome of the increased RF as it affects surface temperature is not so widely agreed. To say here that increased RF is a proxy for increased temperature may be an overstatement. Indeed, why choose a proxy that is a lot more difficult to measure or calculate than the surface temperature itself. If this is still what the authors want to state it can be done more clearly than the present sentence. [Michael Coffey]	Rejected. However, sentence has been reworded for clarity
2-157	A	3:3		This initial statement reads oddly. Better to say: "Radiative forcing IS or CAN BE USED AS …" rather than "works well as". In addition, a "comparative estimator" is not a well-defined nor clearly-understood term. For clarity, it should be replaced with a phrase that clearly communicates the causal nature of changes in RF that drive surface temperature	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				change. On p. 6, for example, it is stated that RF represents "a measure of global mean temperature change". [Katharine Hayhoe]	
2-158	A	3:6	3:6	I am surprised that radiative forcings are given for year 2004. While some of the LLGHGs (e.g. CO2, CH4) might have model runs for that year, any of the short-lived gases (NMVOCs) or aerosols do not have emission inventories for 2004. How were these forcings obtained? This should be clearly explained. [Tami Bond]	Accepted. 2004 is approximate
2-159	A	3:6	3:11	The words "radiative forcings" precedes the acronym in parenthesis "(RF)". This is good. However, the acronym "TAR" is used in line 11 without definition or explanation. Although this is a very common acronym throughout the report, I think it should be defined the first time it is used in a chapter, because many readers will only read one chapter. Actually, "TAR" is defined later on in the chapter on page 2-7 but this is after having been used ten times in the executive summary! [Patrick Hamill]	Acronym will we clarified here
2-160	A	3:6		Could expand to: "Estimates of changes in radiative forcings for greenhouse gases, aerosols, and other radiatively-active species are given for …" [Katharine Hayhoe]	Rejected
2-161	A	3:6		I think you should make this something like "… radiative forcings are given in this chapter (or Executive Summary?) for present day (2004) relative to …" as there are other places in the report where 1750 is not used as the baseline and RF is given for a change over a shorter or longer period - e.g. the table being constructed for the TS. [Martin Manning]	Accepted
2-162	A	3:7	3:8	This is only true in general. It may be that a negative RF causes secondary effects e.g. circulation changes which manifest as climate warming in some isolated region. [Greg Bodeker]	Accepted. Will make clear we refer to global mean
2-163	A	3:7		What is meant by "climate"? If it is intended to refer to the atmosphere + surface + ocean I think a more accurate term would be the Earth system, not climate. Climate changes (in multiple ways), while it is the Earth that warms. [Katharine Hayhoe]	Accepted. Globally averqged surface warming is now used
2-164	A	3:10	3:10	Suggest replacing the words 'climate change mechanisms' with 'climate change drivers'. It is not the effect of the mechanisms that are being compared. [Greg Bodeker]	Accepted
2-165	A	3:11	3:11	You refer to the TAR definition without saying what it is. If its not too long it should be included here. [Greg Bodeker]	Rejected. It is too long

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-166	A	3:11		Define TAR, as this is its first use in Chapter 2. [Michael Coffey]	Accepted
2-167	A	3:12	3:12	"global mean" - this sort of implies that local values of forcing CAN be used as an indicator of detailed climate response [Keith Shine]	Accepted
2-168	A	3:15	3:22	Lines15-22. I am aware that the concept of "Efficacy" is an important diagnostic tool in the quest to isolate the array of climate forcings--large, small, and uncertain. I do, however, find that "efficacy", as an IPCC assessment tool, to be useful, but rather clumsy, and at first (and second) glance, quite obscure. It somehow appears to be a clumsy, and relatively non-intuitive diagnostic, particularly so when it is featured in the highlight opening part of the Executive Summary. [Jerry Mahlman]	Rejected. Tem is needed here. Text will be clarified
2-169	A	3:15		The concept of efficacy and what it means that it lies between 0.75-1.25 is not clear here. Although this is explained in detail in the text, the executive summary should be comprehensible at a basic level without need for further reference. [Katharine Hayhoe]	Accepted. Effiacacy text is imporved
2-170	A	3:17	3:17	Sometimes you refer to 'the TAR' and other times just TAR. I would suggest 'the TAR' throughout. [Greg Bodeker]	Accepted
2-171	A	3:18	3:22	Complicated for the executive summary: emove or simplify? [Cathy Clerbaux]	Accepted. Simplified
2-172	A	3:18	3:22	The first of these sentences talks about enabling future refinements. The second says "Therefore" (we think RF is useful now) which doesn't hang well together. [Joanna Haigh]	Accepted. Text clarified
2-173	A	3:21		Insert after "realistic" the word :"theoretical" [Vincent Gray]	Rejected
2-174	A	3:22	2:27	I support the continued use of Global Warming Potential as the recommended metric for intercomparing the magnitudes of various climate forcing agents, particularly so for the well-mixed gases. It is straightforward and intuitive. [Jerry Mahlman]	Noted
2-175	A	3:22	3:22	Change mechanisms to drivers. [Greg Bodeker]	Accepted
2-176	A	3:24	3:24	I would suggest inserting 'the integral of the RF over a prescribed period' after (GWP). [Greg Bodeker]	Rejected
2-177	A	3:24	3:25	On what basis is this reccomendation made? I cannot find any material in the relevent section of the report that leads to this conclusion.	Section 2.10 states this clearly

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Odd Godal]	
2-178	A	3:24	3:25	Recommending GWP as the metric for GHGs is prescriptive and not supported by science. Suggest "The Global Warming Potential (GWP) remains a common metric for comparing the potential climate effects of emissions of different forcing agents, however, there are well-documented shortcomings, particularly when using GWP to assess the effects of forcing agents that are not well mixed in the atmosphere or that affect climate via mechanisms other than radiative forcing." [Haroon Kheshgi]	Accepted. We now use the word "appropraite"
2-179	A	3:24	3:25	line 24-25 GWP is not defined for aerosols, water, etc. So, GWP is defined only for greenhouse gases, not all forcing agents. [A. R. Ravishankara]	Rejected. GWPs have not ben given by the IPCC for these components, but as long as the climate change is driven by RF GWP values can in principle be calculated (and so has indeed been done (e.g. Schwartz, 1993 for SO2 and Bond and Sun, 2005 for BC)
2-180	A	3:24	3:24	I think it is fairer to say that you only really consider GWP as an appropriate metric. Suggestions for a more thorough assessment of metrics were rejected early on in the planning process for AR4. [Keith Shine]	Partly Accepted. Since TAR there has been some studies estimating the potential cost of using a GWP based strategy compared to a "cost optimal" strategy (O'Neill, 2003; Aaheim etal., 2005). The conclusion is that GWPs do a reasonable job.
2-181	A	3:25	3:27	Propose: 'There are some well documented shortcomings that are inherent in using a single number to characterize complex behavour. These are most significant for short-lived species.' [Ian Enting]	Rejected. But spirit taken on board in rewording
2-182	A	3:29	3:31	"Humans have very likely contributed to a net warming effect on climate..." First, it should be stated that this claim is being made with respect to forward calculations. That is, "Forward calculations indicate that humans have..." This is a critical distinction and a much stronger claim, because forward calculations constitute evidence that is independent of the observed warming. However, the confidence "very likely" is an exaggeration, in my opinion. This confidence appears to stem from the probability density function (PDF) of total anthropogenic forcing displayed in Figure 2.9.1. This PDF, in turn, depends on the reduction in magnitude and lowering of uncertainty of aerosol forcings since the TAR - which I find dubious, as explained in other comments. Finally, the statement is less meaningful than it appears. The forcing could be positive, yet too small to explain the observed warming. A more meaningful threshold has been	Accepted Some of the points are beyond the scope of this chapter.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				established since the TAR based on numerous inverse calculations (cited in Chap 9) as analyzed by Anderson et al., (2003, Science, 300, 1103-1104.) The relevant threshold is +0.8 W/m2. The following statement would be more accurate and meaningful than the current one: "Forward calculations of radiative forcing indicate a likelihood (70% confidence) that humans have exerted a sufficiently large, positive forcing upon the climate system to constitute a plausible explanation of the observed, 20th century warming." It might also be pointed out that no other plausible explanation exists, although this would probably go beyond the scope of Chap. 2. [Theodore Anderson]	
2-183	A	3:29	3:31	Legalistic posturing language should be avoided in a scientific document. Eliminate commentary like "Humans have very likely contributed a net warming effect on climate." also "A negative net RF is very unlikely. [Andrew Lacis]	Rejected. We use wording carefully following ipcc guidelines
2-184	A	3:29	3:32	Lines 29-32. Since this terminology was my invention(Mahlman, J.D. Science, Nov. 1977, pages1416-1417), first used by IPCC in TAR, and continued in FAR, I get to comment on its use. In my nomenclature, Very Likely implies a greater than 90% chance of the phenomenon under question being true. Given the very careful awareness of this chapter to include negative RFs with wide uncertainties, but still revealing substantial net positive RFs, even when negative RFs with generous uncertainty bars are added to the mix(see Fig 2.9.1), a clear net positive RF emerges robustly. I thus argue that a net positive RF now deserves a confident " Virtually Certain" (>99%) evaluation for the FAR. I assert that this "radical conclusion" is a powerful tribute to the very dominant success of the careful diagnostic analyses prepared by this Radiative Forcing part of the climate science community, and is now widely seen to be compelling(i.e., What are the odds of a sustained net negative RF from now to, say, 20 years from now? Gobs of powerfully erupting volcanos? Would even the Contrarians take such a bet? I think not.). I thus offer my deepest congratulations to the global IPCC-participating scientists. [Jerry Mahlman]	Partailly accepted. Two pdf results will now be quoted
2-185	A	3:29	3:32	What is the statistical metric used to assess "likelihood". Is the uncertainty +/- 1.0 W m-2 at the 1 sigma or 3 sigma level? Make this statement more objective if possible. As written, a negative value lies outside the uncertainty of the evaluation and is not a possible outcome. It is essential to convey an accurate assessment of the uncertainties to decision makers. [Charles Miller]	Accepted
2-186	A	3:29	3:32	The bold language here is perhaps the most important in the chapter. There are three results presented with some calibration of uncertainty, but I think the honest thing to do is to show that the uncertainties are linked. Hence, because the RF has been estimated at 1.5	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				with a confidence interval of +-1.0, this implies that it is very likely (i.e. greater than a 90% probability) that the net effect of humans has been to increase the RF, and likewise that it is very unlikely (i.e. less than a 10% probability) that the human effect has been to decrease the RF. I would take the extra 40 characters to spell out the numeris confidence levels here, given how important this result is. [Anthony Patt]	
2-187	A	3:29	3:29	"Humans have very likely contributed a net warming effect on climate". This important conclusion is not very specific. If it is based on Fig 2.9.1 then the time frame andregion could be specified. Please add a specification to make to statement more powerful. [Guus Velders]	Accepted
2-188	A	3:29		I would move this point, concerning the human component of RF, to be after the comments about the total change [Michael Coffey]	Accepted
2-189	A	3:29		What does "very likely" mean? A certain %? [Robert Levy]	Yes. See IPCC guidelines
2-190	A	3:29		Humans have very likely contributed a net warming effect on climate.Better: Humans have very likely exerted a net warming influence on climate.It is essential to distinguish between cause and effect. This chapter deals with forcing, a cause. and throughout. [Stephen E Schwartz]	Accepted . However, don't the phrases say the same thing?
2-191	A	3:31	3:32	This language could confuse policy makers; you are not for example presenting the contributions from each principal emission source such as tropospheric ozone forcing from each of ships, vehicular traffic, power plants, etc. Perhaps better wording would be each principal forcing term? [Susan Solomon]	Accepted. Wording clarified
2-192	A	3:32	3:32	Its not clear to me what you are referring to when you say 'each principal emission source'. Do you mean e.g. CO2 emissions disaggregated by source? [Greg Bodeker]	Accepted. Yes
2-193	A	3:34	3:34	The newly adopted terminology of long-lived greenhouse gases (LLGHG) appears cumbersome and does not really convey any useful information since atmospheric life times of GHGs are seen to range from less than 10 year to more than 10,000 - a fact is not particularly significant for RF considerations. The more common description of well-mixed GHGs at least conveys some physically useful information to distinguish these gases from ozone. [Andrew Lacis]	Rejected
2-194	A	3:34	3:36	This paragraph describes the uncertainties in several different ways, making it less than entirely clear. I would not say that the RF from LLGHGs has the highest confidence level of any forcing agent, since I don't think policy-makers are interested in comparing	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				confidence levels, but rather effects among factors which have high confidence levels. Hence, I would make this paragraph consistent with the last in terms of using the "very likely" and "virtually certain" langauge to describe probability. I would thus say that the RF due to LLGHGs has been estimated at 2.59 +- 0.26, that this estimate is based on a high level of scientific understanding, and that it is thus virtually certain that LLGHG have led to an increase in the RF. [Anthony Patt]	
2-195	A	3:34	3:37	Rewrite. It is confusing. What does "Its" in line 36 refer to? [A. R. Ravishankara]	Accepted
2-196	A	3:34	3:37	It would be good to clarify at this point how much of this 7% is due to increasing forcing and how much due to change in understanding - from the SAR to the TAR the forcing from LLGHGs went down from the SAR to the TAR (Table 2.9.1) [Peter Stott]	Accepted
2-197	A	3:34		It may be misleading, and leave the report open to attack, not to mention here the radiative forcing effect of water vapor. Is water vapor a LLGHG? What are the relative RF of H2O, CO2, CH4, etc? [Michael Coffey]	Accepted. No, water vapor is not a LLGHG.
2-198	A	3:34		Define what is meant by longlived, is it 1 year, 10 years, 100years? [Michael Coffey]	Rejected – but will spell out gas names
2-199	A	3:34		Delete "LLGHGs" throughout and state which gases you mean in each case. It is a cause of confusion. How long is long? [Vincent Gray]	Rejected. However, will spell out on first use
2-200	A	3:34		Insert "increased concentrations of " before "long-lived greenhouse gases" [Vincent Gray]	Accepted
2-201	A	3:35	3:35	A time tag, presumably 1750, should be attached to the RF value. [Andrew Lacis]	Rejected. 1750 is implicit from the first bullet onwards.
2-202	A	3:35	3:36	What is "This is?" The forcing is an increase, not the "confidence" [Robert Levy]	Accepted
2-203	A	3:35		In general, match the number of significant figures to the expected error, in this case 2.6±0.3 [Roger Davies]	Rejected. Other forcings aree quoted in 0.01 Wm-2 units
2-204	A	3:36	3:37	This is clearly an overstatement. First of all, RF (having been defined at the tropopause) is not something that can be measured from a satellite. Second, measuring flux changes at TOA to a 1 W/m2 precision is beyond the current capability of current satellite instruments. At best, the measurements in question confirm a qualitative detection of an increase in atmospheric CO2 and CH4 since 1970. Furthermore, the authors don't	Accepted. Text reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
				actually claim to have measured quantitative changes in greenhouse gases. [Andrew Lacis]	
2-205	A	3:36	3:36	What is 'its RF effect"? [Guus Velders]	Accepted. reworded
2-206	A	3:36		What is "Its" I assume it means the LLGHGs RF effect [Robert Levy]	Accepted. reworded
2-207	A	3:36		What does RF effect mean? (This is confusing because there is a RF and a Radiative Effect defined [Robert Levy]	Accepted. reworded
2-208	A	3:39	3:40	The statement sounds unnecessarily alarmist, and is probably also incorrect without some kind of qualifying explanation or caveat. The CO2 rate of increase in the atmosphere is clearly variable (it was near zero in 1970), and it is not clear wheher one should be selecting a year, or month, or day to identify the maximum increase rate. Perhaps the rate of increase should be expressed over some relevant time frame. [Andrew Lacis]	Accepted ...reworded to point out annual growth rate highly variable but 5-yr ave increase 1999-2004 largest since instrumental records began in 1958.
2-209	A	3:39	3:41	It is claimed that carbon dioxide has the . . . "largest RF of any known agent"? Centrainly not on a per molecule basis. Is "knowledge" to be limited only to greenhouse gases? What about volcanic RF which can be quite large for large eruptions on short time scales. What about asteroid impacts? [Andrew Lacis]	This refers to 1750-2100 time frame and is accurate. Reworded for clarity
2-210	A	3:39	3:48	There is a very powerful message here that highlights the inexorable momentum of human-induced climate warming. Obviously, this "CO2 Report" will command a prominent place in the "Policymakers' Summary. And its punchline will assuredly be confirmed by Chapter 3: Observations: Surface and Atmospheric Climate Change. [Jerry Mahlman]	Noted
2-211	A	3:39	3:40	There have not been observations of CO2 over the last 2000 years. The data for pre-1958 CO2 estimates are taken from the geologic record. Reliable, accurate in situ observations are only available for the past ~50 years. [Charles Miller]	Noted see 2-208
2-212	A	3:39	4:6	I suggest to use simpler and more consistent wording to describe the increase in CO2, CH4 and N2O, such as "CO2 (CH4, N2O) rises at a faster (slower, constant) rate during the past decade compared to the 1980s. [Corinne Le Quere]	Noted see 2-208
2-213	A	3:39		Wow, this is really scary [Robert Levy]	Noted
2-214	A	3:40		Does "agent" imply anthropogenic? You might get people jumping on you wrt (natural)	Accepted. reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
				H_2O having a higher RF. [Joanna Haigh]	
2-2671	B	3:42	3:42	Can you check consistency with chapter 7 (chapter 7 gives 1.88 ppm /yr in its executive summary) [Olivier Boucher]	Noted...both SIO and CMDL data included here to give 1.851 ppm/yr
2-215	A	3:42		Concentration should be described as ppb(ppm) on a mass or volume basis, I.e. ppmv, ppbv [Michael Coffey]	Partially rejected ...ppmv and ppbv can only be used for an ideal gas. Molar mixing ratios are used here as described in 2.3
2-216	A	3:42		In scientific writing the term "concentration" means amount or mass per volume, typical units mol m-3 or kg m-3, respectively. The measure of abundance of CO_2 and other GHGs is mole fraction or mixing ratio (with respect to dry air) with typical unit ppm (µmol mol-1). The use of the term "concentration" in lieu of mole fraction or mixing ratio is of long standing and consequently replacing it throughout might lead to confusion and be net detrimental, but perhaps a footnote stating all of this might be appropriate.Note that mole fraction is occasionally used in the text (without explanation) e.g., page 2-15, line 55, and Figure 2.3.5.fossil fuel emissions rose from 6.5 to 7.2 Gt C yr –1 , [Stephen E Schwartz]	Noted ...please see 2-216
2-217	A	3:44	3:44	Change "fossil fuel emissions" to "emissions from fossil fuels, cement production and gas flaring" to be consistent with the text on Pg.9, lines 10-12 and elsewhere in the chapter. [Lenny Bernstein]	Accepted
2-218	A	3:44	3:44	It would be useful to clarify whether the unit Gt C is Gt C-CO_2 or Gt C-CO_2 equivalent (including other gases like CH_4 and N_2O converted in CO_2 equivalent using the GWP) [G. H. Sabin GUENDEHOU]	Noted ...defined in section 2.3
2-219	A	3:44	3:45	Insert "emission" in the sentence "… representing a period of much higher emission rates than …" [Keith Lassey]	Accepted
2-220	A	3:44		Attention should be paid (here and throughout) to whether this includes CO_2 from cement production? This 7.2 does not seem to conform with the 28 Pg CO_2 in figure 2.10.1 (=7.6 Pg C); maybe this seems like a minor point, but the difference is more than 50% of the growth. This should be checked. [Stephen E Schwartz]	Accepted ...however ref to figure 2.10.1 appears to be wrong
2-221	A	3:46	3:46	Current levels of CO_2 do not cause a radiative forcing - it is the change relative to pre-industrial times that causes the forcing [Keith Shine]	Accepted and recast
2-222	A	3:47	3:47	I think that here, and elsewhere in this chapter, where values in e.g. RF have changed	Accepted. Will clarify

No.	Batch	Page:line From	Page:line To	Comment	Notes
				since the TAR, it is very important to point out how much of this change results from a change in the atmospheric concentration of the forcing agent, and how much of the change results from new and improved calculations i.e. reduced uncertainty since the TAR. [Greg Bodeker]	
2-223	A	3:50	3:53	It would be better to start with observations of methane trends and growth rates, then proceed to describe the RF aspects. [Andrew Lacis]	Editorial decision
2-224	A	3:50	3:50	This statement refers only to methane in the atmosphere. [Charles Miller]	Noted
2-225	A	3:51		Delete "LLGHGs" throughout and state which gases you mean in each case. It is a cause of confusion. How long is long? [Vincent Gray]	Noted LLGHGs are defined above
2-226	A	3:52	3:55	This passage seems to imply that the decrease in CH4 growth will continue into the future, while some of the literature suggests that the decrease may only be temporary (e.g. Wang et al., 2004; Lassey et al., 2005). [James S. Wang]	Rejected. The para describes what has happened. There are no predictions of the behaviour of methane in the future
2-227	A	3:52		Insert after "declined" "to a currently negative value" [Vincent Gray]	Rejected ...the growth rate has declined ..it is currently positive ...but will add statement re negative growth rate years as detailed in comment 2-229
2-228	A	3:53	3:53	It would be good to know what the growth rate was in the previous 5 year period. You say its only 0.8 ppb/year now, but what was it pre1-999? [Greg Bodeker]	Noted..this is the ES only.... full description of recent growth rates in section 2.3
2-229	A	3:54	2:54	after "to 2004" add "and were negative for three of five most recent years." (see Figure 2.3.4) [Howard Feldman]	Accepted ...text added
2-230	A	3:54	3:54	"OH measurements": on the global scale OH is not determined directly by measurements, but indirectly using tracers such as methyl chloroform [Peter Bergamaschi]	Accepted ...text modified
2-231	A	3:54	3:55	"likely to be due to reductions in its emissions": From the existing studies it is not yet clear, whether we see a real reduction of CH4 emissions or just a stabilization [Peter Bergamaschi]	Partially accepted ...text modified
2-232	A	3:54	3:55	Policy makers and other non-experts will not understand the link between OH measurments and decreasing methane concentrations - sentence is poorly worded and unclear.	Noted ...text clarified

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Charles Miller]	
2-233	A	3:54	3:54	"OH measurements" is an inaccurate description. Recommend changing to "Indirect estimates of OH concentrations" or similar. [Dylan Millet]	Accepted ...text modified
2-234	A	3:54	3:54	"There are no OH measurements! They are estimated OH levels! (See general comments on OH levels) [A. R. Ravishankara]	Accepted ...text modified
2-235	A	3:54	3:55	The sentence "OH measurements indicate that this is likely to be due to its emissions" in somewhat unclear. If you replace the word "its" with "CH4" the sentence would be clearer. [Ilkka Savolainen]	Accepted
2-236	A	3:54	3:55	The sign and magnitude of the trend in OH over recent decades have been debated, and there is currently no consensus. Bottom-up and top-down approaches have produced conflicting results (see e.g. Ch.2, p.13, lines 22-28). Since it's far from clear which view is more defensible, I'd suggest giving both views equal weight. This also implies that the causes of the recent slowdown in CH4 growth are not known with certainty. [James S. Wang]	Noted ....but the two recent OH papers cited have changed the situation.
2-237	A	3:55	3:55	its emissions -> the emissions of CH4 [Joyce Penner]	Accepted ...text modified
2-238	A	3:55	3:55	"its" - does this refer to OH?! [Keith Shine]	Accepted ...text modified
2-239	A	3:55		Maybe I am missing something in section 2.3.2 but I do not see the justification for the statement that declining CH4 growth rates are likely to be due to reductions in emissions. The key word is reduction here - rather than stabilization of emissions (perhaps temporarily of course) which was the sense given in the TAR and is what I read into the literature even now. If the authors feel they can make a stronger statement than the TAR in this regard and point to a reduction in CH4 emissions then they really need to show an analysis that supports that as part of the assessment or cite a paper in the literature that makes such a statement unambiguously. As far as I can see this is not done at present and frankly I don't think it can be given the data available. The bottom line for the last 20 years is surely that OH has displayed decadal and shorter term variations but no significant trend, that the declining growth rate is consistent with roughly constant total emissions over that period, and that this has made interannual variations in CH4 growth rate more obvious - these being most likely due to interannual variations in sources rather than sinks. [Martin Manning]	Noted ...text modified
2-240	A	3:57		Delete "LLGHGs" throughout and state which gases you mean in each case. It is a cause	Reejcted. Will be explained on 1st use

No.	Batch	Page:line From	Page:line To	Comment	Notes
				of confusion. How long is long? [Vincent Gray]	
2-241	A	4:1	4:1	I think you mean nitrous oxide is probably the fourth biggest contributor to the WMGG forcing. It is not the fourth most important greenhouse gas [Keith Shine]	Accepted
2-242	A	4:1		If nitrous oxide is the fourth most important greenhouse gas, what is the third most important? This is resolved at line 13 with the statement that the Montreal Protocol gases as a group remain as the third most important LLGHG. This is awkward at best. A category is not a gas. Classifying the Montreal Protocol gases as an entity may be convenient but it does not make them "a gas". Evidently the ranking is by importance in radiative forcing of long-lived greenhouse gases. All this should be clarified. Loose language like this makes the report subject to unnecessary and distractive ridicule by communities who would point, correctly, to water vapor as the most important greenhouse gas in Earth's atmosphere. [Stephen E Schwartz]	CFC12 -reworded for clairty
2-243	A	4:2	4:2	This first paragraph in this section says that CO2 is number 1, the second says that CH4 is number 2, but now the third paragraph says N2O is number 4. It may be worth adding '(the 3rd most important greenhouse gas is CFC-12, discussed below)'. [Greg Bodeker]	Reworded for clairty
2-244	A	4:4	4:6	I am not at all convinced that this punchline about N2O is even qualitatively correct. I may have misinterpreted this argument, but the N2O simulations that I have been associated with do not yield this asserted stratosphere-troposphere exchange conclusion. [Jerry Mahlman]	Accepted ..text changed
2-245	A	4:4	4:4	Recommend changing "tropics emissions" to "tropical emissions" [Dylan Millet]	Noted
2-246	A	4:8	4:8	CF4 it not entirely an industrial Kyoto gas. Rephrase more precisely. [Guus Velders]	Noted ..text added to reflect this
2-247	A	4:10	4:10	Why no CF4? [Keith Shine]	Noted
2-248	A	4:14	4:14	Again … CFC12 is absolutely NOT the third most important LLGHG. [Keith Shine]	Accepted ...rewritten
2-249	A	4:15	4:16	The largest part of CFC-11 banks in foams is present in the foam in buildings/applications, not in landfills. [Guus Velders]	Accepted ..reference added
2-250	A	4:18	4:24	OH by itself makes no appreciable RH contribution. It belongs in a separate sections of chemically important agents along with CO, NOx, OCl, etc. Perhaps the OH discussion	Accepted ..ref to TAR removed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				might be more at home in Chapter 7. [Andrew Lacis]	
2-251	A	4:18	4:23	"OH has shown no net change…". The level of certainty or scientific understanding needs to be stated here. Recommend changing the first sentence to "OH has shown no detectable net change…", and also indicating the level of scientific understanding as is done for other items. [Dylan Millet]	Noted ...but not in the brief of this chapter ..refer to chapter 3 and 7
2-252	A	4:18	4:18	I don't think this statement should be assigned high confidence. The sign and magnitude of the trend in OH over recent decades have been debated, and there is currently no consensus. Bottom-up and top-down approaches have produced conflicting results (see e.g. Ch.2, p.13, lines 22-28). Since it's far from clear which view is more defensible, I'd suggest giving both views equal weight. [James S. Wang]	Accepted ..reference to TAR removed
2-253	A	4:18		"OH has shown no *statistically discernable* net change…" (it might have changed, but our methods cannot yet discern this) [Mark Lawrence]	Accepted
2-254	A	4:19	2:19	Is it correct that OH is the major producer for sulfate? I believe most (or all) global models show that aqueous-phase oxidation of SO2 is a larger source than gas-phase oxidation. [Leon Rotstayn]	Accepted but waiting for 2005 Scripps and CMDL data
2-255	A	4:21	4:22	"notably an *inferred* minimum…" - important to make clear that this is not directly observed [Mark Lawrence]	Accepted ...comparison added
2-256	A	4:22	4:23	Is this statement true even though there were no net changes in OH between 1979 and 2004? [Greg Bodeker]	Accepted ..change made
2-257	A	4:22	4:22	The global wildfires didn't seem to reach Reading! [Keith Shine]	Accepted ..text reduced
2-258	A	4:22	4:22	Estimates of OH in the 1990s are particularly sensitive to the emissions assumed for methyl chloroform. Prinn et al. (2005) use some debatable methods; thus, the view of, e.g., Krol and Lelieveld (2003) should be given more weight. Specifically, Prinn et al. do not fully consider the effect of recent observation-based estimates of MCF emissions on their inferred OH. If the new emissions are used, the OH trend in the 1990s would be closer to 0, and the dip around 1998 would be much shallower. Accounting for Wennberg's revised estimate of the ocean sink/source for MCF would result in even less of a negative trend in OH in the 1990s. [James S. Wang]	Accepted...text changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-259	A	4:25	4:31	The findings that stratospheric ozone is near its minimum and the magnitude of its RF is expected to decrease in the future is correct, based on studies conducted by the Montreal Protocol's TEAP, but is not supported by the underlying chapter. Section 2.3.7 discusses only past trends in stratospheric ozone, and while section 2.3.4 discusses the slow decline in atmospheric concentrations of ODS, there is no discussion of the rate of recovery of stratospheric ozone. Information about the rate of recovery of stratospheric ozone and its impact on RF needs to be included in the chapter, preferably in Section 2.3.7. [Lenny Bernstein]	Accepted..decade 1994 to 2004
2-260	A	4:25	4:32	It could be mentioned here that the first signs of recovery are partially attributed to changes in the dynamics. (eg. Reinsel, G. C., A. J. Miller, E. C. Weatherhead, L. E. Flynn, R. M. Nagatani, G. C. Tiao, and D. J. Wuebbles (2005), Trend analysis of total ozone data for turnaround and dynamical contributions, J. Geophys. Res., 110, D16306, doi:10.1029/2004JD004662 and Krizan P., J. Lastovicka (2005), Trends in positive and negative ozone laminae in the Northern Hemisphere, J. Geophys. Res., 110, D10107, doi:10.1029/2004JD005477.) [Hugo De Backer]	Rejected – too complex
2-261	A	4:25	4:31	While these findings are correct, they are not supported by the underlying chapter. Section 2.3.7 should be expanded to include results from the Montreal Protocol TEAP providing projections of future stratospheric ozone depletion. The IPCC Special Report on Safeguarding the Ozone Layer and the Global Climate System is another potential source for this information. [Jeffrey Kueter]	Accepted. Chapter strengthened
2-262	A	4:25	4:39	Ozone is in a different category from the other greenhouse gases in that it is variable in x,y,z,t. There are no ice core measurements from which to infer reference value for 1750 ozone distributions, unlike the case for he other well-mixed GHGs. Most of the ozone change has taken place in the past several decades, and it has not been globally uniform. Since ozone RF is highly sensitive to changes in the vertical profile of ozone, this should be reflected in stating the RF estimates, including the time period for which the RFs are applicable. Changes in tropospheric ozone have a strong latitude and longitude dependence. This point also should be noted. [Andrew Lacis]	Accepted. But text put in "spatial bullet"
2-263	A	4:25	4:26	Why is the RF of stratospheric ozone expected to decrease in the future if the ozone hole shows signs of recovery? (Section 2.3.7.1 gives details on this topic but I did not find an explanation on the decrease) [Philippe Tulkens]	Accepted – section altered
2-264	A	4:25	4:25	Because "expected" connotes something in the future, please change "expected" to "believed"	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Ray Weiss]	
2-265	A	4:28	4:28	You say that 'Ozone depleting substances are at their peak in the atmosphere' but this is not true. EESC peaked in around 1997 and even if you allow for age of air being longer than 3 years in parts of the stratosphere, its unlikely it would have peaked long after 2000. [Greg Bodeker]	Accepted
2-266	A	4:28	4:28	"CONCENTRATIONS of ODS are at their peak"! [Keith Shine]	Accepted
2-267	A	4:28	4:29	Total ODS concentrations are past their peak in the troposphere (peak around middle of the 1990s) and also in the stratosphere (see EESC calculations or HCl/ClONO2 observations). Only CFC-12 is at its peak in the troposphere. [Guus Velders]	Accepted
2-268	A	4:29	4:29	Change from "slowly reduce" to "slowly diminish" [Brian Magi]	Wording changed
2-269	A	4:30	4:30	Need to be careful how you use the word 'depletion'. Dynamical changes led to midlatitude ozone reductions not to midlatitude ozone depeletion. [Greg Bodeker]	acceepted
2-270	A	4:30	4:30	Presumably the reduction in the ozone radiative forcing since the TAR from -0.15 (note that this was also the value cited in the SROC report) to -0.1 is a result of global ozone not being as low now as it was 5 years ago, rather than a re-evaluation of the RF calculation per se? [Greg Bodeker]	Rejected
2-271	A	4:30	4:31	The "m-2" unit should be all on the same line. Use the Symbol character "minus" rather than the "dash" to prvenet separation by the word processor. [Charles Miller]	Accepted with thanks
2-272	A	4:30	4:30	Replace "depletion" by "reduction" (Do you want to suggest that dynamical changes led to a "depletion?" [A. R. Ravishankara]	accepted
2-273	A	4:31	4:31	The frequent comments of "slightly greater than TAR" or "slightly less than TAR" tend to become distracting. This report should be written as a stand-alone document. The approach to be taken should be more along the lines that after all these years, the results reported in TAR are no longer operative, hence the need for a new report. Whether some quantity is slightly greater, or slightly less than TAR is not exactly a validation of its accuracy or reliability. It is, of course, very much of interest to compare current results with previous TAR, SAR, and FAR results, but this would be more effectively handled in a combined fashion (as it is being done in Chapter 10) with separate sections for "results from TAR that are still valid" and "significant progress made since TAR". Perhaps there	Rejected. Chapter 2 has a different conceptual scope than other chapters.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				could even be a section on "specific progress that needs to be made beyond what is understood at the time of the current report". [Andrew Lacis]	
2-274	A	4:31	4:31	Comments like "medium level of scientific understanding" or "low level of scientific understanding" are a fuzzy form of non-information and should be avoided. It is better to state clearly what is known, and what is not known. There may well be things that are understood, but are not that well known because the relevant measurements are not available, and likewise, somethings may be accurately known, but are not really understood that well. [Andrew Lacis]	Rejected but clarified
2-275	A	4:35	37:	The effect of stratospheric ozone loss on tropospheric ozone at high latitudes is only of minor consequence for RF. This should not be highlighted in the exec.summ. Instead, the observed upward ozone trend at low latitudes is of much greated influence (see also comment below). [Jos Lelieveld]	Accepted
2-276	A	4:38	4:38	Due to re-evaluation of what, the changes in tropospheric ozone or the changes in the models used to calculate the RF from the changes in tropospheric ozone? The difference between these two possibilities is very important. [Greg Bodeker]	Text clarified
2-277	A	4:40	4:43	What about tropospheric water vapour increases or are these not considered to be anthropogenic? [Greg Bodeker]	Accepted. Wording altered
2-278	A	4:40	4:43	"Anthropogenic water vapour changes are likely to have contributed a positive RF." It would be good to mention how much. (Values are given for The other RF's. If the value is unknown or highly uncertain, this should be stated.) [Patrick Hamill]	Accepted
2-279	A	4:40	4:40	This is another example of a legalistic posturing statement that simply should be avoided. Methane does indeed get oxidized into water vapor in the stratosphere, and every H2O molecule in the stratosphere contributes to the atmospheric greenhouse effect. Perhaps the CH4 oxidation related stratospheric water vapor portion might be labeled as an "indirect" radiative frcing. It is legitimate to raise questions regarding where within the stratosphere this water vapor is generated and what its residence time might be - since these are factors which affect the RF that is attributed to the anthropogenic part of stratospheric water vapor. The balance of the stratospheric water vapor is presumably not due to Louis Frank water comets, but rather must be due to atmospheric feedback effects involving stratospheric-tropospheric exchange which may or may not be adequately modeled in current climate GCMs. By stating that the associated RF is 5-10 times higher	Accepted -reworded

No.	Batch	Page:line From	To	Comment	Notes
				than TAR raises eyebrows but does not necessarily suggest which result is more likely to be accurate. [Andrew Lacis]	
2-280	A	4:40	4:40	This is a MAJOR comment. You will cause immense confusion with the reference to water vapour like this - without making it clear that there is an anthropogenically driven component of water vapour change via temperature changes. I have seen this confusion (and indeed wanton misrepresentation) in discussions of previous IPCC reports. [Keith Shine]	Accepted. Text clarified
2-281	A	4:40	4:43	The increase of a factor of 5-10 compared to TAR is very large, and readers will want to understand the specific reason for the change. Can you please clarify how you are distinguishing between observed trends in stratospheric water vapor (which are large, and could give such a large forcing) and that which can be attributed to methane oxidation (which is much smaller)? Previous assessments have noted the large apparent forcing from observed stratospheric water trends, but have also noted that the portion of this that can be attributed to methane may be limited. Any model estimate of this quantity is uncertain, due to its strong dependence on vertical transport rates of methane in the lowermost stratosphere, and this may be helpful to state. Please clarify how you have handled the issue of what may be due to methane versus other possible sources (such as changes in the tropopause temperature) in this assessment, and explain why these numbers are so much larger than in the TAR. [Susan Solomon]	Accepted. Statement will be reworded. Section will also carefully consider how to word this
2-282	A	4:43		Add at end "Since methane concentration is now falling, this effect is less important" [Vincent Gray]	reject
2-283	A	4:45	4:46	The use of the word "medium" is a confusing. I presume it refers to the scale of level of understanding defined later in the chapter. However, without prior knowledge of that scale anyone who reads this section can misinterpret these two lines because what it is stated does not agree with the common understanding of the word "medium". Specifically, it is stated that the direct aerosol RF has a 100 uncertainty and even the sign of the trend can be of question. This fact does not seem to agree with a "medium" level of scientific understanding for anyone who is not familiar with the scale. [Santiago Gassó]	Partially accepted. Footnote will explain losu
2-284	A	4:45	4:46	It seems to me the sue of the word "medium" does not correspond to some of the evidence shown in the rest of the chapter. Fro example, in p29,line26-27, it is stated that "verification of model simlations against reliable observations has yet to be comprehensively perfomed". This sentence challenges the qualifier "medium" since there is not hard (i.e long time series and comprehensive) observational evidence to support it. [Santiago Gassó]	Accepted, LOSU changed to low

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-285	A	4:45	4:46	"...with a medium level of scientific understanding". The magnitude of the uncertainty (0.2) is exactly the same as the estimated radiative forcing value (0.2). This suggests, at the very best, a LOW level of scientific understanding. It is not justifiable to say that we have MEDIUM CERTAINTY that we understand something when the UNCERTAINTY is the same or larger than the measured value. [Charles Ichoku]	Accepted, LOSU changed to low and uncertainty range increased
2-286	A	4:45	4:46	Stating that "Direct Aerosol RFs are considerably better understood than in TAR . . . With a medium level of scientific understanding" is not exactly a ringing endorsement for the material that follows. The approach to be taken should be similar to that for the GHGs. First, it should e noted that the quantities that need to be observed are the aerosol optical depth, composition, size, single scattering albedo, vertical distribution, in addition to the geographic and time variability, and then start describing the obsevations that are available. It also should be noted, that aerosols frequently occcur as internal mixtures and aggregates, and that full spectral refractive indices are not available for many aerosol species, in particular organic carbon aerosols. All of these factors contribute to the considerable uncertainty of aerosols RF. Furthermore, there is no direct information as to what the reference aerosol backgorund level might have been in 1750, or for that matter, what the tropospheric aerosol trend has been over the past several decades. By comparison, the stratospheric aerosol trend is far better documented. [Andrew Lacis]	Accepted, LOSU changed to low
2-287	A	4:45	4:57	This is a nice analysis, but it is hard for me to argue for direct aerosol RFs to be as possibly uncertain all the way to zero. Is there a physical argument for a net of zero. If so what is it? [Jerry Mahlman]	Accepted, best estimate changed (-0.5 Wm-2) and uncertainty range increased with range from -0.1 to -.9 Wm-2.
2-288	A	4:45	4:46	This is TOA – Global forcing, I assume. This statement is very deceptive. Yes, the overall value could be -0.2 W m-2. BUT, the uncertainties are definitely not as small as suggested (more later). Also, this gives the false impression that the RF of different effects are additive everywhere. [A. R. Ravishankara]	Accepted, LOSU changed to low
2-289	A	4:45	4:46	The statement "direct aerosol RF are considerably better understood than in tar" disagrees with what is said later on in the chapter (page 26 line 24 and 29, page 29 line 13-14, page 36 line 10-11). There are still too many experimental discrepancy to say that this effect is well understood (there is an uncertainty of 100% in the estimate). Uncertainties and current problems with this estimate (which are expressed later) should in some way be pointed out here. [Felicita Russo]	Accepted, LOSU changed to low
2-290	A	4:45	4:46	The RF given is -0.2 +- 0.2 Wm2. That indicates the effect is not measurable. It also	Accepted, LOSU changed to low and

No.	Batch	Page:line From	Page:line To	Comment	Notes
				contradicts the statement "medium level of scientific understanding". It cannot be that well understood if the uncertainty is equal to the mean. Such statements in the summary and text are dangerous because it gives the impression that direct clear sky forcing is well understood and could lead to a reduction in funding to such work. The direct Aerosol RF should be changed to a low level of understanding, this still shows an approvement over the TAR where it was labeled very low. NOTE: further support for my view is given in the text itself (page 36 line 52-55) [Ellsworth Welton]	best estimate and uncertainty range changed
2-291	A	4:45	4:46	After reading through section 2.4.2-2.4.5, I can't agree that our understanding of aerosol direct forcing has reached a "medium level". Satellite-model differences are large and "the reason for the discrepancy is not clear" (p.36, line 10-11). The number cited here is based on model simulations forced by the same emission. But we still don't have much confidence on emission inventories and uncertainties associated with them should introduce additional uncertainty to the RF (p.29, line 13-14). Unlike LLGHG, aerosol RF has considerably large spatial and temporal variations, which is still uncertain. [Hongbin Yu]	Accepted, LOSU changed to low. However, not only models with the same emissions! Two groups of models used.
2-292	A	4:45	5:5	"Direct Aerosol RFs are considerably better understood than in TAR. A combined total direct aerosol RF is given as -0.2+/-0.2 W/m2, with a medium level of scientific uncertainty." I recognize that the text (page 2-36, lines 52-55) put a big caveat on the specific numbers given here. In my opinion, the statement is wrong even at a qualitative level. Although this bullet (and the subsidiary ones that follow) may reflect an accurate summary of recent model-based estimates, it does NOT, in my opinion, reflect the state of knowledge with respect to direct aerosol forcing. The underlying uncertainty is much larger than indicated. For example, the AeroCom project has revealed that the ranges among current chemical transport models for global-mean aerosol component mass and optical depth span factors of 2 to11 (Kinne et al., 2005, Table 4). It would be very unwise, in my opinion, for IPCC to commit to such narrow uncertainties when, in fact, we are just on the verge of acquiring robust, empirical constraints from new-generation satellites. A preliminary study based on MODIS satellite data, for example, concludes that the upper limit of anthropogenic direct aerosol radiative forcing is -0.8 W/m2 and that previous model-based estimates may have substantially underestimated direct forcing, at least in the clear-sky situation (Bellouin et al., "Global estimate of aerosol direct radiative forcing from satellite measurements", Nature, in press as of Nov 1, 2005). Another example involves claims made about biomass burning aerosols. Here, both the magnitude and even the sign of the forcing has been changed since the TAR, with a great reduction in the absolute magnitude of uncertainty. As stated here, these dramatic changes (since the TAR) have occurred "owing to better modelling of the effects of biomass burning aerosol	Accepted, LOSU changed to low and uncertainty range increased.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				overlying cloud." This is a remarkably adventurous statement, given that there are almost no empirical data to constrain these models - an exception being a single case study from SAFARI discussed by Haywood et al., 2003. In the next few years, data from the CALIPSO lidar satellite mission (if successful) should permit the first global-scale assessment of the prevalence of aerosol layers overlying boundary layer clouds. For now, this prevalence is essentially unknown. Again, by committing to such a low uncertainty, IPCC seems to me to be setting itself up for being proven wrong in the near future. [Theodore Anderson]	
2-293	A	4:45	5:5	I am concerned that stating the global-mean direct aerosol RFs in this way may give the wrong impression that the aerosol effects are relatively unimportant in climatic terms. Several studies have shown that much of the importance of the aerosol effects is related to their spatial variability (e.g., Rotstayn et al, GRL, 2000, Williams et al, Clim. Dyn. 2001, Rotstayn & Lohmann, J. Clim., 2002, Menon et al, Science, 2002, Chung & Ramanathan, J. Clim., 2003, Ramanathan et al, PNAS, 2005, Chung & Ramanathan, JGR, 2005). This point is made in Chapter 2 for some of the other forcings, but is hardly mentioned with regard to aerosols. As well as being mentioned in the Executive Summary, I think this point should be given a paragraph somewhere in the main text. At the very least, a cross reference to Chapter 7 could be given, since the issue is discussed there. [Leon Rotstayn]	Partailly accepted .Text reworded
2-2672	B	4:46	4:46	This estimate does not include dust, is that right? [Olivier Boucher]	It does
2-294	A	4:47	4:47	"changes" - there are many examples of this loose wording. I believe that you mean that there have been changes in understanding that have led to new estimates of the forcing, rather than the forcing itself that has changed - you have written the latter. [Keith Shine]	Accepted. Text reworded
2-295	A	4:49		Delete "validation" [Vincent Gray]	rejected
2-296	A	4:50		Delete "/validation" [Vincent Gray]	rejected
2-297	A	4:50		Replace "verification" with "evaluation" [Vincent Gray]	rejected
2-298	A	4:54	4:57	As I mentioned above, the classification is not consistent. "Biomass burning" is not a species and is not consistent with classifications of other groups. [Mian Chin]	Noted, Special problem carbonaceous aerosols requires explanation
2-299	A	4:54	4:54	It is stated that "The RF of separate aerosol species is less certain than the combined RF." Is this meant to imply that aerosol errors somehow cancel each other in some fortuitous	Noted, yes cancelation of errors will occur since observations give some

No.	Batch	Page:line From	Page:line To	Comment	Notes
				fashion? [Andrew Lacis]	constraints of the total aerosol. New observational based studies now exist for total aerosol RF
2-300	A	4:54	4:57	The summary states that separate aerosol species RF is less certain than the combined RF (-.2 +- .2 Wm2). Yet, the numbers given for the separate species RF have lower uncertainties than the combined RF (which is 100%). For example, sulphate is given as -0.4 +- 0.2 Wm2, which has an uncertainty of only 50%. I am confused about this statement and the numbers given. I feel the average executive summary reader will also be confused here. [Ellsworth Welton]	Noted, yes less uncertain as compared to a combined value with uncertainty estimate from error propagation. Changes made in the text.
2-301	A	4:55	4:55	Change from "organic carbon ~-" to "organic carbon -" [Brian Magi]	rejected
2-302	A	4:57	4:57	"mineral dust –0.2 to +0.1 W m–2." uncertainty estimates need to revised to be much larger based on comment 2 and published literature. [Natalie Mahowald]	Noted, the anthropogenic fraction of dust is increased and a new paragraph included in the dust section
2-303	A	4:57	5:5	"Significant changes in the aerosol direct RF…" and "For mineral dust the range in the direct RF is reduced…". See comment #1. [Dylan Millet]	Do not understand comment
2-304	A	5:4	5:5	For mineral dust the range in the direct RF is reduced due to the reduction in the anthropogenic fraction." This statement needs to be changed to reflect Comment 2 and published literature: "For mineral dust the range in the direct RF is increased due to increased uncertainty in the anthropogenic fraction. [Natalie Mahowald]	Noted, the anthropogenic fraction of dust is increased and a new paragraph included in the dust section
2-305	A	5:5	5:5	The anthropogenic fraction has NOT reduced - it is our estimates of that fraction that have reduced! [Keith Shine]	Ok
2-306	A	5:7	2:7	We avoided the use of the term "cloud albedo" in the TAR (at least in Ch. 5) because it is not specific--changes to liquid water path (which are normally associated with changes to precipitation efficiency) cause strong changes to the cloud albedo. If you want to avoid using "first indirect effect" you might say the RF of the droplet number change indirect effect. [Joyce Penner]	Rejected
2-307	A	5:7	5:7	the range of the best estimate -1.2 +/- 0.7 seems to differ from the number in 2-40, line 8 (-1.18 +/- 0.45) [Graham Feingold]	Noted
2-308	A	5:7	5:8	It would be better to avoid emphasizing the "low-level scientific understanding" label.	Rejected. They are not

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Hopefully, the error bars are sufficient to indicate the level of certainty. [Andrew Lacis]	
2-309	A	5:10		low-level=low altitude? [Cathy Clerbaux]	accepted
2-310	A	5:11	5:11	Add "understanding" (it is not just observations and models!) [A. R. Ravishankara]	accepted
2-311	A	5:14	5:17	First, I endorse the nomenclature used here of "cloud-albedo effect", "semi-direct effect", and "cloud-lifetime effect." These are simple and convey the nature of each forcing mechanism. (It is an improvement from "type-I" and "type-II".) However, I do not agree with the notion of relegating the semi-direct and cloud-lifetime effects to feedbacks. In this regard, I concur with all three arguments advanced by Steve Schwartz in his comments on this chapter and will not bother to repeat those arguments. The decision of what to call a forcing versus a feedback is largely arbitrary. Estimates of the semi-direct and cloud-lifetime effects are now available, as discussed in Chap 7 (see Figure 7.5.3). By relegating these effects to the category of "feedback," this chapter is artificially lowering the magnitude and narrowing the uncertainty of aerosol forcing as well as total anthropogenic forcing. In my opinion, it would be better to confront these uncertainties openly. To accomplish this, I recommend that the forcing data from Chap 7 (which includes data on the semi-direct and cloud-lifetime effects) be transferred from that chapter into this one and that both these effects be included in the summation of total aerosol forcing (as presented here in Table 2.9.1) and total anthropogenic forcing (as presented here in Figure 2.9.1). [Theodore Anderson]	Noted, but rejected. That part which cannot be folded into the definition of RF, we avoid calling it a forcing.
2-312	A	5:14	5:17	I support putting these aerosol-cloud interactions to be classified as climate feedbacks. [Jerry Mahlman]	Noted
2-2673	B	5:16	5:17	I do not think these effects can be categorised as feedbacks. There are indirect effects, ie they affect climate through a non-radiative forcing, but there is no feedback loop involved. [Olivier Boucher]	Noted. "Feedback" may not be a good choice of word to describe these effects, but they are certainly not "forcing" under the RF definition.
2-313	A	5:16	5:17	You will have to note somewhere that when these "effects" are assessed as feedbacks rather than forcings, the efficacy is very different from 1 (I.e. it is not in the range of +/- 25%) [Joyce Penner]	Accepted
2-314	A	5:16	5:17	I disagree with the description of the cloud lifetime effect as a "feedback". Its character is essentially that of a forcing, rather than the classical feedbacks such as surface-albedo and cloud feedbacks that have been studied for years. If you perturb the droplet concentration in a climate model, the liquid-water contents will "spin up" to their new values within a	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				few hours of simulation. Moreover, Kristjansson (JGR, 2002) showed that it is possible to calculate it as a pure forcing, so the only reason that most other modellers have not done so is that it is too much trouble. Similarly, the cloud-albedo effect is usually (in practice) calculated from the difference of two runs, because to calculate it as a pure forcing is rather complex in a model with interactive aerosol. (You would have to carry separate prognostic variables for natural and anthropogenic components of each aerosol species, and most models don't do this.) Thus, the main difference between the cloud-albedo and cloud-lifetime effects would seem to be that the latter is more uncertain. Perhaps this is sufficient justification for leaving it out of the main results in this chapter (though I am not convinced). I don't really expect that you are going to change this, but I want to register my protest! [Leon Rotstayn]	
2-315	A	5:16	5:16	Sorry to be a stuck record (or iPOD!). I think it bizarre to class semi-direct and lifetime effcsts as a feedback. Surely these are no more feedbacks than NOx driven changes in ozone. I just don't get the distinction. In my view, it is easier to define a feedback as someting driven via surface temperature changes, otherwise all indirect effects become feedbacks. [Keith Shine]	Noted. Framework will be considered carefully again. Not all feedbacks may be drivn by surface temperature changes.
2-316	A	5:17		I understand the motivation behind pushing the aerosol effect on cloud lifetime to feedback, but there area a few critical problems. Climate sensitivity is defined as temperature change per unit radiative forcing. Here aerosol generates a nonradiative forcing that is causing a temperature change. We shall not be able then to use the climate sensitivity. I would have moved back to the old definition and described the added reflected sunlight due to the aerosol effect on cloud lifetime. [Yoram Kaufman]	Noted. Framework will be considered carefully again. Note though that climate response due to aerosol-cloud interactions conceptually accounts for all the processes. It is when there is an attempt to separate "forcing" from "feedback" that the dilemma arises.
2-317	A	5:19	5:21	I would suggest to add estimated reduction of surface radiation here (it is more than a factor of 5 larger than the TOA RF!). Although IPCC reports don't count it as "forcing", the surface radiative perturbation is at least as important as the RF at the TOA. [Hongbin Yu]	Rejected. Text discusses this issue, there are considerable uncertainties in estimates due to these effects.
2-318	A	5:20	5:20	Why only 'could' affect the surface heat and moisture budgets? I would have thought they would affect the budgets without doubt. [Greg Bodeker]	accepted
2-319	A	5:20	5:20	"radiative flux" .. presumably you mean shortwave? [Keith Shine]	Accepted
2-320	A	5:25		What is "high scatter" of emission fluxes ? [Robert Levy]	Text reworded
2-321	A	5:26		I am not familiar with the word afforestation, maybe that is just me.	Text reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Michael Coffey]	
2-322	A	5:31		If "top-down" is identified, it should be defined. [Robert Levy]	Accepted. It will be
2-2674	B	5:34	5:34	human energy production (no capitals) [Olivier Boucher]	accepted
2-323	A	5:35	5:35	Does HEP really have a very low scientific understanding. I would think the global-mean figure has a very high level?? [Keith Shine]	accepted
2-324	A	5:37	5:38	The comment "factor of 3 to 4 smaller compared to values projected from TAR" should be relegated to the fine print rather than being part of the main headline. [Andrew Lacis]	Noted.
2-325	A	5:37	5:45	Similar to other areas of the executive summary this paragraph should include a definitive statement on the level of scientific understanding. Also from the standpoint of a policy-maker it would be helpful to know if that level of scientific understanding is sufficient, or not, to base decisions concerning mitigation measures, such as changes in air navigation systems or aircraft flight procedural changes (e.g. changes in altitudes for cruise operation). [Lourdes Maurice]	Accepted.
2-326	A	5:37	5:44	Continuing to report RF for very short-lived effects (one day) on the same page as those for things like CO2 that have accumulated and will continue to accumulate (and impact climate), will continue the misinterpretation of the relative importance of the sources. I know this is only the executive summary, but I hope when I get to the main text this is clearly explained. [Ian Waitz]	Noted.
2-327	A	5:37	5:38	Change "compared to" to "than the". [Ray Weiss]	Accepted.
2-328	A	5:38	5:38	Why did values need to be 'projected' from the TAR? Do you mean projected to present day contrail frequency? [Greg Bodeker]	Accepted.
2-329	A	5:38	5:38	Replace "Aviation may also alter cirrus clouds." with "Spread contrails and effects on cirrus clouds from aviation may be far more significant than the effects of linear contrails." [Howard Feldman]	Accepted.
2-330	A	5:40	5:40	Write ''smaller" instead of ''revised". [Mikhail Danilin]	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-331	A	5:41	5:43	The "as much as 8 times greater" is not consistent with the text presented in section 2.6.3 which says 1.8 to 10 [Steven Baughcum]	Accepted.
2-332	A	5:41	5:43	I did not understand where this statement come from. Text in Section 2.6 never mentioned that induced cloudiness by contrail spreading and aerosol effects on cirrus microphysics is ''... as much as 8 times greater than persistent contrail cover". Either revise this statement or add necessary references supporting this statement in Section 2.6. [Mikhail Danilin]	Rejected. Statement is in Section 2.6.3.
2-333	A	5:47	5:47	Some readers may wonder why the direct RF due to changes in the solar output (0.12 W/m^2) is so much smaller than the change in solar irradiance itself (0.3-1.6 W/m^2). [Greg Bodeker]	This is explained in the footnotes to Table 2.7.1 The revised ES now gives solar forcing as percentage changes or forcings, not irradiance.
2-334	A	5:47	6:7	The solar impact on climate should start with the fact that the solar cycle variability has been precisely monitored for nearly three decades, that a solar cycle amplitude of about 0.1% has been observed, that no significant longterm trend in solar irradiance has been observed, and that much of the solar irradiance variability occurs in the UV. Hence, longer term solar radiative forcing must be inferred from observed variations in solar activity indicators such as sunspot number and cosmogenic isotopes, or be deduced from backward engineering from the observed temperature record. The newly measured absolute solar irradiance being 5 W/m2 lower than previous results is noteworthy and clearly indicative of instrumental calibration problems. [Andrew Lacis]	Noted/Accepted. The ES has been reworded to include text of this type. Reference to the 5 W/m2 offset has been removed, as per comment 2-344.
2-335	A	5:48	5:48	Wow! A typo (W/m2) My first one in this Chapter. Replace the comma at the end of the sentence with a period. I now know that this very clean text is because it is part of the well-scrubbed Executive Summary. [Jerry Mahlman]	Noted.
2-336	A	5:51	5:51	For much of the readership, the difference between solar forcing and changes in TSI will be way too subtle - I would stick to forcing, otherwise people will confuse the 2.6 Wm-2 with the CO2 forcing and conclude … well, you know what they will conclude. [Keith Shine]	Accepted. Solar irradiance changes are now given mainly as percentage changes, or else as forcings. See response to 2-333.
2-337	A	5:53	5:54	I was surprised to see that the scientific level of understanding of the influence of solar changes had jumped from very low in last assessment to medium in this assessment. Although additional observations in which there have been no major volcanoes has strengthened the observational picture, confirming the response to solar forcing and increasing the statistical significance of those results, and we probably have a slightly better idea of what processes might transfer the solar signal down through the atmosphere to the surface I would maintain that our level of scientific understanding of the role of	Noted/Accepted. Level is now indicated as low.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				solar forcing on climate has, if anything, gone down! This view is based on the new work on the relationship between solar magnetic activity and irradiance. I think this has reduced our confidence in the reliability of past TSI reconstructions - and surely this is the important factor in climate assessment. [Lesley Gray]	
2-338	A	5:54	5:54	Given the conspicuous glitches in the various satellite measurements before "post processing" of insolation, I would call our understanding as "medium to low". [Jerry Mahlman]	Noted. See response to 2-337.
2-339	A	5:54	5:54	I'm a little surprized that you assess the level of understanding as "medium" rather than "low" [Joyce Penner]	Noted. See response to 2-337.
2-340	A	5:54	5:54	"medium" - has or understanding really improved so much that what was "very low" in TAR is now "medium"? There has certainly been a significant revision in the values, but arent we still stuck with the basic lack of observations before the satellite era. [Keith Shine]	Noted/Accepted. See response to 2-337. ES text has been reworded somewhat to convey this.
2-341	A	5:56	5:57	This suggests that SORCE TIM is more accurate than previous measurements of TSI. This may be the case but is not definitely established. [Joanna Haigh]	Noted. The difference between SORCE and other radiometers is a calibratin offset and is no longer mentioned in the text.
2-342	A	5:56	5:57	The following statement " New present day measurements indicate that the absolute value of total solar irradiance is ~5 Wm-2 lower than previous values" is quite important. I am not sure that confidence level associated with statement is very high. I would use more cautious wording, some thing like "New present day measurements from the SOURCE mission (?? is this SOURCE?? one needs to clearly identify the source of information !) indicate that the absolute value of total solar irradiance may be lower by ~5 Wm-2 than previous values". [Alexander P. Trishchenko]	Noted. See response to 2-341.
2-343	A	5:57	5:57	By 'previous values' do you mean the values quoted in the TAR? [Greg Bodeker]	Noted. Relevant text removed in revised ES, as per response to comment 2-341.
2-344	A	5:57	5:57	"previous values" Again, this implies that the Sun has changed by 5 Wm-2, whereas it is our understanding that has changed. Anyway, I do not think that this is an executive summary point - it has almost zero impact on our understanding of forcing [Keith Shine]	Noted. Relevant text removed in revised ES, as per response to comment 2-341.
2-345	A	6:4	6:5	Ozone column changes forced by solar UV are well established. I do not think this is true of the profile of the ozone response. [Joanna Haigh]	Noted/Accepted. Text added to revised ES to convey this.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-346	A	6:4	6:6	I am not sure that this statement has any quantitative meaning. A 3% change in the middle to upper stratosphere would be well within the range of the stratosphere's considerable natural variability. [Jerry Mahlman]	By the same reasoning a change of 0.8C in global surface temeprature is well within the noise of the climate system, and considerably smaller than the seasonal and durnal cycles, for example. Does this mean that the IPCC AR4 lacks quantitative meaning?
2-347	A	6:4	6:4	Models and observations of the solar driven ozone changes are still in only modest agreement. [Keith Shine]	Accepted. Clarified in the revised text that the changes are relatively well established empirically.
2-348	A	6:5	6:5	Are things really this certain? I thought there was still some uncertainty and controversy concerning the amplitude of the solar cycle in total column ozone. [Greg Bodeker]	Noted. See comment 2-345 – the total column changes appear to be well established empirically but the profiles less so.
2-2675	B	6:8	6:29	I am not sure we should treat stratospheric aerosols as relative to no stratospheric aerosols at all. Like for other RFs, the RF from strat aerosols should be relative to a reference period. Alternatively it could be scaled relative to an average value (so that it is 0 on average). [Olivier Boucher]	Accepted. However, contribution from non-volcanic sources in the quiescent state (background state) is so small that this is virtually identical to no-aerosols in the stratosphere. The fact that this is a transitory forcing (of ~2 years duration only) makes it difficult to scale relative to some reference.
2-349	A	6:11	6:14	These sentences are poorly worded and need to be clarified. In particular it sounds like the AO is somehow related to volcanoes? [Greg Bodeker]	Noted. Will revise text.
2-350	A	6:11	6:12	The stratospheric aerosols from the Pinatubo volcanic eruption have been accurately monitored primarily by SAGE II spectral extinction measurements, but also from infra-red limb sounding CLAES and ISAMS measurements, and also from ground based observations. Knowledge of both the optical depth and particle size of the Pinatubo aerosol are probably accurate to within 10%. Balloon borne in situ measurements have identified the aerosol composition unequivocably as concentrated sulfuric acid. From this information, along with knowledge of the aerosol height distribution measured by SAGE II, the radiative forcing for both solar and thermal radiation components can be accurately computed. That several models provide simulations within 75% of observed shortwave anomalies is not really a selling point since there is no real way to actually measure a shortwave (or longwave) anomaly due (only) to the volcanic aerosol. Solar reflectance and LW radiance measurements contain unknown contributions from other atmospheric	Taken into account. Text inside will also be modified. Need to emphasize the fact that the solar reflectance and LW measurements cannot be taken to be quantitative measures of the forcing because of the element of response contained in these variables..

No.	Batch	Page:line From	Page:line To	Comment	Notes
				constituents. [Andrew Lacis]	
2-351	A	6:14	6:14	I thought volcanic aerosols only had a significant affect on stratospheric ozone in a high chlorine world. [Keith Shine]	Taken into account. Text inside will also be revised to convey the sense.
2-352	A	6:16	6:17	Is here any other significant non-LLGHG other than ozone? It would be simpler to just state that RF spatial patterns remain uncertain for ozone, aerosols, and land-use albedo changes. The spatial patterns of aerosol effects on clouds are more than likely to follow the pattern of aerosol uncertainties. [Andrew Lacis]	Noted. Will revise.
2-353	A	6:16		Delete LLGHGs and state which ones you mean. [Vincent Gray]	Noted. Ozone is the non-LLGHG here.
2-354	A	6:17	6:17	It should also be mentioned that even the well mixed GHGs have significant latitudinal gradients, and significant departures from uniform mixing in their vertical profiles (e.g., Minschwaner et al., 1998) - all of which impact their RF. [Andrew Lacis]	Accepted. This is, however, much less uncertain than is the case for the other agents. Will revise.
2-355	A	6:17		Delete LLGHGs and state which ones you mean. [Vincent Gray]	Taken into account. Earlier text in ES points out the gases which are LLGHGs.
2-356	A	6:21	:27	The instantaneous radiative flux change at the surface (hereafter called "surface forcing")The total global-mean surface forcing is very likely to have been negative. Surely it is intended that the surface forcing referred to here, which is denoted as negative, refers only to the shortwave radiative flux, and not the longwave radiative flux, which presumably, has increased as a consequence of the (enhanced) greenhouse effect. Yes? Care should be given (here and in the body) as to whether this shortwave flux change is meant to be downwelling or net flux.Language: Better "hereinafter" than "hereafter". "Hereafter" means for all time (with connotation of the afterlife), whereas "hereinafter" restricts the definition to the present document, which I think is what is intended. [Stephen E Schwartz]	Noted. Will switch to "hereinafter". No, it is the instantaneous *net* radiative flux change that is referred to here. Instantaneous longwave forcing does show an increase but this is not as large because of the overlap with water vapor bands, and these are already 'saturated' over most of the spectrum. Note the SH regions, where the LW effects dominate.
2-357	A	6:23	6:24	Surface radiative forcing, like TOA radiative forcing, could certainly be calibrated to represent an estimate of global mean surface temperature response. But it is not a simple calibration that could be universally applied to all types of radiative forcings since an increase in radiative forcing at the ground surface does not necessarily imply that the ground temperature will also increase. To connect surface radiative forcing to global surface temperature change requires knowledge of the vertical profile of energy deposition and emission. That is why the net flux change at the tropopause is used to represent radiative forcing. Monitoring changes in surface radiative forcing is,	Noted. Will amend text.

No.	Batch	Page:line From	To	Comment	Notes
				nevertheless, important since these changes are an important climate diagnostic and can be compared to measurements of surface irradiance and surface energy balance compiled over a world-wide network. [Andrew Lacis]	
2-358	A	6:23	:24	However, unlike RF, it [surface forcing] does not represent a measure of the global mean surface temperature response.The implication of this statement that RF (Radiative forcing at the TOA) represents a measure of global mean surface temperature response should be corrected. RF does NOT represent a measure of the global mean temperature response. Forcing is not response. [Stephen E Schwartz]	Noted. Will amend text.
2-359	A	6:26	6:35	These last two paragraphs would benefit from one or two tutorial sentences that explain why some of these statements e.g. 'at the surface, tropospheric and stratospheric aerosols are the dominant contributors to the negative surface forcing'. [Greg Bodeker]	Noted. Will amend text.
2-360	A	6:26	6:35	These are some very interesting new insights. [Jerry Mahlman]	Thank you!
2-361	A	6:26	6:29	I am not sure in high confidence of the following statement "The total global-mean surface forcing is very likely to have been negative whilst the total RF is positive. LLGHGs have been the principal contributor to RF, with aerosols providing some offset. In contrast, at the surface, tropospheric and stratospheric aerosols are the dominant contributors to the negative surface forcing". There is a substatial negative trends in surface albedo over temperate and polar regions in the Northerm hemisphere as detected from the ISCCP data over last 20 years, that may provide more positive RF than it was estimated earlier. This positive forcing due to albedo change may offset negative RF due to aerosols. Reference: Wang, S., A.P.Trishchenko, K.V.Khlopenkov, A.Davidson, 2005: Comparison of IPCC AR4 climate model simulations of surface albedo with satellite products over Northern Latitudes Journal of Geophysical Research. Revised. [Alexander P. Trishchenko]	Noted. The time period in question here is the preindustrial (1750) to present. Land-surface estimates made for this period do not suggest that these have dominated over aerosol effects in terms of the global-mean. Locally, of course, this could be different.
2-362	A	6:27		Delete LLGHGs and state which ones you mean. [Vincent Gray]	Rejected. Since the gases which are LLGHGs have been mentioned earlier in the ES, it need not be repeated here.
2-363	A	6:50	6:52	To what extent do changes (relative to TAR) reflect choice of start date relative to Maunder minimum? [Ian Enting]	They do not
2-364	A	7:1	7:9	The discussion should begin by focusing first on what is known about the observed changes in radiative forcing agents. [Andrew Lacis]	Rejected. It already does

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-365	A	7:3	7:41	There is a mixture of tenses: present, future, present perfect. An harmonization of tenses would improve an easy-to-follow logical continuation. I would suggest to make sentence like: "…are discussed in the section…" or the chapter presents ….in section....". [G. H. Sabin GUENDEHOU]	Accepted
2-366	A	7:3	7:41	While this introductory section claims that this chapter "will focus on what is needed to explain the trends in forcing agents", I believe it falls short of this objective with respect to adequately discussing changes in atmospheric composition. The fact that about 80% of this introduction is about RFs, and only a couple of sentences are devoted to changes in composition, is consistent with the bias of this chapter. If the balance of this chapter is changed (see comments above), then the balance of this introduction should also be changed. [Ray Weiss]	Accepted. Composition discussion expanded
2-367	A	7:6	7:6	Avoid the use of "what is needed". It is good to say what is need i.e. the main things. [G. H. Sabin GUENDEHOU]	Accepted
2-368	A	7:10	7:28	A case could be made that all climate forcings are radiative in nature. Perhaps one might exclude the dynamic and mechanical transports of heat energy, but in a sense, they are simply feedback effects driven by temperature gradients that are set up by differential radiative heating and cooling. The different climate forcings are known and recognized by their radiative consiquences. Thus, we recognize the radiative forcing by dust aerosols, and do not attribute the resulting climate forcing to the wind that raised the dust particles. Similarly, climate forcing by CH4 conversion in the stratosphere and irrigation in the troposphere must first be formulated in terms of how much extra water vapor is deposited (where) in the atmosphere due to the physical processes in question. Then it is relatively straight forward to calculate the radiative forcing. The same applies to the various aerosol indirect effects. It would be nice if model physics could incorporate them directly, but absent that, various parameterizations are devised to include the relevant radiative effects to the extent possible. [Andrew Lacis]	Partially accepted. Wording will be clarified
2-369	A	7:11	7:11	Replace 'Natural solar' with 'Natural, solar'. Or do you really mean 'Natural solar'? What other types of solar are there? [Greg Bodeker]	Accepted
2-370	A	7:14	7:14	So given that for the troposphere only changes in water vapour from irrigation are considered, I am assuming that only direct anthropogenic influences on tropospheric water vapour are considered? Things like increases in tropospheric temperatures (from anthropogenic activities) and hence the ability of the troposphere to hold more water vapour are not considered to be direct anthropogenic perturbations to tropospheric water vapour. Maybe this needs to be highlighted when the scope of this chapter is discussed.	Accepted. Water vapour feedback will be eluded to here

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Greg Bodeker]	
2-371	A	7:19	7:19	Delete "say" [Dylan Millet]	Accepted
2-372	A	7:19		Delete LLGHGs and state which ones you mean. [Vincent Gray]	Rejected – will spell out on first use
2-373	A	7:20	7:21	as these mechanisms are not routinely or well represented in most current GCM simulations - carry this forward to summary as a caveat [Stephen McIntyre]	Rejected. Beyond scope of ES
2-374	A	7:21	7:21	GCM should be defined here instead of on the page 8, line 14. [G. H. Sabin GUENDEHOU]	Accepted
2-375	A	7:29	7:41	These are valuable paragraphs that describe the nature of what is treated in Chap 2 ("bottom-up" forcings) as well as the organization of forcing-related topics among the all the chapters. The distinction between "bottom-up" and "top-down" estimates is important and is nicely described. However, the nomenclature could be improved. The terms "forward calculations" and "inverse calculations" more accurately convey the logical difference between these two methods. These were the terms recommended by Anderson et al. (2003, Science, 300, 1103-1104) - a team of authors from both camps who undertook a systematic comparison of the two approaches. [Theodore Anderson]	Accepted
2-376	A	7:29	7:35	It should be made clear that once the atmospheric structure, composition, and temperature profile have been determined, the radiative fluxes can be accurately computed throughout the atmosphere. Large databases of atmospheric line data exist (e.g., HITRAN, GEISA, none of which are referenced). Closure experiments (e.g., Turner et al., 2004) have validated line-by-line calculations of downwelling longwave flux to better than 1.5 W/m2 over a wide range of atmospheric conditions. However, a similar comprehensive database of radiative parameters is not available for the refractive indices of the different atmmospheric aerosol compositions. This leads to the need for educated approximations, proxy indicators, chemistry-transport model simulations, and reverse engineering constraints to obtain reasonable estimates for aerosol contributions to the radiative forcing of climate change. [Andrew Lacis]	Rejected .Too much detail for this section
2-377	A	7:36	7:36	Perhaps a generalized discussion of radiative forcing in climate might be useful. The climate system is always undergoing radiative forcing, and it is never ever actually in equilibrium with the applied radiative forcing. The largest radiative forcing occurs diurnally with the rising and setting of the sun, whereby the solar irradiance may very from more than 1000 W/m2 at high noon to zero at night. There is a corresponding change in temperature that may be larger than several tens of degrees C, although over	Rejected. Unnecessary background already covered in previous assessments

No.	Batch	Page:line From	Page:line To	Comment	Notes
				ocean surfaces, the diurnal temperature change may be near zero. There is also a seasonal change in solar irradiance that may be as large as several hundred W/m2, and the corresponding temperature change, at some locations, may be even larger than the diurnal temperature change. Taken as a global average, the Earth receives about 20 W/m2 more during January than it does during July as a reslt of the Earth's orbital eccentricity. But, renmarkably, the global mean surface temperature in January (with more incident solar energy) is actually several degrees colder than it is during July. [Andrew Lacis]	
2-378	A	7:36	7:37	All of these radiative forcings are explicitly modeled in typical climate GCMs. However, none of these radiative forcings are contributors to what is typically described as the measure of radiative climate forcing, anthropogenic or otherwise. The quantity that is generally defined as the measure of radiative forcing is not even a diagnostic output quantity in a climate GCM. In fact, significant effort is required in order to obtain a well defined measure of radiative forcing from typicall climate GCMs since a number of physical processes and model feedbacks need to be shut down or constrained so as to avoid feedback contamination. [Andrew Lacis]	Agree, but to relevent to this section
2-379	A	7:39	7:40	The draft says that "Future RF scenarios that were presented in Ramaswamy et al. (2001) are not updated in this report." But, it is doubtful whether this policy is sufficiently convincing at this stage. For example, Dr. Hansen pointed out that these scenarios are rather pessimistic (http://www.sciam.com/media/pdf/hansen.pdf). I understand that conventional scenarios are still useful, but the question raised by Dr. Hansen is already well known through internet. Thus, the readers of the IPCC report will feel strange if such questions are neglected, and hence, the value of the report might be adversely affected. [Kiminori Itoh]	Rejected. Scenarios Discussed in chapter 10 which we reference
2-380	A	7:43	7:43	This section should be moved to follow the Chemically and Radiatively Important Gases section. [Andrew Lacis]	Rejected. Idea discussed. Will empasize llghg section more though
2-381	A	7:43		line 43 ff Here or very close to here a reason should be given why radiative forcing needs to be known and why its uncertainty needs to be bounded. I suggest that a key need to know radiative uncertainty is as input to empirical determination of Earth's climate sensitivity and/or as input to climate models. See my paper Schwartz S. E., Uncertainty requirements in radiative forcing of climate change. J. Air Waste Management Assoc. 54, 1351-1359 (2004). which sets out these reasons and provides estimates of the accuracy with which forcing needs to be known. The need for knowing forcing should be stated here and discussed in the body. Only when the accuracy requirements are specified will we be able to say	Partailly accepted, text modifed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				whether forcing is known well enough for a given purpose. [Stephen E Schwartz]	
2-382	A	7:45	7:50	This paragraph is confusing and needs to be reworded. In particular saying that 'The RF approach is used to avoid uncertainties associated with modelling the actual climate response' is a little misleading. The RF approach simply divides the problem in half. It tackles what can be calculated quite accurately i.e. the change in energy balance resulting from a change in concentration of a GHG, and leaves the less certain part i.e. the calculation of how that change in energy balance manifests as a change in surface climate (which in turn depends on the somewhat uncertain climate sensitivity parameter) to someone else. It certainly doesn't avoid the uncertainty associated with modelling the actual climate response. It just postpones it. [Greg Bodeker]	Accepted. Text reworded
2-383	A	7:45	7:45	Are there other means to assess climate change agents? [G. H. Sabin GUENDEHOU]	Noted
2-384	A	7:45		This chapter assesses climate change agents through RF. RF is intended to be a simple measure for both quantifying and ranking the many different climate change mechanisms. It quantifies mechanisms in terms of a W m –2 change in the radiative energy budget. Attention needs to be paid to the use of the word "mechanism", which appears to include climate system response in addition to causative influence, which "forcing" is intended to denote. I suggest the following language: RF is intended to be a simple measure for both quantifying and ranking the many different influences on climate change. It quantifies these influences in terms of a W m –2 change in the radiative energy budget. Better to distinguish what quantity is a measure of from the units of the quantity; these are independent: It quantifies these influences in terms of changes in the radiative energy budget. RF is conventionally measured in units of W m-2. Even better, to make clear that the several forcings refer to different components of the radiative energy budge: It quantifies these influences in terms of changes in different components of the radiative energy budget. RF is conventionally measured in units of W m-2. Even better, to note that these components are, in fact, the several fluxes that comprise Earth's radiative energy budget: RF is intended to be a simple measure for both quantifying and ranking the many different influences on climate change. It quantifies these influences in terms of changes in the several fluxes that comprise Earth's radiative energy budget. RF is conventionally measured in units of W m-2.	Accepted .Text reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Finally, why "is intended to be"? RF is a simple measure for both quantifying and ranking the many different influences on climate change. It quantifies these influences in terms of changes in the several fluxes that comprise Earth's radiative energy budget. RF is conventionally measured in units of W m-2. I suggest attention be paid throughout to the use of "mechanism". [Stephen E Schwartz]	
2-385	A	7:46	7:46	quantifying and ranking the many different mechanisms driving climate change. [Ian Enting]	Accepted .Text reworded
2-386	A	7:46	7:48	The sentence beginning "Despite many aspects …" is very important for laying interpretive foundations for the report as a whole, but it is phrased in an awkward way that conceals its meaning. I suggest the following slight alteration: "Many aspects of the climate system are qualitatively well-understood, but climate sensitivity and other key aspects of the response of the climate to external forcings are poorly quantified." [Ross McKitrick]	Accepted .Text reworded
2-387	A	7:47	7:48	include clear statement that solar radiation has low-entropy and that IR radiation has high-entropy and that the forcing properties may differ accordingly and the differences may be significant [Stephen McIntyre]	Rejected. Meaning not understood
2-388	A	7:48	7:48	...qualitatively well understood, climate sensitivity and other aspects of climate response need improved quantification. [Jerry Mahlman]	Accepted. Text reworded
2-389	A	7:49	7:49	A clear discussion of radiative forcings, feedback effects, and equilibirum climate response is needed. The first quantitative discussion of radiative forcings and feedback effects is given by Hansen et al. (1984) (see also Hansen et al., 1997). Hansen et al. (1984) express their radiative forcing for doubled CO2 (and 2% solar irradiance increase) in terms of Delta-T-zero, which is the equivalent of adjusted forcing, but expressed in terms of a global surface temperature change with no feedbacks allowed to operate. For estimating global climate change, this is actually a more robust quantity than adjusted forcing. Lacis and Mishchenko (1995) show that for a globally uniform forcing, such as doubled CO2, Delta-T-zero is essentially independent of latitude while the adjusted flux has a significant latitudianl dependence because it depends directly on the magnitude of the local Planck radiation, whereas Delta-T-zero has already taken that into account. [Andrew Lacis]	.Partailly Accepted. Section will be expanded, clarified and reworded. Hansen formaulation discussed elesewhere
2-390	A	7:50	7:50	Hansen et al. (1984) show that the radiative equivalent of different climate feedback contributions can be readily identified and quantified. Upon running the doubled CO2 model to equilibrium, precise changes in water vapor distribution, clouds, lapse rate, and	Rejected. Ideas discussed elsewhere

No.	Batch	Page:line From	Page:line To	Comment	Notes
				surface albedo can be tabulated from the GCM diagnostic output, which can then be easily evaluated using a 1D radiative-convective model. Lacis and Mishchenko (1995) perform this evluation with a 2D model and include also the latitudinal contribution of advective feedbacks which, by definition, average to zero globally. This analysis is made possible because the total equilibirum surface temperature response in the doubled CO2 exeriment has to be completely sustained by the radiative effects due to the changes in water vapor, lapse rate, clouds, and surface albedo that were induced by the total temperature change. (This shows, in effect, that the change in global temperature can serve as a "medium of exchange" between different feedback effects, even though cloud formation is not directly a function of temperature.) [Andrew Lacis]	
2-391	A	7:51	7:51	Hansen et al show that while the feedback efficiencies of the different feedback processes can be compared in linear fashion, the feedback effects on the global surface temperature are multiplicative in nature and do not combine linearly. While the radiative effects of atmospheric constituents can be evaluated with good accuracy, the model physics involved in producing the different feedback processes are necessarily more complex, and thus differ more widely between different GCMs - hence the rationale to express climate forcings in terms of adjusted radiative forcings instead of equilibrium surface temeprature changes with model feedbacks included. [Andrew Lacis]	GISS is only one model. This section makes general true statements about forcing-response
2-392	A	7:52	7:52	This is a poor figure with which to illustrate climate change processes. It requires significant improvement or preferably removal from the Report. First, is "non-radiative" forcing not just another name for some feedback process? A change in lapse rate or geopotential energy may be considered to be non-radiative, but those are feedback effects. Should irrigation be just another part of ground hydrology? What feedback processes are there that lead from Climate Response to Human Activities? Perhaps warmer temperatures may lead to more humans and more fossil fuel consumption for running air conditioners. Water vapor, clouds, snow, and ice should be included as part of Changes in Climate System Components. But these changes are not caused directly by greenhouse gases - they are the result of feedback processes. Chemical processes produce diferent aerosols, and this includes volcanic aerosols. But there are aslo wind driven aerosols, which certainly have a radiative effect, but the extent to which dust or sea salt aerosols are important to anthropogenic climate change forcing is a question. [Andrew Lacis]	Rejected. Figure will be improved but it portrays conceptual arguecemnt of chapter
2-393	A	7:52	7:53	Whether some radiative constituent is to be treated as a radiative forcing or feedback depends on the nature of the model. Typically, current climate GCMs treat ozone changes as radiative forcing. But in an interactive chemistry-GCM, ozone changes would	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				become part of the model feedback processes. The same considerations apply to aerosol indirect effects. With a physically robust microphysical model, the aerosol indirect effects would become a part of the model's feedback processes. The more physically realistic climate GCMs become, the more difficult it is to draw clear distinctions between forcings and feedbacks. It would appear that all of the atmospheric water vapor is due to feedback processes (except for that due to CH4 conversion in the stratosphere, and some tropospheric water vaor due to irrigation). There are temperature dependent feedback processes that directly affect the CO2 and CH4 amounts in the atmosphere. But in the context of current climate GCMs, the full atmospheric amounts of CO2 and CH4 are invariably being prescribed as external radiative forcings. [Andrew Lacis]	
2-394	A	7:54	7:57	The definition of radiative forcing (RF) perhaps should be re-labeled as radiative climate forcing (RCF) since RF specifically does not refer to the radiative forcings that are actually used in a climate GCM, referring instead to climate forcings that are radiative in nature. The standard definition of RCF arises naturally in the context of 1D radiative-convective model operation. However, in the context of a climate GCM a number of difficulties arise. First, the location of the tropopause is not well defined. Second, since RF depends on the local temperature profile, water vapor distribution, and cloud condition, there is an implied need to average the RF results globally and annually. Instantaneous flux changes may be easily obtained, but adjusted forcing requires special handling and an arbitrary definition of the tropopause. There are also multiple objecives to keep in mind. One objective is to assess and compare the performance of radiation codes, the other to define a standardized means for comparing the efficacy of different climate forcings. [Andrew Lacis]	Rejected. Historical precident and well defined meaning of RF
2-395	A	7:54	8:6	Definition of RF is confusing and not consistent with the results presented in the chapter. See my comment #1-2 above. [Mian Chin]	Partially accetped – text reworked
2-396	A	7:54	8:7	A rigorous definition of forcing is presented here. But discussion of aerosol RF later on doesn't adopt this definition, i.e., not at the tropopause, not allowing for stratospheric temperatures to readjust. Some explaination is needed. Should we clarify that the forcing refers to the radiative perturbation due to anthropogenic activities ONLY? [Hongbin Yu]	Noted. Text will be reworked
2-397	A	8:0	18:0	Couldn't all of this be "donated" to, or be a key part of, a later section on budgets of the greenhouse gases? This is very interesting, valuable, and informative work that I admire and find to be very important and relevant. However, it contains almost exclusively the observational information and its underpinning science that provides invaluable context	Rejected. Needed up front to lay groundwork for chapter, but beeter context applied

No.	Batch	Page:line From	Page:line To	Comment	Notes
				for the quantification of RF in Chapter 2, but it does not assume the more direct responsibility for the quantification of RF that is the major commission for the authors of Chapter 2. I assume that this suggestion will likely be rejected, so I will make my comments as if the current placement for this section stands. Actually, this section(pages 8-18) is very well written and informative, and would add nicely to this Chapter, even if parts of it appear to be misplaced, and seemingly burdened by too much, very interesting detail that doesn't speak very directly to the core purpose for the existence of Chapter 2. [Jerry Mahlman]	
2-398	A	8:3	8:7	Complicated: RF=F=Fa. I like the cartoon but as Fa,Fs,Fg are discussed in detail later (chapter 2.8.4) maybe remove these sentences here? [Cathy Clerbaux]	accepted
2-399	A	8:3	8:3	Section 2.2 already idenitfied Radiative Forcing = RF. Re-introducing it as the variable F here is confusing, as is the subsequent development of the RF components. [Charles Miller]	Accepted. Equation dropped
2-400	A	8:3	8:3	"Radiative forcing (F)…" Acronym already defined in section title as RF. [Dylan Millet]	Accepted. Equation dropped
2-401	A	8:3		Delete "LLGHGs" throughout and state which gases you mean in each case. It is a cause of confusion. How long is long? [Vincent Gray]	Rejected.defined on first use
2-402	A	8:5	8:6	"Radiative Forcing (F) can be related..." Why "F" when it is "RF" everywhere else? Is this a typo? [Patrick Hamill]	Accepted. Equation dropped
2-403	A	8:5	8:5	The formula "Delta T = Lambda F" is presented without explanation and without even a reference. Since the entire underlying concept in this chapter is that surface warming is proportional to the forcing, I am surprised to see this formula introduced with no justification. In my opinion, the assumption made in that formula is one of the weakest links in the global warming argument. I expected to find a very extensive discussion of this point. Is it elsewhere in the Assessment Report? If so, a cross reference should be given! Later in the chapter (Section 2.8) there is a discussion of "Utility of Radiative Forcing." and a cross reference to Section 2.8 is given. However, Section 2.8 does not address the validity or theoretical basis of the formula either. [Patrick Hamill]	Partailly accepted. Referecnes and conext given
2-404	A	8:5	8:5	It is important to keep a clear distinciton between radiative forcings, feedbacks, and climate sensitivity. Radiative forcings, be they instantaneous or adjusted, have a reasonbly clear definition, and are expected to be basically reproducable between different models. The radiative forcings have a direct linear relationship to the no-feedback temperature change Delta-T-zero as described by Hansen et al. (1984).	Text reworded for clarity – Hansen idea rejected

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Feedback effects, which combine to produce a model's "climate sensitivity" to an applied radiative forcing, can vary considerably from model to model, and they do not combine linearly - as illustrated by the +2% and -2% solar flux experiments described by Hansen et al. (1997). The adjusted forcing and Delta-T-zero are seen to have a perfectly symmertic response, but the equilibrium surface temperature changes are quite different in the two experiments because of the strong non-linearity in the model's snow/ice feedback response. Note also the nonlinearity in the CFC increase combined with ozone decrease experiment also described by Hansen et al. (1997). [Andrew Lacis]	
2-405	A	8:7	8:7	After line 7, some cautionary notes on the equation in the previous paragraph are needed, in particular concerning the scientific limitations of the variables and the equation itself. I suggest the following text: "Note that this equation is not a physical theory (i.e. a law of nature), it is a model that expresses an empirical relationship. It is of necessity an abstraction, but not an exact function. The "mean equilibrium temperature" is not itself an actual physical quantity, since the Earth never experiences climate equilibrium, nor does the (non-equilibrium) temperature field have a unique, well-defined mean value. Hence DTs is not directly observed, it is a model calculation. Likewise RF itself is not observable or measurable, it is computed by a model that is designed to represent, in simplified form, the complex behaviour of the actual climate system in response to observed and hypothetical changes." [Ross McKitrick]	Partiallyaccepted. Equation introduced with caveats
2-406	A	8:9	8:9	Increase the font size in figure 2.2.2 [MARCOS S. P. GOMES]	Accepted
2-407	A	8:11	8:15	Regarding off-line radiative transfer schemes, Section 10.2.1 of Global Climate Projections really belongs here as part of the Radiative Forcings discussion. Priority should be given to the line-by-line results, even though they are for instantaneous forcing for clear-sky conditions. The line-by-line results represent the most accurate conversion from atmospheric GHG changes to W/m2 consequences. The further comparison to GCM radiation models gives a clear indication of how accurately the radiative effects of atmospheric GHG changes (and water vapor) are represented in GCM simulations. Clearly, flux change differences at the tropopause do not tell the whole story of how accurately radiation is being modeled. Flux changes at TOA and ground surface provide additional information, as do the accurate rendering of stratospheric cooling and overlapping absorption. Adjusted radiative forcings, and how they might get averaged between clear and cloudy conditions, are more of a diagnostic quantity and thus of lesser significance, particularly since they are not a direct GCM output result. [Andrew Lacis]	Rejected. Retained in section 10.2.1 – cross reference made

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-408	A	8:11		Delete "LLGHGs" throughout and state which gases you mean in each case. It is a cause of confusion. How long is long? [Vincent Gray]	Rejected
2-409	A	8:12		Delete "LLGHGs" throughout and state which gases you mean in each case. It is a cause of confusion. How long is long? [Vincent Gray]	Rejected
2-410	A	8:14	8:14	suggest using "model output" instead of data when considering models [Graham Feingold]	Accepted
2-411	A	8:17	8:20	This sentence hints at things without giving the reader any resolutions or hard conclusions. Is the debate unresolved? [Greg Bodeker]	Text clarified
2-412	A	8:20	8:20	Practically speaking, all climate forcings can effectively be considered to be radiative in nature in that when introduced into the climate system, their presence becomes known through their radiative effects. There may well be many physical events - irrigation, building dams and canals - that are not directly radiative in nature, but which eventually produce radiative effects as a consequence of subsequent impacts, for example, on the water vapor redistribution that might not otherwise have taken place. There may also be many physical interactions (feedback processes) that occur in nature that are not being explicitly modeled in climate GCMs. Their consequences (if not included as parameterizations) may be prescribed as a change in some radiative constituent and thus become a forcing as far as the climate GCM performance is concerned. Thus, from the model's point of view, only "radiative" forcings produce temperature change, with additional impact on the magnitude of the temperature change due to different model feedback processes that may be involved in responding to the applied forcing. [Andrew Lacis]	Text clarified
2-413	A	8:20	8:20	Fa should read Fa (with the a subscripted) and reference to figure 2.2.2 should be given. [Philippe Tulkens]	Equation dropped
2-414	A	8:21	8:23	The direct surface temperature change that is required in order to restore the radiative energy balance that existed before the radiative perturbation occurred, is the Delta-R-zero quantity described by Hansen et al. (1984). The full equilibrium surface temperature response that is achieved when the model reaches its new thermal equilibrium includes the effects of feedback contributions. If the equilibrium surface temperature change is greater than Delta-T-zero, this is a clear indication that the feedbacks are positive. If the equilibrium temperature change is smaller than Delta-T-zero, the feedbacks are negative. [Andrew Lacis]	Noted
2-415	A	8:25	8:29	In principle, the preparation of fossil fuels for burning at specified geographic locations could be considered climate forcings at their basic non-radiative level, assuming then that	Noted. Text clarified

No.	Batch	Page:line From	Page:line To	Comment	Notes
				chemistry-transport climate modeling will make all the appropriate conversions and transformations to place the resulting CO2 and aerosols into the atmosphere where these radiative constituents can then perform their radiative activities. This approach tends to digress from the basic objective of assessing existing changes in atmospheric constituents and their corresponding radiative forcing. There certainly are energy transports other than radiation that are important in climate processes and climate change. But these are more conveniently fitted into the category of feedback processes. Thus on balance, since this paragraph does not contribute to clarity, it shoud be omitted. [Andrew Lacis]	
2-416	A	8:25		Make a link with Figure 2.2.1 [Cathy Clerbaux]	Accepted
2-417	A	8:28	8:29	It is inconsistent to call the role of moisture availability a RF, rather than a response (as changes to clouds from aerosols are considered) [Joyce Penner]	Accepted
2-418	A	8:31	8:38	This para is not clear. What is meant by "To evaluate the climate response"?: running a GCM?, using a climate sensitivity parameter? Similarly GWPs won't tell you the climate response - just how relatively important a particular injection of a particular gas is. [Joanna Haigh]	Accepted. Text reworded
2-419	A	8:31	8:34	It is not clear what point is being made here. Perhaps it is intended to discuss aspects of radiative forcing efficacy (section 2.8.5). [Andrew Lacis]	Accepted. Text reworded
2-420	A	8:34	8:37	I recommend that you emphasive that if one wishes to understand the net impact of a new unit of emissions they must integrate over the lifetime of its effect. The difference in relative importance one interprets from rank ording RF, compared to ranking GWP can be significant when comparing short-lived and long-lived effects (for aviation in particular). I also suggest that you note that for RF one typically looks at the cumulative effect of the reservior -- e.g. the built-up emissions of CO2 -- and this is different from the marginal future impacts of a new unit of emissions (as would be more appropriate for setting policy -- since policies can only impact the future, not the past). More attention should be given here at the beginning to qualifying the usefulness of RF as a metric. [Ian Waitz]	Accepted
2-421	A	8:35		Insert: "from a unit pulse emission", relative to CO2, "and are a way…." [Michael Coffey]	Accepted
2-422	A	8:42	8:42	Do not italicize chemical formulae, even if included in italicized text. [Ray Weiss]	Noted ...editorial and style issues decided by the TSU
2-423	A	8:42	12:10	This subsection contains a lot of information, but is not well organized and structured. The many paragraphs discusses mainly (1) levels of atmospheric CO2 (including present	Accepted ...good suggestions to improve the logical flow ..text

No.	Batch	Page:line From	Page:line To	Comment	Notes
				and past), (2) technical approaches to obtain the concentrations of atmospheric CO2, (3) growth rate of atmospheric CO2 and its change, (4) emission of CO2 and its variation, (5) distribution of fossil fuel CO2 among different parts of the Earth system, (6) radiative forcing of atmospheric CO2. All this should be discussed, but in a better structure. Information regarding a same topic has been distributed in different paragraphs apart from each other. For example, the growth rate of CO2 concentration is discussed between line 53 of page 8 and line 8 of page 9, and between line 1 and line 7 of page 11; emission is discussed between line 10 and 15 of page 9, between line 15 and 20 of page 10, and between line 23 and 27 of page 11; forcing is discussed between line 17 and 23 of page 9, and between line 46 of page 11 and line 8 of page 12. I suggest to re-organize the text in this subsection and believe this will lead to more concise and readable text. [Xiaobin Xu]	reordered
2-2676	B	8:42		I don't think we need a summary of the section at the top. Some numbers in the second paragraph are repeated further down, sometimes with small changes. Since there is an executive summary, the section shouldavoid duplication of information, which could possibly make it a bit shorter. [Olivier Boucher]	Accepted ...repetition removed
2-424	A	8:42		Section 2.3.1. Units. Chapters 5 and 7 (and the TAR) used PgC/y and instead of GtC/y. It would be easier if these units would be used here as well. [Corinne Le Quere]	Accepted Gt-C changed to Pg-C
2-425	A	8:44	8:44	Replace 'RF on any' with 'RF of any'. [Greg Bodeker]	Accepted ..change made
2-426	A	8:44	8:45	The second sentence needs to be reformulated since it is not clear. [G. H. Sabin GUENDEHOU]	Accepted
2-427	A	8:44	8:44	In the context of accumulated GHG forcing since 1750, CO2 is the largest contributor. [Andrew Lacis]	Accepted ..sentence rewritten
2-428	A	8:44	8:44	Typo: "… has the largest RF of any LLGHG …" [Keith Lassey]	Accepted change made
2-429	A	8:44	8:44	Change "on any" to "of any" [Brian Magi]	Accepted change made
2-430	A	8:44	8:44	. ....has the largest RF of any LLGHG... [Jerry Mahlman]	Accepted change made
2-431	A	8:44	8:44	Change to "Carbon dioxide (CO2) has the largest RF of any LLGHG" [Charles Miller]	Rejected ..used suggestion in comment 2-427
2-432	A	8:44	8:44	'on" should be "of" [Peter Siegmund]	Accepted change made

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-433	A	8:44	8:44	replace "on" by "of" (spelling error) [Peter Van Velthoven]	Accepted change made
2-434	A	8:44	9:23	If possible, provide uncertainty associated with data. It would also be useful to explain briefly why the trend is changing in each period. [G. H. Sabin GUENDEHOU]	Accepted
2-435	A	8:44	12:10	This entire section can be substantially shortened. In the end, only three numbers are really of importance here. The atmospheric CO2 concentration in 2004, in preindustrial times, and the RF resulting from that. [Nicolas Gruber]	Accepted ..the section has been shortened
2-436	A	8:44		Delete "LLGHGs" throughout and state which gases you mean in each case. It is a cause of confusion. How long is long? [Vincent Gray]	Rejected ..LLGHGs defined at start of chapter
2-437	A	8:44		replace "on" by "of" [Joanna Haigh]	Accepted change made
2-438	A	8:44		This section should begin with a discussion of long-term ice core CO2 records as does section 2.3.2 to maintain consistency and place the subsequent discussion of more recent changes into the long-term context. [Katharine Hayhoe]	Noted ....paleo chapter has a full discussion of ice core records
2-439	A	8:44		typo ?: Carbon dioxide (CO2) has the largest RF of any LLGHG (TAR). [Stephen E Schwartz]	Accepted change made
2-440	A	8:44		on to in [Junying Sun]	Accepted change made
2-441	A	8:48		275-285ppm [Junying Sun]	Accepted ..change made
2-442	A	8:49	8:51	This text, emphasizing the accelerating rate of CO2 accumulation in the atmosphere should be captured in the executive summary. [Charles Miller]	Noted
2-443	A	8:49	8:49	...the growth rate of CO2…" -> "...the absolute growth rate of CO2... [Xiaobin Xu]	Accepted ..change made
2-444	A	8:50	8:50	Perhaps you should say that by pre-industrial you mean pre-1750 otherwise the 'after more than 200 years' clause that comes later in the sentence has no reference value. [Greg Bodeker]	Accepted ..text changed
2-445	A	8:50	8:57	These are very impressive numbers. I was unaware of the level of acceleration of CO2 emissions that are highlighted here. [Jerry Mahlman]	Noted
2-446	A	8:53	9:3	These numbers need to be used with caution. The growth rates published by	Noted ..text added to reflect this

No.	Batch	Page:line From	Page:line To	Comment	Notes
				NOAA/CMDL have often reflected the growth rate in the mean surface concentration. Since sources are at the surface, surface means will have more short-term varibility (on time-scales comparable to, or shorter than, atmospheric mixing times) than atmospheric mean concentrations. (see for example, Enting, CSIRO Atmos Res. tech paper 40, available on-line) [Ian Enting]	
2-447	A	8:53	9:3	This quantitative comparison of CO2 concentrations @ TAR vs FAR should be included in the executive summary. [Charles Miller]	Noted
2-448	A	8:55	8:56	Sentence beginning 'The TAR did not report…' is awkward [Tami Bond]	Accepted ...rewritten
2-449	A	8:55	8:56	…observed in 1998. Add reference.to support this. [Cathy Clerbaux]	Accepted ..reference added
2-450	A	8:55	8:55	It is hardly noteworthy that TAR did not report something that occurred in 1998. [Andrew Lacis]	Accepted ..ref to TAR removed
2-451	A	8:55	8:56	It is striking that in 1998 both the global mean temperature and the CO2 increase were the largest on the record. This should be mentioned, and, even better, a possible link, or the absence of such a link, between these two observations should be given. [Peter Siegmund]	Noted ...but not in the brief of this chapter ..refer to chapter 3 and 7
2-452	A	8:55		There are probably lots of things that TAR did not report, reference to TAR not necessary here [Michael Coffey]	Accepted ..reference to TAR removed
2-453	A	8:55		Where does the 2.8/3 ppm/yr come from? In the TAR, atmospheric CO2 were reported as the mean of MLO+SPO. If this is simply because more stations were use, this should be said explicitely. [Corinne Le Quere]	Accepted
2-454	A	8:56	9:3	I think the highlight of years 2001-2003 do not belong here because there is a lot of interannual variability in CO2 growth rate, and these years are not particularly unusual if they are not accompanied by a discussion of the correlation of ElNino events. [Corinne Le Quere]	Accepted but waiting for 2005 Scripps and CMDL data
2-455	A	8:56	9:3	A comparison of CO2 growth rate for periods of 5 years would be relevant here, since we have a new period since the TAR. [Corinne Le Quere]	Accepted ...comparison added
2-456	A	8:57		5 year to 5-year [Junying Sun]	Accepted ..change made
2-457	A	9:1	9:24	It would be more effective to let the CO2 observational record speak for itself, rather than	Accepted ..text reduced

No.	Batch	Page:line From	Page:line To	Comment	Notes
				being so frequently compared to previous TAR results. [Andrew Lacis]	
2-2677	B	9:2	9:2	CO2 concentration AVERAGED in 2004 [Olivier Boucher]	Accepted...text changed
2-458	A	9:6	9:8	"...the last decade has the highest average growth rate." It is unclear (a) how the average is determined and (b) what is meant by the "last decade." Is this 1994-2004 or is it 1995-2005? [Patrick Hamill]	Accepted..decade 1994 to 2004
2-459	A	9:7	9:8	Add this to the executive summary as part of the increasing rate of CO2 accumulation statemetn suggested in Comment #8 above. [Charles Miller]	Accepted ..added to executive summary
2-460	A	9:10	9:13	Give reference for the emission data also here, although details follow later, see figure 2.3.1. [Ralf Koppmann]	Accepted ..reference added
2-461	A	9:11	9:15	The statements here seem to me to be among the most important results in the entire AR4, if I understand them correctly. They should be clarified and emphasized in the executive summary. I take these statements to mean that CO2 emissions are now rising faster than anticipated in even the highest emission scenario of the TAR and that emission rates are rapidly catching up to the highest emission scenario of the FAR. In other words, while the emission estimates of the FAR appeared to be exaggerations in relation to actual emissions in the 1990's, more recent data indicates otherwise. As I said, if this interpretation is correct, it is a stunning turn-around that deserves emphasis. But I am not at all clear about what is meant by the FAR BAU scenario. Is this the same as IS92a scenario? Where is this scenario shown in the AR4? If it is not shown in the AR4, I do not see how this result can possibly be meaningful to the reader. A graphic illustrating these statements (i.e. actual CO2 emissions in relation to various forecast scenarios from the FAR and the TAR) would be extremely useful and important. I do not find any such information in Chap 10 (climate projections) and do not know where else to look. [Theodore Anderson]	Noted. Dropped reference to scenarios and text reworded. Time frame properly cited
2-2678	B	9:12	9:12	I assume you mean the growth of the emission rate is higher, not just the emission rate itself. [Olivier Boucher]	Accepted clarified
2-462	A	9:13	9:15	FAR is another acronym that is introduced without definition in this chapter. [Patrick Hamill]	Accepted
2-463	A	9:13		Define FAR [Cathy Clerbaux]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-464	A	9:14	9:14	Three more commas are needed: "…from 1999 to 2004, however, exceeds the predictions of the FAR, and if continued, the…" [Xiaobin Xu]	Accepted ..commas added
2-2679	B	9:15	9:15	shouldn't it be "emission rate" rather than "growth rate"? [Olivier Boucher]	Accepted ..clarified
2-465	A	9:17	9:17	replace "when compared to" with either "in addition to" or "above", because the phrase "when compared to" suggests a dimensionless ratio rather than an augmentary component. [Keith Lassey]	Accepted ..text changed
2-466	A	9:17	9:21	Include this QUANTITATIVE information in the Executive Summary [Charles Miller]	Noted ...this data is in the executive summary
2-467	A	9:17	9:17	At this point in the chapter, it is not clear how this forcing value has been calculated [Keith Shine]	Accepted ..add pointer to derivation
2-468	A	9:17		Current levels of atmospheric CO2 contribute a RF of 1.63 ± 0.16 W m –2 when compared to preindustrial levels; a contribution that dominates that of all other forcing agents considered in this chapter. This is an increase of 12% since the value of 1.46 reported for 1998 in the TAR and is also much larger than the RF changes due to other agents. It should be made clear to what extent the increase of 12% (0.17 W m-2) is due to increase in CO2 mixing ratio and to what extent if any it is due to any changes in calculation of forcing per incremental CO2. [Stephen E Schwartz]	Accepted
2-469	A	9:18	9:18	I think saying that the CO2 contribution dominates that of all other forcing agents is too strong. [Greg Bodeker]	Noted
2-470	A	9:18	9:18	Dominates' is a relative term. It is better to compare with other magnitudes directly. [Tami Bond]	Noted ...this is done in the rest of this and the next sections
2-471	A	9:18	9:20	It is not clear why the RF of CO2 has changed by 12% since the TAR. Certainly the anthropogenic concentration has not changed that much. This should be explained. [Tami Bond]	Noted
2-472	A	9:18	9:13	With the Keeling definition (i.e. neglecting land-use) it is not true that the airborne fraction has to be less than 100%. It will exceed 100% whenever net land flux exceeds ocean uptake. (e.g. a year with a lot of clearing and fires with a low-uptake ENSO phase) [Ian Enting]	Airborne fraction discussion moved to chapter 7
2-473	A	9:18	9:20	See comment #1. [Dylan Millet]	Noted
2-474	A	9:19	9:19	Recommend replacing "… an increase of 12% since the value of 1.46…" with "an	Accept ..text changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				increase of 12% over the value of 1.46…". [Dylan Millet]	
2-475	A	9:19	9:19	Replace "… for 1998 in the TAR and is much larger…" with "…for 1998 in the TAR, much larger…". [Dylan Millet]	Accept ...text changed
2-476	A	9:20	9:20	Insert comma at end of line after the units watts per meter squared. [Patrick Hamill]	Comma added
2-477	A	9:21	9:21	Insert period after word "era" so it reads "...the industrial era. See also..." [Patrick Hamill]	Period added
2-478	A	9:23	9:24	Following the above, the correlation with fossil isn't that good, with early 20th century CO2 increase (from ice core data) reflecting land-use change (then the largest source) more than fossil. [Ian Enting]	Accepted ...discussion on landuse change added
2-479	A	9:24	9:24	It would be much more effective to include figures showing the measured changes in atmospheric CO2, CH4, N2O. For CO2, a 3-D plot showing the time-latitudinal CO2 trend would emphasize the fact that CO2 is being, and has been, accurately monitored world-wide. This would also illustrate the characteristic CO2 seasonal variability. [Andrew Lacis]	Accepted ...new figures added
2-480	A	9:25	9:25	It would be useful to explicitly state that the radiative forcings are referenced to 1750. I would also be helpful to describe the nature of the radiative forcing - e.g., instantaneous, adjusted, clear-sky, total-sky, etc. [Andrew Lacis]	Noted...text added
2-481	A	9:29	9:29	Replace 'for of' with 'for'. [Greg Bodeker]	Accepted
2-2680	B	9:29	9:29	for of ==> foe [Olivier Boucher]	As above
2-482	A	9:29	9:29	1. remove "for" [Graham Feingold]	As above
2-483	A	9:29	9:29	"for of" should be 'for' [Jón Egill Kristjánsson]	As above
2-484	A	9:29	9:29	...for the preindustrial ...(Delete the "of"). Shouldn't one note that El Nino can, and does, alter the CO2 time series on the interannual time scales? [Jerry Mahlman]	Noted
2-485	A	9:29	9:29	Change "for of the preindustrial" to "for the preindustrial". [Dylan Millet]	As above
2-486	A	9:29	9:29	omit "of" (syntax error)	As above

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Peter Van Velthoven]	
2-487	A	9:29		Delete: "of" [Michael Coffey]	As above
2-488	A	9:30	9:30	Note that the ice core GHG record for CO2 and CH4 now extends back 650,000 yrs [Fortunat Joos]	Noted and referred to
2-489	A	9:30	9:30	up to 740,000 years (considering the EPICA CO2 results, even if partial) [Robert KANDEL]	Noted and referred to
2-490	A	9:30		500,000, up to 740,000 years in EPICA (Nature 2004, Vol.429 P.623-628) [Junying Sun]	
2-491	A	9:31	9:31	Is "SIO" explained somewhere? [Rolf Philipona]	Accepted ...SIO defined
2-492	A	9:33	9:48	This paragraph does not give adequate recognition to the importance of C. D. Keeling's South Pole flask record for CO2, which actually is about 1 year longer than the Mauna Loa record. The emphasis in this paragraph on the shorter record at Baring Head seems out of place in this context. [Ray Weiss]	Accepted. Here we use the two longest continuous NDIR analyser recordes. In the NH this is Mauna Loa and in the SH it is Baring Head (longer than Cape Grim) and also started by CD Keeling in 1969
2-493	A	9:38	9:38	It should be mentioned here that ice core data show the rise since preindustrial: Neftel et al., Nature, 1984 [Fortunat Joos]	Noted
2-494	A	9:39	9:40	I think it better to acknowledge the WMO/GAW activity of calibration, data collection and quality aurance about CO2 concentration measurement (http://www.wmo.ch/web/arep/reports/gaw161_final_3jun.pdf). [Takashi Maki]	Noted check whether 13th meeting report available?
2-495	A	9:39	9:40	I think it better to show the current CO2 observational network (WMO/GAW) in figure (http://gaw.kishou.go.jp/wdcgg.html). [Takashi Maki]	Noted ...link to this added
2-496	A	9:39		Replace: "analyser" by "analysis" [Michael Coffey]	Accepted
2-497	A	9:43		Delete: "analyser" [Michael Coffey]	Accepted
2-498	A	9:47	9:47	Replace 'was be' with 'was'. [Greg Bodeker]	Accepted
2-499	A	9:47	9:48	CO2 was documented. (delete the "be") [Jerry Mahlman]	Accepted
2-2681	B	9:48	9:48	was be ==> was	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Olivier Boucher]	
2-500	A	9:48	9:48	remove "be" [Graham Feingold]	Accepted
2-501	A	9:48	9:48	Change "was be documented" to "was documented". [Dylan Millet]	Accepted
2-502	A	9:48	9:48	delete "be" [Rolf Philipona]	Accepted
2-503	A	9:48	9:48	omit "be" (syntax error) [Peter Van Velthoven]	Accepted
2-504	A	9:48	9:48	Delete the word "be". [Ray Weiss]	Accepted
2-505	A	9:48		Delete: "be" [Michael Coffey]	Accepted
2-506	A	9:48		be documented" delete: "be [Hartmut Grassl]	Accepted
2-507	A	9:52	9:55	These 2 sentences mis-state the logic. The RF of CO2 is a function of the excess CO2 remaining in the atmosphere. This in turn is a function of the cumulative emissions provided that most of the emitted CO2 that is redistributed leaves the atmosphere quickly (which it does), not over 100s of years as stated. I suggest restating the first sentence as: "After entering the atmosphere, CO2 exchanges rapidly with the short-lived terrestrial biosphere and surface ocean, and is redistributed on time scales of hundreds of years among all active carbon reservoirs including the long-lived biosphere and deep ocean." [Keith Lassey]	Accepted... in actual fact the lifetime of CO2 is complex because it is determined by many processes with different lifetimes. Inlcude suggested sentence but move the bulk of this to chapter 7
2-508	A	9:52	9:52	replace "distributed" by "redistributed" - this better describes the exchanges between the reservoirs [Peter Van Velthoven]	Accepted
2-509	A	9:52	10:2	This paragraph is weak and misleading. It is very difficult to difficult to summarize the complexities of the global carbon cycle in a few sentences without being misleading. The second sentence of this paragraph is certainly incorrect, and even lay-persons who read the newspapers know that deforestation and aforestation hae something to do with global warming. The third sentence of this paragraph is also misleading because the role of the deep ocean is much more important than sedimentation and erosion on time scales of hundreds to thousands of years. I suggest that both of these sentences be stricken, and that the discussion of the relationship beteen atmosheric CO2 and the carbon cycle be referred entirely to Chapter 7. [Ray Weiss]	Accepted ...the subject is too complex to be discussed fully here ....short comment only as above and bulk of discussion shifted to chapter 7

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-510	A	9:53	9:55	Radiative forcings are basically instantaneous in nature, and thus referenced to the prescribed amount of CO2 (or other GHG gas) that happens to be in the atmosphere at the time of the calculation. [Andrew Lacis]	Noted
2-511	A	9:53	9:55	The second sentence should avoid the word "never". Carbon does eventually leave these "active carbon" reservoirs through fossilisation, mineralisation, leaching, etc. [Keith Lassey]	Accepted
2-512	A	9:54	9:54	Never is too strong. Exchange with sediments occur on a time scale of 5000 years and some carbon gets buried. [Fortunat Joos]	Accepted
2-513	A	9:54		not "never" just very slowly [Joanna Haigh]	Accepted
2-2682	B	9:55	9:55	…on timescales shorter than thousand years… or change never to hardly. [Olivier Boucher]	Accepted
2-514	A	9:56		Does not erosion tend to add CO2 to the ocean reservoir, rather than to remove it [Michael Coffey]	Accepted ...text changed
2-515	A	10:13		Add at end "There are relatively fewlong-term measurements of CO2 over industrial areas making it difficult to characterise RF over these areas" [Vincent Gray]	Noted
2-2683	B	10:15	10:20	You assume here that there hasn't really been yet any climate-carbon feedback, which is probably true. [Olivier Boucher]	Noted
2-516	A	10:15	10:18	This sentence weakens the following text because it is not quantitative. Read on its own, one gets the impression that the statement is not based on scientific evidence. I would suggest to remove it. [Corinne Le Quere]	Rejected ...sentence leads into following qunatitative text
2-517	A	10:15	10:16	The end of the sentence should be (in line 16) "emissions from combustion of fossil fuels and those from cement production." [Ilkka Savolainen]	Accepted ..order changed
2-518	A	10:16	10:16	"...emissions from the combustion of cement production..." Syntax error. Replace with, "...combustion emissions generated by cement production..." or skip the word "combustion" entirely. [Patrick Hamill]	Accepted ..order changed
2-519	A	10:16	10:16	reorder emissions. Fossil fuel is dominant. [Fortunat Joos]	Accepted ...order changed
2-520	A	10:16	10:16	from cement production, gas flaring, and, most importantly, the combustion of fossil	Accepted ..order changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				fuels. [Robert KANDEL]	
2-521	A	10:16	10:16	Avoid the non-sensible phrase "combustion of cement production". It should be sufficient to say "combustion of fossil fuels and cement production", which also states the more important first. (Gas-flaring is just another form of combustion of a fossil fuel). [Keith Lassey]	Accepted ..order changed
2-522	A	10:16	10:16	Change to "...from the combustion of fossil fuels, gas flaring, and cement production." [Charles Miller]	Accepted ..order changed
2-523	A	10:16	10:16	Recommend deleting "the combustion of". [Dylan Millet]	Accepted ..order changed
2-524	A	10:16	10:16	replace "from .. fuels" by "from cement production, gas flaring and combustion of fossil fuels" - cement is not being burnt ! [Peter Van Velthoven]	Accepted ..order changed
2-525	A	10:16		correct word position to: "fossil fuels, cement production, and gas flaring" [Hartmut Grassl]	Accepted ..order changed
2-526	A	10:17	10:17	please quote the paper by Van der Werf et al. (2003) about biomass burning [Philippe Bousquet]	Accepted
2-527	A	10:18	10:18	suggest to delete 'ocean warming' For a discussion of the impact of global warming see Joos et al., Science 1999; Plattner et al., Tellus 2001 or others. Less CO2 uptake due to ocean warming is not considered as an emission, but rather as a feedback. [Fortunat Joos]	Accepted
2-528	A	10:20		Gt C= Gigatonnes of…is defined here but used earlier e.g p2.9 line 12 [Cathy Clerbaux]	Noted Gt C changed to Pg C throughout text
2-529	A	10:22		I suggest moving Figure 2.3.2 to line 56 because the first reference to it is on the following page. [Derek Cunnold]	Accepted
2-530	A	10:24	10:40	I found this paragraph to be a little confusing. For a start I think it would be helpful to state why the 13C/12C isotopic ratio in CO2 emitted from coal, gas and oil combustion and land clearing is less than in atmospheric CO2. [Greg Bodeker]	Noted ....but this would require a lot of description and there is not enough space for this
2-531	A	10:24	10:55	The paragraphs on 13C and O2 need some basic statement of changes since the TAR, such as "Since the TAR, atmospheric O2 and 13C continued to decrease at a rate that is consistent with the emission of CO2 from fossil origin." [Corinne Le Quere]	Noted sentence added
2-532	A	10:27	10:28	Please change the sentence as "… occurring isotopes denoted as 12C, 13C and 14C." [Ramachandran Srikanthan]	Accepted ...reference to 14C removed and order changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-533	A	10:28	10:30	The comments on 14C appear out of place. I suggest either to say more on observed 14C changes, or to only focus this paragraph on 13C. [Corinne Le Quere]	Accepted ...reference to 14C removed
2-534	A	10:32	10:34	Could cite the following reference which constructs the delta13C history in the CO2 emission from fossil fuels and cement manufacture: [Andres, R.J., Marland, G., Boden, T., Bischof, S., 2000. Carbon dioxide emissions from fossil fuel consumption and cement manufacture, 1751–1991, and an estimate of their isotopic composition and latitudinal distribution. In: Wigley and Schimel (Ed.), The Carbon Cycle. Cambridge University Press, Cambridge, UK, pp. 53-62.]. [Keith Lassey]	Accepted ...paper cited
2-535	A	10:33	10:33	Recommend changing "and is a function" to "as a function". [Dylan Millet]	Accepted
2-536	A	10:34	10:34	Its not clear to me what you mean by 'mix of fossil fuels'. [Greg Bodeker]	Accepted ...definition clarified
2-537	A	10:38	10:40	The comparison of the fossil fuel emissions (a rate) with the increase in 13C (a concentration ratio) is slightly misleading I think. I would expect that the change in 13C should correlate with fossil fuel emissions, not 13C itself. [Corinne Le Quere]	Accepted ...reworded
2-538	A	10:39	10:40	I object to "showing strong correlation". I suggest to replace this by "increase in line with" and to omit "increasing" in line 40. If correlation coefficients would be calculated between these data they would probably be large as all time series are increasing. I do not believe there will still be a strong correlation if the trend is removed from the data, as the local extremes of the time in figure 2.3.1-bottom panel do not coincide. Using the term "strong correlation" suggests that the time series agree in detail. Any 2 increasing time series will be strongly correlated.I consider the present formulation too suggestive. [Peter Van Velthoven]	Accepted and reworded as suggested
2-539	A	10:47	10:47	The sentence should read as "… of the difficulty in resolving…" [Ramachandran Srikanthan]	Accepted
2-540	A	10:53	10:53	replace "here" by "in figure 2.3.1" - this makes the text easier to read. [Peter Van Velthoven]	Accepted
2-541	A	11:1	11:7	These sentences seem to repeat the information that was presented from line 53 of page 8 to line 3 of page 9. [Greg Bodeker]	Accepted ..repetition removed
2-2684	B	11:1	11:7	Some repetition with above. [Olivier Boucher]	Accepted ..repetition removed
2-542	A	11:1	11:7	I found somewhat difficult the logical sequence of the discussion. The CO2 growth rate	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				is discussed here and in pages 8, lines 53-57 + page 9, lines 1-3. I would concentrate this discussion in a single place within the section. [Alcide di Sarra]	
2-543	A	11:1	11:7	large overlap of text with paragraph starting on page 8 line 1 [Corinne Le Quere]	Accepted ..repetition removed
2-544	A	11:4		See comment #8 *…observed in 1998. Add reference.to support this. [Cathy Clerbaux]	Accepted reference added
2-545	A	11:6		Rotate Figure 2.3.2 so that year is the horizontal axis [Michael Coffey]	Accepted figure removed
2-546	A	11:9	11:29	This paragraph is questionable. Please note that land use emissions contribute significantly to the total emission (1 to 2 GtC/yr) from land use compared to 7 GtC/yr from fossil sources. The last sentence of the paragraph is not needed. [Fortunat Joos]	Accepted ..all reference to airborne fraction moved to chapter 7
2-547	A	11:9	11:29	The discussion and quantification of "airborne fraction" needs to be harmonised with that in Chapter 7 (page 7-17, line 56 to page 7-18 line 7), where a mean value for 1959-2000 of 0.55 is favoured (see also Figure 7.3.1, page 7-123), compared to 0.57 in this chapter for 1970-2004 of 0.57 (Figure 2.3.1(a), page 115). Both chapters cite Keeling and Whorf in CDIAC as their sources, though oddly Chapter 2 cites "Keeling and Whorf (2005)" whereas Chapter 7 cites Keeling and Whorf (2004)!! Perhaps Chapter 2 authors have acessed CDIAC data more recently than Chapter 7 authors? Harmonisation is definitely needed! These points are also raised for Chapter 7. [Keith Lassey]	Accepted ..all reference to airborne fraction moved to chapter 7
2-548	A	11:9	11:21	Here "airborne fraction" is introduced and it is stated that this can be used to remove the strong short-term variations in figure 2.3.2. I would greatly favour inclusion of a curve describing the airborne fraction in figure 2.3.2 to illustrate this. [Peter Van Velthoven]	Accepted ..all reference to airborne fraction moved to chapter 7
2-2685	B	11:11	11:11	Is this consistent with the definition used in chapter 7? Does this mean that CO2 from deforestation is not accounted for in the denominator? [Olivier Boucher]	Accepted ..all reference to airborne fraction moved to chapter 7
2-549	A	11:11	13:11	The airborne fraction can exceed 100% if the land emits CO2, as was the case in March 1998. I suggest to start right away with the quantitative statement of the following sentence ("The airborne fraction was between 30 and 80% ...). [Corinne Le Quere]	Accepted ..all reference to airborne fraction moved to chapter 7
2-550	A	11:17	11:19	"Thus long-term trends in the atmospheric CO2 growth rate over decades and longer, reflect the CO2 emission rates from fossil fuel burning whereas shorter term variations are due to fluctuations in other sources and sinks of CO2." This sentence is a little	Accepted ...sentence reworded to improve clarity

No.	Batch	Page:line From	Page:line To	Comment	Notes
				misleading. In my opinion, shorter term variations of atmospheric CO2 can also be due to fluctuations in emissions from fossil fuel burning. [Xiaobin Xu]	
2-551	A	11:20	11:20	ocean warming": There are more processes than just ocean warming. A reduction in the anthropogenic CO2 uptake for example, due to an increase in stratification will lead to a larger air-borne fraction. I therefore suggest to replace this with "changes in the ocean carbon cycle [Nicolas Gruber]	Accepted text changed as suggested
2-552	A	11:20	11:20	add references to LeQuere et al. (2003), McKinley et al. (2004), Feely et al. (1999), Takahashi et al. 2003) etc. [Nicolas Gruber]	Accepted 3 references added
2-553	A	11:20	11:20	Remove "and ocean warming". Short term variations in the ocean are primarily due to changes in physical transport. It is enough to have "ocean sinks" at the end of this sentence. [Corinne Le Quere]	Accepted text removed
2-554	A	11:23	11:23	Where does this value of 57% come from? [Greg Bodeker]	Accepted ...airborne fraction discussion shifted to chapter 7
2-555	A	11:23	11:23	As in the comment on chapter 2-10-39 I object to the phrase "excellent correlation". Any 2 increasing time series show positive correlation. The word "excellent" adds a subjective judgement about the correlation. Again, if the trend would be removed from the black and the red/blue lines in figure 2.3.1 not much correlation will remain. The emissions do not seem to show the same interannual variability as the concentrations once the seasonal cycle and long-term trend are removed. I would therefore use a weaker formulation than "excellent correltion". All one can say is that emissions and concentrations rise in parallel. [Peter Van Velthoven]	Accepted ...airborne fraction discussion shifted to chapter 7
2-556	A	11:24	11:25	I find it misleading to mention the 2002 emissions as a record high, when in fact emissions are simply increasing and we are expecting a record high nearly every year. [Corinne Le Quere]	Accepted ..wording changed
2-557	A	11:25	11:25	Recommend changing "6.975 Gt C up 2% on the …" to "6.975 Gt C, up 2% from the …" [Dylan Millet]	Accepted ..text changed
2-558	A	11:27	11:29	I do not see what this statement brings. The statements about the airborne fraction is already made clearly in the same paragraph. [Corinne Le Quere]	Accepted ...airborne fraction discussion shifted to chapter 7
2-559	A	11:27	11:29	This forecast of future atmospheric CO2 accumulation, based on a larger fossil fule budget, should be added to the executive summary. [Charles Miller]	Accepted ...executive summary changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2686	B	11:29	11:29	for 2004? What is meant by "future"? Will the airborne fraction remain the same in the future? [Olivier Boucher]	Accepted ...airborne fraction discussion shifted to chapter 7
2-2687	B	11:31	11:40	Is it a mean over all stations (excluding some of them)? If so should it be called a global mean? A global mean would involve some sort of correction to account for inadequate sampling. [Olivier Boucher]	Noted ...what would a "true" global network be? We use the most comprehensive networks available ie NOAA/CMDL and SIO. Text states limiations
2-560	A	11:31	11:40	I would suggest to move this paragraph at the beginning of section 2.3.1 (after the first paragraph) and include comments on how atmospheric CO2 was reported in the TAR (2 stations only). I feel though that this text must be accompanied by a statement of the kind "the atmospheric CO2 is very well known because of the relatively good coverage of measurement stations, and because of the fast mixing rate of the atmosphere (1-12 months) compared to the life time of CO2 (decades to centuries)." [Corinne Le Quere]	Accepted ...part of material moved as suggested
2-561	A	11:31	11:31	SIO?? Has this been defined? [Joyce Penner]	Noted SIO has been defined
2-562	A	11:31		Did you tell the reader what SIO is? (Sorry if I missed it) [Joanna Haigh]	Accepted and defined
2-563	A	11:32	11:32	Replace 'uncertainties statistically' with 'uncertainties are statistically'. [Greg Bodeker]	Accepted
2-564	A	11:32	11:32	Insert "were" (The uncertainties "were" statistically derived ...) [Ralf Koppmann]	Accepted
2-565	A	11:32	11:32	The verb is absent. The probable intent is: "The uncertainties are statistically derived in different ways for each network", but this is essentially a truism, as uncertainties should always be statistically derived. Try instead the following sentence: "The statistical derivations of uncertainties are different for each network". [Keith Lassey]	Accepted and text altered
2-566	A	11:32	11:32	Change "The uncertainties statistically" to "The uncertainties were statistically" [Brian Magi]	Accepted and text altered
2-567	A	11:32	11:32	Insert "are" between "uncertainties" and "derived". [Dylan Millet]	Accepted and text altered
2-568	A	11:32	11:32	The uncertainties statistically derived…" -> "The uncertainties were statistically derived… [Xiaobin Xu]	Accepted and text altered
2-569	A	11:32		The uncertainties statistically" add: "are statistically	Accepted and text altered

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Hartmut Grassl]	
2-570	A	11:36	11:37	"Also the error for the SIO measurements of the annual average for Mauna Loa in 2004 was only +/- 0.05 ppm". It is not clear why this statement is relevant. Recommend deleting. [Dylan Millet]	Accepted and deleted
2-571	A	11:37	11:38	To get a more real picture of atmospheric CO2, data from marine boundary layer and continental sites and from different altitude are needed. Why only low altitude MBL sites are used? [Xiaobin Xu]	Accepted...there are very few long term high altitude stations ...we use the most extensive low altitude sites (not just MBL) to derive the means for use n trend analyses
2-572	A	11:42	11:42	What does "updated" mean in the reference "(Etheridge et al., 1996 updated)"? The basis for the updating should be explained and attributed. If this "updated" reference is repeated (though I haven't found a repeat), this basis could be described in a footnote or following the reference citation. [Keith Lassey]	Accepted and reference added
2-573	A	11:44	11:44	Please synchronize ice core data references in text and figure with chapter 6. There are other records available, e.g. Siegenthaler et al, Tellus, 2005, Monnin, GBC, 2004 ... [Fortunat Joos]	Accepted and refer to chapter 6
2-574	A	11:45		Are IPCC Authors happy to use the phrase "Little Ice Age"? I thought the idea that this was a global phenomenon was now being played down (Jones and Mann??) [Joanna Haigh]	Noted ...referred to chapter 6
2-575	A	11:46	11:57	The section covers CO2 concentration only. However, the text given from L.46 to 57 refers also to CH4 and N2O pre-industrial concentrations. I wonder if this part of the text could be shifted further in the text (before section 2.3.4 for instance). [Philippe Tulkens]	Noted
2-576	A	11:48	11:50	Why take 1860? Surely it is many decades after the commencement of significant anthropogenic fossil-fuel CO2 emissions. It should be made clear whether or not AR4 is recommending that 1860 be taken as an "alternative start date for the RF calculations" (I hope it is not). The error incurred by using 1860 would surely exceed the error incurred if 1750 were taken (but would be of opposite sign). [Keith Lassey]	Noted ...this sentence added for comparison only ...meaning not altered
2-577	A	11:49	11:50	Why is figure 2.3.2 referenced here? It does not refer to the use of 1860 as a start date. [Patrick Hamill]	Accepted ...ref to figure 2.3.3 removed
2-578	A	11:50	11:50	The "Australian Antarctic Territory" is not recognized by the Antarctic Treaty (which Australia has signed). I suggest that Law Dome be described as being in Antarctica or East Antarctica.	Accepted ...text changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Ray Weiss]	
2-579	A	11:50		Australian Antarctic Territory" delete: "Australian" and "Territory [Hartmut Grassl]	Accepted ...text changed
2-580	A	11:50		delete "in the Australian Antarctic Territory" [Junying Sun]	Accepted ...text changed
2-581	A	11:51	11:51	Please note that the age distribution of the bubbles in the ice has a typical width of about 20 years (Etheridge et al, JGR, 1997). Thus, the 5 yr figure given in parentheses is not appropriate. [Fortunat Joos]	Noted
2-582	A	11:56	11:56	Figure 2.3.3. The section covers CO2 concentration only. However, the text given from L.46 to 57 refers also to CH4 and N2O pre-industrial concentrations. I wonder if this figure could be shifted further in the text (before section 2.3.4 for instance). [Philippe Tulkens]	Noted ...figure position changed
2-583	A	12:1	12:1	Replace 'formula' with 'formulae'. [Greg Bodeker]	Accepted ...text changed
2-584	A	12:1	12:2	"The simple formula ...quoted in Ramaswamy...are still valid" Should read either "The simple formula...is still valid" or "The simple formulae...are still valid." Also I would suggest including the formula (or formulas). [Patrick Hamill]	Accepted...text changed
2-585	A	12:1	12:10	The radiative forcing results cited in Chapter 10 (Section 10.2.1) should be moved to Chaper 2. They don't really fit well in Chapter 10, and are far more relevant to the radiative forcing discussion here in Chapter 2. [Andrew Lacis]	Noted ... Discussion will stay in Chapter 10 as climate modelling community need to understand limitations of their radiation schemes
2-586	A	12:1		Spell out " long-lived greenhouse gases" [Vincent Gray]	Noted and defined earlier in the text
2-587	A	12:2	12:10	To a non-specialist, these comparisons are hard to follow. Specifically what is a "line-by-line radiation scheme" or "line-by-line model"? Explain "single atmospheric background profile". [Keith Lassey]	Noted text reworded
2-588	A	12:2	12:4	A reference to the intercomparison of line-by-line RF GCMs should be provided [Charles Miller]	Noted see above
2-589	A	12:2	12:4	An description of "surface forcing agreed very well (better than 10%)" is confusing. [Yukitomo Tsutsumi]	Noted see above
2-590	A	12:2	12:2	replace "are" by "is" (grammatical error)	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Peter Van Velthoven]	
2-591	A	12:5	12:6	The GCM radiation schemes were less accurate, with ~20% errors in the CO2 RF and their surface forcing calculations were unreliable - carry forward to summary [Stephen McIntyre]	Noted
2-592	A	12:9	12:10	This sentence sort of implies that the simple formula doesn't include the effects of clouds and stratospheric adjustment. [Keith Shine]	Noteds ..sentence deleted
2-593	A	12:10	12:10	replace "cloud" "clouds" [Peter Van Velthoven]	Noted
2-594	A	12:12	14:42	Section 2.3.2 lacks discussion of: Atmospheric lifetime of CH4 Major loss mechanisms of CH4 Comparison of line-by-line and GCM analyses of RF [Charles Miller]	Accepted ..methane lifetime and dependence on its own sink etc is discussed in chapter 7
2-595	A	12:12		The discussion on atmospheric CH4 is mainly based on flask and insitu measurements from NOAA/CMDL and AGAGE. These are certainly the most important global monitoring networks today (for CH4). Nevertheless it should be mentioned that further measurement programs exist (of which many are reported to the GAW database [WMO, 2003]. Furthermore, much progress has been made with complementary measurements such as measurements from tall towers [Vermeulen et al., 2003], aircraft, and satellites [Frankenberg, 2005]. References: WMO: Global Atmospheric Watch World Data Centre for Greenhouse Gases, 92, Japan Meteorological Agency in co-operation with World Meteorological Organisation, 2003. Frankenberg, C., Meirink, J.-F., van Weele, M., Platt, U., and Wagner, T.: Assessing Methane Emissions from Global Space-Borne Observations, Science, 308, 1010–1014, 2005. Vermeulen AT, Hensen A, Bulk WCM van den & Erisman JW, 2003. Emission Verification of Greenhouse Gases on the Sub-continental Scale Using Tall Tower Observations and Inverse Trajectory Modeling. Eos Trans. AGU, 84(46), Fall Meet. Suppl., Abstract A51B-04. [Peter Bergamaschi]	Accepted....extra references added here on this point and in CO2 section Frankenberg reference to satellite data also added
2-596	A	12:12		Although it is clear that the discussion of this chapter should focus mainly on the global distribution of CH4 it should be mentioned that some progress has been made to analyze regional CH4 signals (and relate them to regional emissions). This seems to be very important in the context of potential verification of Kyoto targets [Bergamaschi et al., 2005; Manning et al., 2003], but also for verification of bottom-up inventories of natural	Accepted ...text added to reflect this but most discussion left in Chapter 7

No.	Batch	Page:line From	Page:line To	Comment	Notes
				sources on regional scales. References: Bergamaschi, P., M. Krol, F. Dentener, A. Vermeulen, F. Meinhardt, R. Graul, M. Ramonet, W. Peters, and E. J. Dlugokencky, Inverse modelling of national and European CH4 emissions using the atmospheric zoom model TM5, Atmos. Chem. Phys., 5, 2431–2460, 2005. Manning, A. J., Ryall, D. B., Derwent, R. G., Simmonds, P. G., and O'Doherty, S.: Estimating European emissions of ozone-depleting and greenhouse gases using observations and a modeling back-attribution technique, J. Geophys. Res., 108, (D14), 4405, doi:10.1029/2002JD002312, 2003. [Peter Bergamaschi]	
2-597	A	12:12		Section 2.3.2. Section contains an interesting overview of recent developments, but needs to be restructured. Its organization presently rambles a little. The question of whether decreasing sources or increasing sinks causes changes in methane growth rate is central to the section. This could be clearly laid out in an early discussion, then evidence for decreasing sources summarized, followed by evidence for increasing sinks. [Tami Bond]	Noted ...text reorganised ...however as mentioned in this and the next section there is no evidence for decreasing OH. Cite very recent Keppler et al article as possible cause of decreasing growth rate
2-2688	B	12:12		The same information can be found in different places of this section. It would be nice if it could read more linearly. This section could refer to chapter 7 for climate feedbacks involving methane. [Olivier Boucher]	Accepted ...reference made to chapter 7
2-598	A	12:12		Section 2.3.2. Are the methane emissions associated with permafrost melting negligible ? I found no reference to these emissions in this section and I wonder if the amount is significant. [Philippe Tulkens]	Noted ...presently considered to be a small source
2-599	A	12:12		Section 2.3.2 is rather disorganized. Related ideas and papers should be grouped together, and jumping ahead to a topic that's discussed again later in the section should be avoided. E.g. the discussion of the Simpson et al. and Dlugokencky et al. papers (p. 13, lines 17-20) could be grouped with the discussion of future CH4 emissions later in the section. [James S. Wang]	Noted ...text reorganised
2-600	A	12:14	12:19	If possible, provide uncertainty associated with data. [G. H. Sabin GUENDEHOU]	Noted ...introductory sentence only
2-601	A	12:14	12:14	In the context of accumulated GHG forcing since 1750, CH4 is the second largest contributor. [Andrew Lacis]	Noted...first sentence reworded
2-602	A	12:14	12:16	A more recent paper than Petit et al (1999) is available that goes back further in time (450,000 years) and has improved resolution: Delmotte, M., Chappellaz, J., Brook, E.,	Noted ...reference added

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Yiou, P., Barnola, J.M., Goujon, C., Raynaud, D., Lipenkov, V.I., 2004. Atmospheric methane during the last four glacial-interglacial cycles: Rapid changes and their link with Antarctic temperature. J. Geophys. Res., 109: D12104, doi:10.1029/2003JD004417. "Over the last half million years" should be changed to "Over the last 450,000 years". [Keith Lassey]	
2-603	A	12:14	14:41	Section 2.3.2 correctly focuses on trying to explain the reasons for the slowdown in the growth of methane concentrations. However, any such explanation must make use of correct emission inventory data. Frankenberg, et al (2005) (Frankenberg, C., J.F. Meirinkim, M. van Weele, U. Platt and T. Wagner, 2005: Asessing methane emissions from global space-borne observations. Science, 308, 1010-1014) indicate that current emission inventories considerably underestimate methane emissions from tropical rainforests. This finding needs to be included in the section's assessment. It indicates that understanding of the atmosphric behavior of methane is even poorer than previously believed. [Lenny Bernstein]	Accepted text changed to reflect this and new references added on vegetative emissions of methane
2-604	A	12:14	14:41	Frankenberg, et al (2005) (Frankenberg, C., J.F. Meirinkim, M. van Weele, U. Platt and T. Wagner, 2005: Asessing methane emissions from global space-borne observations. Science, 308, 1010-1014) indicate that current emission inventories considerably underestimate methane emissions from tropical rainforests -- this should be reflected in the text. [Howard Feldman]	Accepted text changed to reflect this and new references added on vegetative emissions of methane
2-605	A	12:14	14:41	The focus of Section 2.3.2 is on explaining the reasons for the slowdown in the growth of methane concentrations. It assumes that methane inventories are well understood. However, at least one recent study raises questions about that assumption. Frankenberg, C., J.F. Meirinkim, M. van Weele, U. Platt and T. Wagner, 2005: Assessing methane emissions from global space-borne observations. Science, 308, 1010-1014, presents satellite measurements that show that current emission inventories considerably underestimate methane emissions from tropical rainforests. This reference should be included in this section's assessments of methane concentrations. [Jeffrey Kueter]	Accepted text changed to reflect this and new references added on vegetative emissions of methane
2-606	A	12:14	14:40	It would be helpful if the methane section could briefly indicate how the accounting for methane forcing and tropospheric ozone forcing used here differ from that suggested in Shindell's recent paper, perhaps referring forward to the section on tropopheric ozone. Shindell, D.T., G. Faluvegi, N. Bell, and G. Schmidt, 2005: An emissions-based view of climate forcing by methane and tropospheric ozone. Geophysics Research Letters, 32, doi:10.1029/2004GL021900.2 [Susan Solomon]	Accepted text changed to include this + ref to accounting in ozone section

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-607	A	12:14		Spell out " long-lived greenhouse gases" [Vincent Gray]	Noted ..defined in executive summary
2-608	A	12:19	12:19	"[Etheridge et al., 1998 updated]" It is not clear, what exactly has been updated [Peter Bergamaschi]	Accepted ...word "updated" removed
2-609	A	12:19	12:19	What does "updated" mean in the reference "(Etheridge et al., 1998 updated)"? The basis for the updating should be explained and attributed. Since this "updated" reference is repeated in several places, this basis could be described in a footnote or following the reference citation. The "update" is not simply due to the new NOAA04 calibration scale: see my comment referencing p. 14, lines 19-21. [Keith Lassey]	Accepted as above
2-610	A	12:19		It would be nice to have a composite figure of ice core CH4 concentrations with a blow-up of the last 250 years here. [Katharine Hayhoe]	Noted ...will try but space problems may preclude this
2-611	A	12:21	12:34	Despite some short explanations about the uncertainty of the global average CH4 abundance the exact calculation remains unclear. In particular striking is the very low uncertainty from the NOAA/CMDL network ("+- 0.60 ppb") compared to the AGAGE network ("+- 44.8 ppb"). Is that mainly a statistical effect related to the much higher number of sites from NOAA/CMDL in the marine boundary layer ? [Peter Bergamaschi]	Noted ...text added to clarify the difference
2-2689	B	12:21	12:21	same remark as above regarding the global-mean value. [Olivier Boucher]	Noted
2-612	A	12:21	12:34	It is good that CMDL has corrected their calibration scale for CH4 by about 1%. What is obscured in this paragraph is that the Japanese Tohoku or Nippon Sanso calibration scale was the first to show the atmospheric CH4 values that the CMDL scale now confirms. In fact, the two scales are now in excellent agreement, as is reflected by the agreement between CMDL and AGAGE, which uses the Japanese scale, that is cited in this paragraph. This paragraph should give credit where it is due. It should also omit the sentence "This scale has been accepted by WMO and...", which is purely subjective and has nothing to do with the science or the correctness of the CH4 measurements. [Ray Weiss]	Accepted ..text altered to reflect this comment
2-613	A	12:22	12:30	The stated uncertainty in the global mean methane from the five AGAGE sites of 44.8 ppb is in stark contrast to the global mean from the 40 NOAA/CMDL sampling locations of 0.60 ppb. The accuracy and precision of the AGAGE measurements is at least equal to those of the NOAA measurements. It is therefore obvious that the quoted uncertainties in the means from the two networks are being computed by entirely different techniques. This should be rectified. One possible way to do this would be to compare how well the means from the NOAA/CMDL measurements at or close to the locations of the 5	Accepted ...text added to clarify the difference

No.	Batch	Page:line From	Page:line To	Comment	Notes
				AGAGE sites represent the global means determined from the 40 NOAA/CMDL locations and to then combine this difference with the imprecision from 5 (as opposed to 40) sets of measurements. It should also be noted in the text that the new NOAA calibration scale is almost identical to the calibration scale that AGAGE has been using. [Derek Cunnold]	
2-614	A	12:22	12:22	replace "1777.60" by "177.6" and "0.60" by "0.6" - the last digit contains no information. [Peter Van Velthoven]	Accepted
2-615	A	12:23	12:24	the calibration scale used by it" should be "its calibration scale [Tami Bond]	Accepted
2-616	A	12:24	2:25	Instead of saying that "the new scale … increases all previously reported methane concentrations … by about 1%", why not cite the increase to all significant figures: "… previously reported methane mixing ratios … by 1.24±0.07%". Note also that "concentrations" should give way to "mixing ratios" or "molar ratios". [Keith Lassey]	Accepted ...new text added
2-617	A	12:24	12:30	NOAA04 vs. AGAGE CH4 scale: It should be specified, how these two scales relate to each other [Peter Bergamaschi]	Accepted ...text added
2-618	A	12:25	12:26	"The systematic error is estimated to be ~2ppb (90%)": Do the authors mean that this new scale is wrong by ~2ppb or is this an estimate of the maximum potential systematic error ? [Peter Bergamaschi]	Noted ref Ed Dlugokencky..text clarifed
2-619	A	12:26	12:26	explain what "90 %" refers to. Is it a confidence interval? [Peter Van Velthoven]	Noted ref Ed DlugokenckyNoted text clarified
2-620	A	12:26	12:26	What does "90%" represent? Confidence? [Xiaobin Xu]	Noted ref Ed DlugokenckyNoted text clarified
2-621	A	12:27	12:27	Following the sentence "… Global Atmospheric watch Programme as a "common reference", add another sentence "All methane mixing ratios reported in this report are adjusted to the NOAA04 calibration scale." [This implies that mixing ratios reported in Chapter 7 conform too: I hope that they do -- I have made this point there]. [Keith Lassey]	Accepted
2-622	A	12:27		correct "Global Atmospheric" to "Global Atmosphere" [Hartmut Grassl]	Accepted
2-623	A	12:27		Atmospheric to "Atmosphere" [Junying Sun]	Accepted
2-624	A	12:28		AGAGE has only had 5 sites which have been used at any one time for long term ground based measurements. The Oregon and Adrigole, Ireland sites were eventually replaced by sites at Trinidad Head, California and Mace Head, Ireland.	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Derek Cunnold]	
2-625	A	12:31	12:31	Clarify whether 90% is confidence interval. If yes, reformulate the sentence. It would be good to replace "technique" with "simulation" or "analysis". [G. H. Sabin GUENDEHOU]	Noted
2-626	A	12:31	12:32	An expression of "90% uncertainty" is confusing. [Yukitomo Tsutsumi]	Noted text clarifiedref Ed Dlugokencky
2-627	A	12:33	12:34	A whole sentence is highlighted! [NADIA GAMBOA]	Noted Noted ref Ed Dlugokencky text no longer highlighted
2-628	A	12:36	12:41	Paragraph is too wordy. Graph is given and its details do not need to be enumerated. Paragraph would be stronger if it simply emphasized the most interesting feature of the graph. [Tami Bond]	Accepted ...text reduced
2-629	A	12:36	12:41	Why does this paragraph not also mention the significant reduction in methane values at the end of 2004, which is implied by Fig. 2.3.4? Methane values were apparently anomalously high at the end of 2003. [Derek Cunnold]	Accepted
2-630	A	12:36	13:12	There seems to be some redundancy in these 4 paragraphs: page12, line 36-41 is discussing the annual growth rate and its slowdown, followed by a discussion about potential reasons (page12, line 43-48). The next paragraph (page 12, line 50 - page 13, line 3) then again discusses the slowdown of the annual growth rate, and again followed by another paragraph with discussion of potential reasons (page 13, line 5-12). [Peter Bergamaschi]	Accepted text flow improved
2-2690	B	12:36	13:12	Some repetition. [Olivier Boucher]	Accepted text flow improved
2-631	A	12:39		5 year to 5-year [Junying Sun]	Accepted
2-632	A	12:40		"negative growth rates" (?) [Hartmut Grassl]	Noted
2-633	A	12:40		The first part of the sentence talks of growth rates - but presumably the mean referred to in the latter part of the sentence is for the mixing ratio (not growth rate) - if so the wording should be clarified. [Martin Manning]	Accepted ...text clarified
2-634	A	12:43	12:47	I would move this paragraph at page 13, line 13. In this way the discussion is not coming back to the same topic twice. [Alcide di Sarra]	Accepted text flow improved
2-635	A	12:43		This paragraph and the two following it are poorly organized, with the paragraph starting	Accepted text flow improved

No.	Batch	Page:line From	Page:line To	Comment	Notes
				on line 5 of the next page seeming to repeat the subject of this one but with a contradictory statement. These three paragraphs should be combined into two shorter ones. These three paragraphs should be combined into two shorter ones. [Steven Sherwood]	
2-636	A	12:44	12:44	The first reference to OH should include its chemical name, hydroxyl radical. (Presently it is defined on page 13, line 11). [Lourdes Maurice]	Accepted
2-637	A	12:45	12:45	a "(" is missing before "using two different tracers for OH" [Philippe Tulkens]	Accepted
2-638	A	12:45	12:48	The sign and magnitude of the trend in OH over recent decades have been debated, and there is currently no consensus. Bottom-up and top-down approaches have produced conflicting results (see e.g. Ch.2, p.13, lines 22-28). Since it's far from clear which view is more defensible, I'd suggest giving both views equal weight. This also implies that the causes of the recent slowdown in CH4 growth are not known with certainty. [James S. Wang]	Rejected ...see section 2.3.5 OH discussion and figure
2-639	A	12:46	12:46	Remove superfluous ")" after "OH". [Keith Lassey]	Accepted ..done
2-640	A	12:46	12:46	Delete ")" after "OH".' [Dylan Millet]	Accepted ..done
2-641	A	12:46	12:46	"its"? OH or CH4?? [Keith Shine]	Noted ..text refers to OH
2-642	A	12:46	12:46	remove ")" after "OH" [Peter Van Velthoven]	Accepted ..done
2-643	A	12:46		Unpaired ")" [Michael Coffey]	Accepted ..done
2-644	A	12:46		delete the bracket [Hartmut Grassl]	Accepted ..done
2-645	A	12:47	12:48	"it is therefore likely that only a reduction in the source strength of methane...": There is no clear evidence yet that CH4 emissions are really decreasing. The atmospheric CH4 mixing ratios do not yet decrease (apart from interannual variations). Assuming constant sinks, stabilisation of atmospheric CH4 mixing ratios means that emissions do not further increase (but not decrease). [Peter Bergamaschi]	Accepted
2-646	A	12:47	13:36	This paragraph makes the conclusion that "a reduction in source strength for methane is responsible for the decline in its growth rate over the past two decades". On the next page in the paragraph beginning on line 22 this subject is reopened and two possible reasons	Accepted text flow improved

No.	Batch	Page:line From	Page:line To	Comment	Notes
				for the decline in the growth rate are indicated. The two sets of conclusions need to be reconciled and discussed in one, not two, paragraphs. In addition the approach of methane to a steady state balance does not require decreasing, but constant, emissions as Dlugokencky has pointed out and this has not been discussed. [Derek Cunnold]	
2-647	A	12:50	12:50	" … in at least the last 450,000 years." Be accurate rather than needlessly rounding (and potentially attracting criticism for exaggerating). The appropriate is: [Delmotte et al. (2004): Delmotte, M., Chappellaz, J., Brook, E., Yiou, P., Barnola, J.M., Goujon, C., Raynaud, D., Lipenkov, V.I., 2004. Atmospheric methane during the last four glacial-interglacial cycles: Rapid changes and their link with Antarctic temperature. J. Geophys. Res., 109: D12104, doi:10.1029/2003JD004417.] [Keith Lassey]	Accepted but check new ref back to 800,000 years
2-648	A	12:50	12:50	are they precedented in other periods? - disclose [Stephen McIntyre]	Noted ...refer to earlier periods also
2-649	A	12:50	13:3	Discussion jumps between percentage and ppb. It would be easier to understand if ppb were given for all changes, perhaps with percentage points occasionally given in brackets. [Tami Bond]	Accepted
2-650	A	12:52	12:52	"…has increased by about 40%," from which year? [Xiaobin Xu]	Accepted and clarified
2-651	A	13:1	13:36	comment: too much detail after the main reason has been given on page 2-12, line 48 [Hartmut Grassl]	Accepted
2-652	A	13:1	13:3	This paragraph should also record that the growth rate fell below zero in ca 2004 (with appropriate caveats about the increased errors near the end-points of a fitted time series -- though they do not seem to be especially large at the upper end), which is evident from Figure 2.3.4. Leaving the reader with the parting statement that the global growth rate is presently increasing could attract criticism for potentially misleading the reader or for putting a biassed spin on the growth rate. [Keith Lassey]	Accepted but check latest data for 2005
2-653	A	13:1	14:17	This discussion is long and, albeit interesting, sometimes difficult to follow. A table summarizing the main suggestions (with references) that have been put forward to explain the recent decrease in growth rate as well as the 1992 and 1998 anomalies would be very helpful. [Katharine Hayhoe]	Noted
2-654	A	13:3	13:3	The sentence seems not to be completed. [Xiaobin Xu]	Noted
2-655	A	13:5	13:11	Figure 2.4.1. Caption intermixes CCN, CDCN, and CDNC. Why not just use cloud	Comment does not belong to this

No.	Batch	Page:line From	Page:line To	Comment	Notes
				droplet number concentration (CDNC). [Tim Bates]	section
2-656	A	13:5	13:12	This paragraph is unclear and needs to be rewritten. In the second sentence, what does the 'they' refer to? After reading the paragraph it wasn't clear to me whether or not the lack of understanding in methane growth rates presented in the TAR remain or whether they have been resolved. [Greg Bodeker]	Accepted text flow improved
2-657	A	13:5	13:8	First two sentences of paragraph: Is this text saying that no progress has been made since TAR? I doubt that is what you mean but it sounds that way. [Tami Bond]	Accepted and clarified
2-658	A	13:5	13:6	Consistency should be improved with line 43 to 45 on page 12. [G. H. Sabin GUENDEHOU]	Accepted and clarified
2-659	A	13:5	13:6	"In TAR...understood." Sentence is clumsy. Rewrite. [Patrick Hamill]	Accepted ...rewritten
2-660	A	13:5	13:5	Should read "In TAR the…" [Eleanor Highwood]	Accepted
2-661	A	13:5	13:6	The fact that reasons for the decrease in atmospheric methane's growth rate were not understood in TAR would not appear to be particularly noteworthy. [Andrew Lacis]	Accepted and removed
2-662	A	13:5	13:6	Sentence should read: "In the TAR… were not understood". This inserts "the" and uses past tense. [Keith Lassey]	Accepted
2-663	A	13:5	13:6	The references to "(Prather et al., 2001)" and to "TAR" are repetitive: both refer to "TAR", which is the accepted abbreviation for all references to that Assessment Report (see p. 7, lines 3-4). [Keith Lassey]	Accepted
2-664	A	13:5	13:6	reasons for the decrease in atmospheric methane's growth rate and the implications for future changes in its atmospheric burden are not understood - carry forward to summary [Stephen McIntyre]	Noted
2-665	A	13:5	13:5	Recommend deleting "In TAR". [Dylan Millet]	Noted
2-666	A	13:8	13:10	Avoid the use of "poorly"; the statement is too strong. Many researches are conducted on CH4 emissions from differents sources. And countries are reporting CH4 emissions from different sources in the framework of the UNFCCC. I'd like you to consult the different guidelines developed by the IPCC NGGIP for the establishment of GHG inventories. [G. H. Sabin GUENDEHOU]	Rejected...ranges reported by 7 different groups in TAR show > 100% for many sources ...this is "poorly known" GHG inventories not discussed in this chapter

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-667	A	13:8	13:9	Mention fossil contribution to CH4 emission explicitly. It is not negligible. [Fortunat Joos]	Accepted
2-668	A	13:8	13:12	I recommend the marked text to be moved up front (page 13, line 14) to serve as a first intruction to section 2.3.2. [Caroline Leck]	Noted
2-669	A	13:8	13:9	"…are mostly biogenic and include wetlands, rice agriculture, biomass burning, and ruminant animals…". This is poorly worded since rice agriculture is anthropogenic, as are to some extent biomass burning and ruminant animals. [Dylan Millet]	Accepted ...redefined
2-670	A	13:10		After "Wang et al. (2004)" please insert "By comparing model results with satellite measurements, Frankenberg et al. (Frankenberg, C., J.F. Meirink, M. Van Weele, U. Platt and T. Wagner, Assessing methane emissions from global space-borne observations, Science 308, 1010-1014, 2005) concluded that the tropical terrestrial biosphere is a much larger methane source than previously thought". [Jos Lelieveld]	Accepted ...good point alsowill add new Nature ref Keppler et al 2006 ref on methane emissions from living vegetation
2-671	A	13:13	14:41	Some explanation for the observed methane change is useful and desirable. But perhaps a significant fraction could more properly be moved to Chapter 7. [Andrew Lacis]	Accepted
2-2691	B	13:14	13:14	in ANTHROPOGENIC methane emissions [Olivier Boucher]	Accepted
2-672	A	13:14	13:20	The wording 'whereas' implies that these two papers give opposing views when they are saying essentially the same thing. (This opposing view is that described in the following paragraph). [Ian Enting]	Accepted
2-673	A	13:14	13:17	The two parts of the sentence separated by "whereas" could leave the reader with an impression that they are contradictory. They are not. [Keith Lassey]	Accepted
2-674	A	13:17	13:20	"Others have argued that predicting future atmospheric burdens is impossible…": (1) sounds very vague; (2) Of course such predictions are difficult and will always have some inherent uncertainties, but they should not be called 'impossible' (3) seems somewhat out of context as the discussion changes here from the analyis of trends and interannual variations of the last years to future scenarios [Peter Bergamaschi]	Accepted ..text changed
2-675	A	13:17	13:20	"…predicting future atmospheric burdens is impossible…" This is a strong statement-- too strong, I believe, for this document. For example, decoupling of population growth and emissions does happen, but can be handled with economic models, albeit with some	Accepted ..text changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				uncertainty. [Tami Bond]	
2-676	A	13:17		After "..since 1982" please insert "Measurements along the world largest natural gas transport pipeline system in Russia indicate that technological improvements have reduced methane leakages (Lelieveld, J., S. Lechtenböhmer, S.S. Assonov, C.A.M. Brenninkmeijer, C. Dienst, M. Fischedick and T. Hanke, Low methane leakage from gas pipelines, Nature 434, 841-842, 2005)". [Jos Lelieveld]	Accepted new ref and text inserted
2-677	A	13:22	13:36	The discussion on OH does not belong to this section and should be either in section 2.3.5 or in Chapter 7. It duplicates some of the discussions already provided in those OH section. [Didier Hauglustaine]	Noted
2-678	A	13:22	13:23	insert "partly" before "due" in line 23 Dentener et al relaxed towards latitudinally averaged observed surface concentrations of CH4 Hence there may be yet other factors. [Peter Van Velthoven]	Accepted
2-679	A	13:22		Changes in tropospheric water vapour could also be mentioned as a cause of changes in OH (here or in section 2.3.5) [Jan Fuglestvedt]	Noted
2-680	A	13:24	13:25	"Wang et al. (2004) attribute the slow down in methane emissions…" should be the slow down in methane growth rate in the atmosphere. [Dylan Millet]	Noted
2-681	A	13:25	13:26	"slowdown" is one word, not two [Patrick Hamill]	Accepted
2-682	A	13:25	13:25	emissions" is probably a typo - since emissions can only be sources not sinks.I could agree to "growth rate" instead of "emissions [Peter Van Velthoven]	Accepted
2-683	A	13:25	13:25	Change "emissions" to "concentration growth". [James S. Wang]	Noted ..changed to growth rate
2-684	A	13:28	13:33	The discussion of the OH evolution is repeated twice, in this paragraph and on page 12, lines 43-48. I would avoid repetitions. [Alcide di Sarra]	Accepted ..repetition removed
2-685	A	13:28	13:28	"… leading to a faster removal of methane …". The word "faster" is more appropriate than "greater". [Keith Lassey]	Accepted
2-686	A	13:29	13:29	Recommend changing "… imply no net change in OH" to "…imply no detectable net change in OH" or "…imply no significant net change in OH".	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Dylan Millet]	
2-687	A	13:30	13:31	"…Manning et al. (2005) using atmospheric 14CO as a tracer for global OH…" This study used 14CO measurement from New Zealand and Antarctica and concluded "no significant trend in the southern hemisphere OH during the 1990s". However this study is not sensitive to potential trends in the Northern Hemisphere. [Peter Bergamaschi]	Noted ...referred to discussion on OH
2-688	A	13:33	13:35	"Stable long-term OH concentrations indicate that the reduced source of methane is the likely cause of its decreasing long-term growth rate": There is no clear evidence yet that CH4 emissions are really decreasing. The atmospheric CH4 mixing ratios do not yet decrease (apart from interannual variations). Assuming constant sinks, stabilisation of atmospheric CH4 mixing ratios means that emissions do not further increase (but not decrease) [Peter Bergamaschi]	Accepted ...as above
2-689	A	13:35	13:36	"…its remarkable variability, most of which remains unexplained": Sounds very vague and negative. The studies which are discussed in the subsequent paragraph provide some explanations. [Peter Bergamaschi]	Accepted text flow improved
2-690	A	13:35	:36	The stated "remarkable variability" of methane is not generally accepted (see also comment nr. 12 below). In fact, it is counter-argued by Lelieveld et al. (Lelieveld, J., F.J. Dentener, W. Peters and M.C. Krol, On the role of hydroxyl radicals in the self-cleansing capacity of the troposphere, Atmos. Chem. Phys. 4, 2337-2344, 2004). The 5-10% variability inferred by Prinn et al. (2001) and the even higher variability up to 20% inferred by Manning et al. (2005) are inconsistent with methane growth rates inferred from NOAA/CMDL measurements, as suggested by Lelieveld et al. (2004). This can easily be seen from figure 2.3.4 (page 118), showing a global methane growth rate variability of 0.1-1%. Since the changing methane growth rates integrate the variability of sources and sinks (including trends), and since OH represents the main sink, the large OH variabilities inferred by Prinn et al. (2001) and Manning et al. (2005) must be an artefact of the methods applied. Alternatively, figure 2.3.4 would be totally wrong. I strongly recommend balancing the discussion and deleting the sentence "A feature of the slowdown...". [Jos Lelieveld]	Accepted ....refs added and argument balanced ....however two techniques based on totally different priciples with the same conclusion is compelling
2-691	A	13:38	13:38	Why isn't Figure 2.3.4 inserted much earlier, given its first cross-refeence on p. 12, line 41? [Keith Lassey]	Accepted
2-692	A	13:41	13:52	Wang et al. (2004) find that a combination of a decrease in wetland emissions and an increase in OH after the Pinatubo eruption results in good agreement between model and	Accepted ..text changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				observed CH4 growth rate at sites across the globe. [James S. Wang]	
2-693	A	13:44	13:44	A relevant reference on this issue that could be added: Fuglestvedt, J, J. E. Jonson and I. Isaksen, 1994. Effects of reductions in stratospheric ozone on tropospheric chemistry through changes in the photolysis rates. Tellus, 1994 (46B): pp. 172-192. [Jan Fuglestvedt]	Noted
2-694	A	13:45	:47	This was also proposed by Lelieveld et al. (Lelieveld, J., P.J. Crutzen and F.J. Dentener, Changing concentration, lifetime and climate forcing of atmospheric methane. Tellus 50B, 128-150, 1998), thus earlier than Walter et al.(2001). [Jos Lelieveld]	Accepted ..ref added
2-695	A	13:47	13:49	"13C/12C anomaly": I would not consider this as a very robust finding: Measurements from 3 other groups in the Southern Hemisphere do not confirm this anomaly: Measurements by CSIRO and university of Washington at Cape Grim [Francey et al., 1999] and measurement by university of Heidelberg at Neumeyer station [Marik, 1998] could not confirm this anomaly. Unfortunately, it seems that all (direct atmospheric) measurement before 1992 had somewhat poorer precision / accuracy. Reference: Francey et al., A history of d13C in atmospheric CH4 from the Cape Grim Air Archive and Antarctic firn air, J. Geophys. Res., 104, 23631-23643, 1999. Marik, T., Atmospheric d13C and dD measurement to balance the global methane budget, PhD thesis, university Heidelberg, 1998. [Peter Bergamaschi]	Noted and text clarified
2-696	A	13:57	13:57	Probably worth noting that the Langenfelds results is on the basis of measurements of a suite of biogenic trace gases. [Ian Enting]	Accepted and text added
2-697	A	14:3	14:7	"However Warwick et al. (2002) and Lowe et al. (2004) show…": The discussed changes of transport patterns related to El Nino / La Nina and NAO is certainly important for the interpretation of signals from individual sites. However, this should not be too important for the global average growth rate. [Peter Bergamaschi]	Accepted
2-698	A	14:3	14:6	As well as Warwick et al and Lowe et al, Dentener et al (2003) also showed the influence of inter-annually varying meteorology on methane removal rates. [Keith Lassey]	Accepted ref added
2-699	A	14:3	:6	Dentener et al. (Dentener, F., W. Peters, M. Krol, M. van Weele, P. Bergamaschi and J. Lelieveld, Interannual variability and trend of CH4 lifetime as a measure for OH changes in the 1979-1993 time period. J. Geophys. Res. 108, 4442, doi: 10.29/2002JD002916, 2003) calculated that meteorological variability is the largest factor determining the inter-	Accepted and ref added

No.	Batch	Page:line From	Page:line To	Comment	Notes
				annual variability of methane. [Jos Lelieveld]	
2-700	A	14:5	14:16	Figure 2.4.2 Add ICARTT to (b) International Consortium for Atmospheric Research on Transport and Transformation [Tim Bates]	Noted This comment seems to be misplaced
2-701	A	14:12	14:17	"Lassey et al show…:" If projections about future CH4 levels are to be discussed in this chapter this should not be based on a single conference presention. Such a discussion should include the IPCC scenarios and in addition a lot of further literature is available on this topic. Furthermore, a very important question in this context is the behaviour of natural sources (wetlands, permafrost, CH4 hydrates) in response to climate change. [Peter Bergamaschi]	Noted
2-702	A	14:12	14:16	This part should be more elaborated. The assumptions considered should be clearly stated for transparency. [G. H. Sabin GUENDEHOU]	Accepted ...text clarified
2-703	A	14:12	14:17	The reference to Lassey et al (2005) refers to a presentation at NCGG-4 and accompanying short paper in the proceedings. This paper (and several others from NCGG-4) are to appear in expanded form in a special issue of Environmental Sciences. Therefore it would be more approriate to exchange this reference for the following: [Lassey, K.R., Scheehle, E.A. & Kruger, D. in press. Towards reconciling national emission inventories for methane with the global budget. Environmental Sciences.]. [Keith Lassey]	Accepted ...reference changed
2-704	A	14:13	14:13	UNFCC should read UNFCCC [Philippe Tulkens]	Accepted
2-705	A	14:13		Define: UNFCC, is this the same UN FCCC on page 15, line 13 [Michael Coffey]	Accepted
2-706	A	14:13		correct "UNFCC" to "UNFCCC" [Hartmut Grassl]	Accepted
2-707	A	14:15	14:16	correct the phraseology: "… then atmospheric methane mixing ratios will resume growth rate approaching those of the 1960s by 2020." [Keith Lassey]	Accepted ...text clarified
2-708	A	14:16	14:16	Typo: "… takes into account only those mitigation measures …" [Keith Lassey]	Accepted
2-709	A	14:16	14:16	remove "of" before "those" (syntax errror) [Peter Van Velthoven]	Accepted
2-710	A	14:16		only of those mitigation" delete "of [Hartmut Grassl]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-711	A	14:19	14:21	The information is already given on page 12, lines 16-19. [Alcide di Sarra]	Accepted ...repetition deleted
2-712	A	14:19	14:21	This paragraph would seem to be more appropriate on page 2-12 after line 35. [Patrick Hamill]	Noted
2-713	A	14:19	14:21	What does "updated" mean in the reference "(Etheridge et al., 1998 updated)"? The basis for the updating should be explained and attributed. Since this "updated" reference is repeated in several places, this basis could be described in a footnote or following the reference citation. The "update" is not simply due to adjusting data from the CMDL83 calibration scale to the new NOAA04 scale in which reported mole fractions are 1.24% larger, because the value of 715 ppb here attributed to Etheridge et al. (1998 updated) in 1750AD is only 0.70% larger than the value of 710 ppb actually cited by Etheridge et al. (1998). [Keith Lassey]	Accepted ...updated removed from reference
2-714	A	14:19	:21	This paragraph appears out of context. [Katharine Hayhoe]	Noted
2-715	A	14:23	14:24	The RF due to changes...is calculated with the simplified expression given in the TAR." Is this the same as the "simple formula" mentioned on page 2-12 line 1 and again on page 2-14 lines 34-35, or is it something else? Please clarify. [Patrick Hamill]	Noted referred to Gunnar Myhre...calculated with the simplified expression for methane given in the TAR
2-716	A	14:24	14:24	The averages from AGAGE and CMDL networks are 1774.6 ppb and 1777.6 ppb, respectively. How can 1774 ppb be derived from the two averages? [Xiaobin Xu]	Noted ...these data will be updated with 2005 values for the SOD
2-717	A	14:28	14:28	Recommend changing "…preindustrial uncertainty estimate which is solely…" to "…preindustrial uncertainty estimate derived solely…" [Dylan Millet]	Accepted
2-718	A	14:30	14:30	Explain what "overlap" refers to. [Steven Massie]	Noted referred to Gunnar Myhre Noted partly taken into account ...text added to clarify
2-719	A	14:34	14:41	The CH4 radiative forcing discussion cited in Chapter 10 should be moved to Chapter 2. [Andrew Lacis]	Noted referred to Gunnar Myhre Accepted ...text moved to chap 10
2-720	A	14:35	14:41	To non-specialist, these comparisons are hard to follow. Specifically what is a "line-by-line model"? Explain "atmospheric background profile". [Keith Lassey]	Noted referred to Gunnar MyhreNoted this section moved to Chap 10
2-721	A	14:35	14:37	The Collins reference was unavilable to this reviewer, but it is highly doubtful that the Collins work used a line-by-line model that included ALL of the known CH4 bands. A known deficiency of the recent release of the HITRAN database is that it lacks a complete	Noted referred to Gunnar Myhre Noted this section moved to Chap 10

No.	Batch	Page:line From	Page:line To	Comment	Notes
				methane line list. This is especially important in the visible and near infrared regions. [Charles Miller]	
2-722	A	14:35	14:35	It seems an overstatement, especially considering the authorship of this chapter and the cited TAR work (Ramaswamy et al., 2001) to claim that the earlier work "remains valid". How about simply saying that this earlier formulation "is used here as well"? [Ray Weiss]	Rejected. Remains valid ...no studies since the TAR
2-723	A	14:43	17:39	Sections 2.3.3 and 2.3.4: Please see my comments above regarding the emphasis in this chapter away from atmospheric observations and toward radiative forcing. These two sections are the most affected by this bias. There are no graphs of the observational data other than one for N2O, and minimal discussions of the uncertainties in calibrations and other outstanding issues. Instead, the reader is referred to the Velders et al. (2005) chapter in the IPCC-SROC report. This report is not as widely available or as influential as the IPCC FAR will be. Indeed, I do not have a copy and I cannot easily get one before this review is due because it must be purchased and is not available on the web (except in summary form). [Ray Weiss]	Accepted. New Figures added showing growth of all gases in sections 2.3.3 and 2.3.4.
2-724	A	14:43	17:39	Sections 2.3.3 and 2.3.4 continued: The result of these omissions is that an entire experimental community, its accomplishments and its outstanding issues are denied representation to the readers of the IPCC FAR, including policymakers, funding agencies, etc. I understand from Lead Authors of this chapter that such information was, in fact, included in an earlier draft of this chapter, and was subsequently eliminated for the sake of brevity. I believe this was unwise and inappropriate, and I strongly encourage the reinstatement of a substantial amount of this key missing information. [Ray Weiss]	Accepted. See reply to 2-723
2-725	A	14:43		Section 2.3.3 - There is no mention of the atmospheric lifetime of N2O [Charles Miller]	Accepted. Lifetime quoted.
2-726	A	14:45	14:45	Clarify "important". Important in terms of what? [G. H. Sabin GUENDEHOU]	Accepted. Text clarified to mention RF.
2-727	A	14:45	14:55	Three references to "(Prather et al., 2001)", one to "(Ramaswamy et al., 2001)" and one to "TAR" all in fact refer to "TAR", which is the accepted abbreviation for all references to that Assessment Report (see p. 7, lines 3-4). [Keith Lassey]	Editorial decision. Should we say TAR (Chap xx)?
2-728	A	14:45	14:45	This sentence is plain wrong. You are referring to contributors to the forcing not contributors to the greenhouse effect [Keith Shine]	Accept. See 2-726 reply.
2-729	A	14:45	15:32	There are several unnecessary references to TAR that convey little useful information. [Andrew Lacis]	Editorial Decision. Do we summarize TAR to highlight differences from

No.	Batch	Page:line From	Page:line To	Comment	Notes
					AR4?
2-730	A	14:45		add: "increase" after first formula [Hartmut Grassl]	Noted. Order is however obviously increasing.
2-731	A	14:45		correct "important greenhouse gas" to "important anthropogenic greenhouse addition" [Hartmut Grassl]	Noted. See reply to 2-726.
2-732	A	14:45		This section would benefit from moving some of the material from the second paragraph to the first, as does section 2.3.2, to present the long-term records first then place more recent concentrations in context. [Katharine Hayhoe]	Noted. Will see if this is useful.
2-733	A	14:51	14:53	This sentence is unclear. How can the rate of increase be consistent, when the sources are not known [Fortunat Joos]	Accept. Reworded.
2-2692	B	14:54	14:54	"was thought": has thinking changed since then? [Olivier Boucher]	Accept. Thought changed to concluded.
2-734	A	15:1	15:7	Calculation of RF is given. For compatibility with the earlier sections, are there updates or confirmations of the calculations of line-by-line forcing? [Tami Bond]	Refer to Gunnar Myhre
2-735	A	15:1	15:1	Synchronize ice core data set with chapter 6. Data are available back to the LGM [Fortunat Joos]	Editorial decision. How far back do we go in Chap 2?
2-736	A	15:1	15:1	Should that leading sentence read: "Ice-core data … are now available extending back 2000 years before present …". (Two errors corrected here: "data" is plural, and "extending by 2000 years" is incomplete). [Keith Lassey]	Accepted all changes.
2-737	A	15:1	15:1	Change "extending by" to "extending back". [Dylan Millet]	Accepted. See 2-736 reply
2-738	A	15:1	15:1	Change "is now available" to "are now available". [Ray Weiss]	Accepted. See 2-736 reply
2-739	A	15:1		replace "by" by "back" [Joanna Haigh]	Accepted. See 2-736 reply
2-740	A	15:3	15:3	When compared to CO2 to CH4 and to several other GHGs, the rise in N2O is hardly "rapid". [Keith Lassey]	Accept. Add "relatively" before "rapid".
2-2693	B	15:9	15:31	Tg N yr-1 is clear enough [Olivier Boucher]	Noted.
2-741	A	15:9	51:11	Replace "Kroeze et al. ...." with "The results of various studies, which quantified the global N2O emissions from coastal areas such as continental shelves, estuaries, coastal	Accepted. References and text added.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				upwelling areas and rivers suggest that coastal areas contribute significantly (0.3 - 6.6 Tg N /yr-1 or 7 – 61 %) to the global N2O oceanic emissions (Bange et al. 1996, Nevison et al., 2004, Kroeze et al. 2005). Bange, H.W., S. Rapsomanikis, and M.O. Andreae, Nitrous oxide in coastal waters, Global Biogeochem. Cycles, 10 (1), 197-207, 1996; Nevison, C., T. Lueker, and R.F. Weiss, Quantifying the nitrous oxide source from coastal upwelling, Global Biogeochem. Cycles, 18, GB1018, doi:10.1029/2003GB002110, 2004 [Hermann W. Bange]	
2-742	A	15:10		Define N2O-N [Cathy Clerbaux]	Accepted. Definition added.
2-743	A	15:11		Please insert cross reference to chapter 7, section 7.4.2.1 [Hermann W. Bange]	Accepted. Crossref added.
2-744	A	15:31	15:31	By when should the N2O RF take over third place from CFC-12 (or is that for a future forcing chapter) [Eleanor Highwood]	Refer to Gunnar Myhre
2-745	A	15:38		"Chapter 2" should be added before "(Velders et al., 2005)". [Derek Cunnold]	Accept. Done.
2-2694	B	15:40	15:40	is this 1-sigma? [Olivier Boucher]	Comment misplaced. There are no numbers quoted on this line or near it.
2-746	A	15:42	15:50	Figure 2.4.3 Is this the clear-sky, 24-hour averaged annual mean? [Tim Bates]	Comment misplaced. Refer to scetion 2.4 for action.
2-747	A	15:42	15:52	There is some unnecessary repetition here. [Andrew Lacis]	Not really repetition since one refers to cross-sections and the other to RF. However the more relevant topic is RF so text shortened. Also cross-reference to section 2.10.1 added.
2-748	A	15:48	15:52	These lines seem to be a restatement of lines 42-45 [Michael Coffey]	See 2-747 reply.
2-749	A	15:50	15:52	Redundant information with page 2-66 [Cathy Clerbaux]	Accept. See 2-747 reply.
2-750	A	15:55	15:57	Definition of HFCs, HCFCs, …(E.g HFC-134a=CF3CH2F) should come earlier, E g. in Table 2.3.1 [Cathy Clerbaux]	Noted. Definitions are left here but could be removed if Table 2.3.1 amended to include them.
2-2695	B	16:9	16:9	a bit awkward. Destruction is as important as emission for a budget. Changes in budget are more influenced by emissions, is this what is meant? [Olivier Boucher]	Replaced "budget is" with "current trends are".
2-751	A	16:15		delete: "ist significantly"	Accept.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Hartmut Grassl]	
2-752	A	16:21	16:25	Figure 2.4.4 What is "albedo radiative forcing"? It can't be forcing if it includes sea salt and dust. [Tim Bates]	Comment misplaced. Refer to section 2.4 for action.
2-753	A	16:23	16:23	Need references to the lifetimes of PFCs. [A. R. Ravishankara]	Noted. We already refer to section 2.10 for this, so comment unclear. Have added refernce to Table 2.10.1 where all lifetimes are listed.
2-754	A	16:24	16:26	These two sentences don't make sense. [Eleanor Highwood]	Accepted. Text clarified.
2-755	A	16:24	16:25	"SF6 and CF4 concentrations and RF have increased by over 20% since TAR, CF4 concentrations have not updated." It seems that there is a contradiction (as well as poor grammar) here. [Dylan Millet]	Accepted. See 2-754 reply.
2-756	A	16:24	16:24	SF6 and CF4" should probably be "SF6 and C2F6 [Peter Van Velthoven]	Accept. Correction made.
2-757	A	16:25	16:25	I am surprised that no changes in CF4 are cited. [Keith Shine]	No updated measurements are available. Need to followup to see if they can be obtained.
2-758	A	16:25	16:25	add "been" before "updated" (grammatical error) [Peter Van Velthoven]	Accept. Done.
2-759	A	16:25		comment to: "concentrations have not updated" incomplete [Hartmut Grassl]	Accept. See 2-758 reply.
2-760	A	16:27	16:27	Recommend using either CF4 or PFC-14 consistently rather than going back and forth. [Dylan Millet]	Accept. Use CF4.
2-761	A	16:31	16:31	Recommend changing "and continue to increase linearly over the past decade" to "have continued to increase…". [Dylan Millet]	Accept. Done.
2-762	A	16:32	16:32	"emissions may be levelling off" would mean to me that the growth in emissions has stopped.This is not true, as its concentrations continue to increase linearly. What is probably meant is that the growth rate of SF6 is levelling off. [Peter Van Velthoven]	Reject. With no effective sink its constant trend (dC/dt) implies constant emissions. Text modified to clarify.
2-763	A	16:39		correct "Halons" to "halons" [Hartmut Grassl]	Accept. Done.
2-764	A	16:46	16:46	7 should be 7.5 GtCO2-eq (see approved IPCC/TEAP special report) [Guus Velders]	Accept. Done.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2696	B	16:52	16:52	could you spell out acronyms. [Olivier Boucher]	Editorial decision. This should be done in Table 2.3.1 or in an appendix defining all acronyms in the chapter or in the entire report.
2-765	A	16:53		Definition of CFC11 and CFC113 already provided [Cathy Clerbaux]	Accept. CFC names used alone.
2-766	A	17:0	19:	I think that the entire discussion of the OH trends is one-sided. The measured MCF abundances do not suggest a need for a significant trend in the "average" value of OH. This average value is strictly speaking only applicable to MCF loss and those of similar lifetimes and temperature dependence in OH reactivity. This section makes it seem like we really know what OH is doing in the troposphere. I strongly suggest that this section present all sides and be rewritten for balance. Use of words like" deduced" (line 10) and confirmed (line 14) are way too strong! [A. R. Ravishankara]	Noted. The use of these methods to deduce loss rates for MCF-like and 14-CO-like gases is well established (see FAR/SAR/TAR/SROC and multiple WMO ozone reports). No claim is made that these OH estimates tell us all that is going on with tropospheric OH. Have changed "deduced" to "inferred" and "confirmed" to "supported". CH 7 contains discussions of the photochemical kinetics controlling OH that this reviewer may be seeking.
2-767	A	17:1	19:10	OH is certainly an important factor affecting atmospheric GHG concentrations, particularly methane. However, OH is not a direct contributor to RH. Perhaps this section would be more at home in Chapter 7. Otherwise, what about the role of CO, NOx, OCl, ect.? [Andrew Lacis]	Reject. Since OH estimates are inferred directly from long-term measurements of LLGHGs and CO they are similar in principle to RF estimates from LLGHGs. Agreement between CH 2 and CH 7 is to discuss inferred OH trends in CH 2 and possible reasons for these trends in CH 7.
2-2697	B	17:16	17:16	it's ==> its [Olivier Boucher]	Accept. Done.
2-768	A	17:16	17:16	its", not "it's [Ian Enting]	See 2-2697 reply.
2-769	A	17:16	17:16	its relatively short lifetime (not it's) [Robert KANDEL]	See 2-2697 reply.
2-770	A	17:16	17:16	A lifetime of 5 years is given in Table 2.10.1 [Guus Velders]	Noted. Prinn et al (2005a) inferred 4.9+/-0.3 for 1978-2004.
2-771	A	17:16		replace "it's" by "its" [Joanna Haigh]	See 2-2697 reply.

No.	Batch	Page:line From	To	Comment	Notes
2-772	A	17:21	:22	Referring to the data by Reimann et al. (2005) as "surface data" is highly deceptive since the measurements were performed on the Swiss mountain Jungfraujoch, at an altitude of nearly 4 km. Real surface data in N-Italy and Greece have shown that the Jungfraujoch data are not representative for southern Europe. Note that Krol et al. (2003) assumed a substantial post-1997 methyl chloroform source in southern Europe (see also Gros, V., J. Williams, J.A. van Aardenne, G. Salisbury, R. Hofmann, M. Lawrence, R.von Kuhlmann, J. Lelieveld, M. Krol, H. Berresheim, J.M. Lobert and E. Atlas, Origin of anthropogenic hydrocarbons and halocarbons measured in the summertime European outflow on Crete in 2001), Atmos. Chem. Phys. 3, 1223-1235, 2003.). I strongly recommend to to more neutrally indicate that Krol et al. (2003) and Reimann et al. (2005) come to different conclusions. [Jos Lelieveld]	Noted. Jungfraujoch is a surface station and it does episodically receive air from much lower altitudes as documented by measurements of many urban-industrial pollutants. This is discussed in the Reimann et al paper which was reviewed for the same journal and by the same editor as the earlier Krol, Lelieveld et al paper. The language used on lines 21-22 here is not perjorative but simply states the published results.
2-773	A	17:24	17:24	"…more limited measurements by Millet and Goldstein, 2004". This is a bit misleading since the Li et al results are based on data from only 1 site, while the Millet and Goldstein results are more limited temporally but are from 2 sites. [Dylan Millet]	Accepted in part. For their USA estimates Li et al used 2 sites (Trinidad Head and Harvard Forest). Have added "temporally but less geographically" before "limited".
2-774	A	17:32	17:32	The wording gives a misleading impression of causality. Propose: 'The fact that (unlike ...) a significant interhemispheric gradient of CCl4 exists, in spite of its moderately long lifetime of 25-30 years, results from a persistence of northern hemispheric sources. [Ian Enting]	Accepted. Done.
2-775	A	17:34	17:34	Why is a lifetime of 25-30 years given for CCl4? Table 2.10.1 gives 26 yr and their are indications that it is lower. [Guus Velders]	Accepted in part. Now say "about 20-30 years". See 2-776 reply.
2-776	A	17:34		Soil sinks of carbon tetrachloride have recently been argued for. It is estimated that these would decrease the lifetime to 20 years. Therefore I suggest changing "25-30 years" to "20-30 years". [Derek Cunnold]	Accepted. Done.
2-777	A	17:34		The long lifetime of CCl4 would tend to smooth out the interhemispheric gradient, so it wouldn't explain the persistence of a significant gradient. [James S. Wang]	Accept. See 2-774 reply.
2-778	A	17:36	17:39	The 2002 WMO report on ozone has provided an at least equally good discussion of HCFC trends and emission rates up to 2000 and it should certainly be referenced here. [Derek Cunnold]	Accept. Reference to Montzka et al (WMO, 2002) added.
2-779	A	17:41	19:9	There is some redundancy with section 7.4.5 which should be clarified between Ch. 2 and Ch 7. Probably from my Chap. 7 side. The tone of section 2.3.5 seems a bit odd though. It looks like the assessment is pushed a bit too far here trying to justify the Prinn et al.	Noted. At the Christchurch LA meeting a division was agreed upon between CH 2 and CH 7 for these OH

No.	Batch	Page:line From	Page:line To	Comment	Notes
				results in light of more recent studies. AR4 is not the place to provide a plea in favor of such or such study. There is a debate going on in the community but please do not use AR4 to convey this expert discussion to a more general audience. [Didier Hauglustaine]	discussions (the agreement included Dr Hauglustaine). See 2-767 reply. The "tone" of the discussion here is simply reporting what has appeared in the peer-reviewed literature which clears up some but not all of the prior debated points.
2-780	A	17:41	19:9	Section 2.3.5: This section is well written, but it has major overlaps with Section 7.4.5 in Chapter 7. It even uses essentially the same figure (Figure 2.3.6 and Figure 7.4.7). This should be reconciled. Given the chapter titles, I suggest that the deduced OH changes should be in Chapter 2 and their biogeochemical causes should be in Chapter 7. This is a bit awkward, but I think the Coordinating Lead authors of these two chapters can find a solution. In general, I found the discussion in Chapter 7 to give a better perspective. Whatever solution is found, I hope this will not be lost. [Ray Weiss]	Accept. See 2-767 and 2-779 replies. New Figure to appear in CH 2 that includes MCF and 14CO methods for OH through 2004.
2-781	A	17:41		In order to make it easier to digest the discussion of sections 2.3.2-2.3.4, I recommend section 2.3.5 to be moved to page 12, prior to section 2.3.2. This as the OH-radical is the major player in oxidizing both CH4 (2.3.2) and the other gases discussed in 2.3.3 and 2.3.4. [Caroline Leck]	Editorial decision. Case exists for either ordering.
2-2698	B	17:44	17:44	ameliorating => could you say limiting instead? [Olivier Boucher]	Accept. Done.
2-782	A	17:44	17:44	Do you really mean "ameliorating RF", shouldn't it mean "reducing RF" ? [Ralf Koppmann]	Accept in principle. See 2-2698 reply
2-783	A	17:47	17:47	Recommend changing "measured" to "estimated". [Dylan Millet]	Accepted in principle. Use "inferred".
2-784	A	17:48	17:48	Insert "is" between "purpose" and "CH3CCl3". [Dylan Millet]	Accept. Done.
2-785	A	17:48		Include: "purpose" is "CH3CCl3" [Michael Coffey]	Accept. See 2-784 reply.
2-786	A	17:48		add: "is" after "for this purpose" [Hartmut Grassl]	Accept. See 2-784 reply.
2-787	A	17:50	17:52	This sentence is difficult to understand. [Tami Bond]	Accept. Add "14CO"before "source".
2-788	A	17:50	17:54	The absolute accuracy of the 14CO source is definitely lower than that for MCF - but I would argue that Manning et al (2005) now demonstrates that we probably know relative	Accept in part. If one adds in the NOAA MCF in situ and flask sites

No.	Batch	Page:line From	Page:line To	Comment	Notes
				changes in 14CO production over time very well - see the Supplementary material associated with that paper which shows the strong contrast in simulated 14CO time series for different estimates of 14C production that previously could not be distinguished. This means that 14CO should be useful for OH trend analyses. I would also query the statement that spatial coverage of 14CO measurements does not match that of MCF - there are about the same number of fixed sites for 14CO as in ALE/GAGE and for 14CO we have added information from regular ship (N & S Pacific out of NZ), aircraft (CARIBIC + NZ to Antarctica), and train (Trans-Siberia) measurements. The statement that 14CO gives information on regional OH is important and deserves emphasis, but it should also be noted that it is more sensitive to short term OH variability (month-scale) than MCF. [Martin Manning]	there are many more for MCF. Text added to address the relative source change and short term OH points.
2-789	A	17:52	17:52	A lifetime of 5 years is given in Table 2.10.1 [Guus Velders]	Noted. See 2-770 reply. Editorial decision needed?
2-790	A	17:56		Definition of HFC134a ect already provided [Cathy Clerbaux]	Accept. HFC/HCFC notation used.
2-791	A	18:4	18:7	"the global weighted average OH..." - weighted by what? I recall that Prinn et al. used the MCF mass (*not* its reaction rate), while I think Krol and Lelieveld used the MCF reaction rate in this study (in earlier studies Krol weighted by volume). A couple lines later Quay et al 2000 is cited stating "although the weighting here is different." The actual weightings used need to be checked carefully and specified properly. Lawrence et al. (Lawrence, M.G., P. Jöckel, and R. von Kuhlmann, 2001: What does the global mean OH concentration tell us? Atmospheric Chemistry & Physics, 1, 37–49) would be appropriate to cite here, it showed that the difference in global mean OH concentrations using different weightings with the same fields could be substantial (even exceeding the trends compted in most studies). [Mark Lawrence]	Accept in part. Different weightings affect absolute but not relative OH changes so comment on trends not valid. Text added to alert reader to the issue of weighting brought up here (although it has been well discussed in previous IPCC and WMO Assessments).
2-792	A	18:6		Suggest "spatial" before weighting for clarity. [Martin Manning]	Accept. Done.
2-793	A	18:7	:8	After "..different" please insert "Methods to infer global average , however, can be insensitive to regional OH changes. Lelieveld et al. (Lelieveld, J., W. Peters, F.J. Dentener and M.C. Krol, Stability of tropospheric hydroxyl chemistry. J. Geophys. Res. 107, 4715, doi: 10.1029/2002JD002272, 2002) argue that OH concentrations over the continents have increased concurrent with anthropogenic NOx emissions, whereas they have decreased over the oceans, and these regional changes have compensated on a global scale. While the global average OH concentration appears fairly well defined by the	Accept. Text and reference added.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				indirect methods...". [Jos Lelieveld]	
2-794	A	18:22	18:29	Estimates of OH in the 1990s are particularly sensitive to the emissions assumed for methyl chloroform. Prinn et al. (2005) use some debatable methods; thus, the view of, e.g., Krol and Lelieveld (2003) should be given more weight. Specifically, Prinn et al. do not fully consider the effect of recent observation-based estimates of MCF emissions on their inferred OH. If the new emissions are used, the OH trend in the 1990s would be closer to 0, and the dip around 1998 would be much shallower. Accounting for Wennberg's revised estimate of the ocean sink/source for MCF would result in even less of a negative trend in OH in the 1990s. [James S. Wang]	Reject. Reviewer misquotes Prinn et al (2005) who included both observation-based emission information and Wennberg sink. See 2-258 reply for more.
2-795	A	18:27	18:28	Why is the OH minimum assocated with global wildfires and the intense El Nino? (briefly discuss e.g. enhanced CO levels) [Steven Massie]	Accepted. Brief phrase added. Main discussion should be in CH 7.
2-796	A	18:31	:41	This paragraph would fit better with the previous description of methods to calculate OH rather than following a discussion of estimated changes in OH. [Katharine Hayhoe]	Noted. Case can be made for either ordering .
2-797	A	18:33	18:34	I suggest rephrasing …'and may even become a small source to the troposphere beginning in 1999 (Prinn et al, 2001; 2005a)' [Tami Bond]	This is a model-dependent result so best to leave as is.
2-798	A	18:39	19:39	…column ozone anomalies from is…" -> "…column ozone anomalies estimated from ground-based and satellite-based measurements is… [Xiaobin Xu]	Comment is misplaced. Refer to section 2.4?
2-799	A	18:43	18:55	The comments of the authors on Bousquet et al. (2005) paper (I am the first author on this paper) need to completed to reflect the two main conclsions of the paper: 1/OH variations are found substantial when applying the same methodology than Prinn and Krol previous studies, even when using interannual meteorology and 3D modeling BUT 2/When optimizing both MCF emissions together with OH variations, OH varibility is reduced by 65%. This second statement cannot be separated from the first one. I propose to modify the text as follows: "More recently, Bousquet et al. (2005) have used an inverse method with a 3D model and methyl chloroform measurements and concluded that substantial year-to-year variations occurred in global-average OH concentrations between 1980 and 2000. This conclusion was previously reached by Prinn et al. (2001), but subsequently challenged by Krol and Lelieveld (2003) who argued that these variations are caused by model shortcomings and that models need, in particular, to include observationally-based, interannually-varying meteorology to provide accurate	Accepted with minor variations. New Figure will include what is proposed here but extended to 2004 to include Prinn et al (2005) and Manning et al 14CO –derived OH. Also effect of emission uncertainty text to include points made here plus Prinn et al (2001,2005) ensemble Monte-Carlo runs which also allow smaller OH variations.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				annual OH estimates. The Bousquet et al. (2005) analysis, which uses observationally-based meteorology and estimates both MCF emissions and OH on monthly time scales, yields interannual OH variations that agree remarkably well with the Prinn et al. (2001) and equivalent Krol and Lelieveld (2003) estimates when letting OH variations to adjust freely (see Figure 2.3.6). However, when OH variations are optimized with MCF emissions authorized to vary within inventory bounds (Mc Culloch and Midgley, 2000), then OH variations are reduced by 65% (dashed line on Figure 2.3.6), indicating that uncertainties on MCF emissions are critical to estimate OH variability in this approach. However the phasing of the OH variations seems robust in Bousquet et al (2005). Bousquet et al. (2005) also deduced that OH in the Southern Hemisphere shows a zero to small negative OH trend in qualitative agreement with Prinn et al. (2001)." I also propose the following figure to update figure 2.3.6. This new figure is a synthesis of the figures 10 & 12 from Bousquet et al. (2005) paper . This figure will be sent to Dr PRINN independently. [Philippe Bousquet]	
2-800	A	18:43	18:55	The Bousquet et al reference is selectively cited to provide evidence that the interannual OH variations are real. I suggest to make the story more balanced by adding after line 55: "However, Bousquet et al. (2005) also show that the magnitude of the derived OH variations are very sensitive to the assumed error in the emission estimates. They conclude that a more robust determination of the OH variations can only be attained if the uncertainties in the methyl chloroform emissions are significantly reduced." [Peter Van Velthoven]	Accepted. But will use Bousquet's proposed text. See 2-799 reply.
2-801	A	18:54	18:55	"OH" can be deleted from "… to small negative OH trend…". [Xiaobin Xu]	Accept. Done.
2-802	A	18:54		delete "OH" [Hartmut Grassl]	Accept. See 2-801 reply.
2-803	A	19:0	21:	For both stratospheric and tropospheric ozone, the space is limited and the authors have worked hard to cover the key points, with an appropriate emphasis on modelling. It might be helpful, however, to say more in each subsection about advances in available data and model/data comparisons. With regard to stratospheric ozone, the UNEP/WMO (2002) assessment is a helpful reference, and satellite measurements of e.g., HCl and ClO could merit mention, along with their comparisons to models These support the conclusion that the ozone decrease is stable, or beginning to decline. With regard to tropospheric ozone, satellite observations of CO and NO2 could be helpful to note. These have revealed important information on the distribution of precursors and trends, etc., aginst which the models cited have been compared. New insights have been gained on such issues as regional emissions (recent paper in Nature by Richter et al) and the role of	Noted. Will coordinate with chapte r7 and add WMO references

No.	Batch	Page:line From	Page:line To	Comment	Notes
				lightning. I realize that this requires coordination with chapter 7. [Susan Solomon]	
2-804	A	19:5	19:6	The eruption of Mt. Pinatubo likely increased global OH in 1992-93 due to increased levels of UV penetrating into the troposphere (Wang et al., 2004). [James S. Wang]	Noted. Pinatubo aerosol reflected UV so likely to decrease OH in troposphere? Or did strat O3 decrease offset this? Any discussion on this belongs in CH 7 by agreement.
2-805	A	19:7	19:9	"This evidence for substantial OH variability obtained from both CH3CCl3 and 14CO is not mirrored in global atmospheric chemistry models." Really? This statement is not appropriate without some references and a bit of discussion. Is that the case even when phenomena such as Pinatubo and the 1997 Indonesian fires (which, it is suggested, are at least part of the reason for the inferred variability) are included in the models? [Dylan Millet]	Sentence deleted and subject deferred to CH 7.
2-806	A	19:7	19:9	Statement is incorrect: chemical models do show substantial interannual variability in OH, e.g. Karlsdottir and Isaksen (2000), Dentener et al. (2003), Wang et al. (2004). [James S. Wang]	Noted. This comment contradicts comment 2-808. Discussion belongs in CH 7. See 2-805 reply.
2-807	A	19:8	19:9	It would be nice to support this statement with some references. [Eugene Rozanov]	Deleted. See 2-805 reply
2-808	A	19:9		Please add after "atmospheric chemistry models" "and is not consistent with the interannual variability in methane growth rates of about 1% or less, as shown in figure 2.3.4 (Lelieveld, J., F.J. Dentener, W. Peters and M.C. Krol, On the role of hydroxyl radicals in the self-cleansing capacity of the troposphere, Atmos. Chem. Phys. 4, 2337-2344, 2004). [Jos Lelieveld]	Noted. This comment contradicts comment 2-806. Discussion belongs in CH 7. See 2-805 reply.
2-809	A	19:10		What is the conclusion of this section regarding OH trends? [Katharine Hayhoe]	Given in Executive Summary on pg. 2-4.
2-810	A	19:11	19:29	The AIRF approach may be a useful new perspective, but the presentation could be improved. The fact that this is not an available paper makes it difficult to go deeper into this for the reader. The explanation in the text could be harmonized somewhat better with the figure text. [Jan Fuglestvedt]	Section dropped
2-811	A	19:11	19:29	This section serves no useful purpose in this Chapter. Accordingly, this section and Figure 2.3.7 should be omitted. It is of course important to know the sources and sinks of the radiatively significant GHGs, and this can be readily expressed in terms of emission rates. There is absolutely nothing gained by trying to express this in terms of radiative forcing. Radiative forcing is an "instantaneous" quantity and depends only on the current atmospheric distribution of absorbing gases. Sinks and sources is more a topic for	Section dropped

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Chapter 7. [Andrew Lacis]	
2-812	A	19:11	19:11	I wasn't quite sure why this paper gets a whole sub-section to itself - it sounds quite interesting, but not so sure what the "useful insights" are, beyond those that we are generally aware of. I hope (line 27-28) that it doesn't say that we know which molecules are removed in a given year (i.e. we know that they are the ones that were emitted in that year)! Is it proopsed as an GWP alternative or what? [Keith Shine]	Section dropped
2-813	A	19:11	19:29	I think it is better to place this subsection directly before the subsection 2.3.9. [Xiaobin Xu]	Section dropped
2-814	A	19:11		Section 2.3.6. This is an interesting concept. However it needs slightly more explanation; the present discussion cannot be understood without reading the Manning (2005) paper. Is this AIRF the radiative forcing added each year from emissions? Is it the RF experienced during the year, or is a time horizon associated with it, as with GWP? [Tami Bond]	Section dropped
2-2699	B	19:11		This is a potentially interesting new concept but I did not understand it (sorry I did not read the original manuscript). Can it be made more clear? [Olivier Boucher]	Section dropped
2-815	A	19:11		Although the text is transitioning at this point from discussion of direct to indirect forcing, this is implied rather than stated and section 2.3.6 appears an awkward fit here. [Katharine Hayhoe]	Section dropped
2-816	A	19:11		This section 2.3.6 introduces ideas and concepts new to almost all readers, yet are ill-explained. It is possible to summarise Manning (2005) more effectively even if a little more expansively. Specifically, it should be reported that: (a) Manning used the term "Input Radiative Forcing" (IRF), not "Annually Input Radiative Forcing" (AIRF); (b) that it is the "net RF" that is associated with what is otherwise and hitherto known as the RF. The concept could usefully be introduced by saying something like (after first introducing the Manning citation): "The annual change in abundance that is used to calculate the annual change in RF arises as a balance between an annual anthropogenic emission and an annual removal of excess (over natural) abundance. It is therefore possible to regard the RF as a net forcing that also arises from a balance between that due to the same annual emission (the "Annual Input Radiative Forcing", or AIRF) and that due to the annual removal ("removed RF)". Examining the AIRF and removed RF separately provides insight into the magnitude and trend of "net RF" that cannot be gained by looking at the net RF in isolation." [Keith Lassey]	Section dropped
2-817	A	19:11		The paper (Manning, 2005) referred to in this section was not accepted by PNAS and is	Section dropped

No.	Batch	Page:line From	Page:line To	Comment	Notes
				unlikely to be resubmitted elsewhere in time to be cited in the second draft. [Martin Manning]	
2-818	A	19:13		At least put (Long-lived greenhouse gases) in parentheses after the first acronym [Vincent Gray]	Section dropped
2-819	A	19:20	19:20	The caption to the figure is confusing. "In order to identify the effect of emissions on radiation forcing AIRF compares the radiative forcing due to an actual change in abundance of a particular greenhouse gas with the radiatiove forcing that would have occurred in the absence of emissions." Very strange sentence. I thought the AIRF is the top of the bars. So this is only the radiative forcing that would have occurred in the absence of sinks. It is not comparing the radiative forcing due to an actual change in abundance. (which is, I think, the "net" shown in the figure). The next sentence is also confusing. "The AIRF (this is what "it" refers to?) uses the estimated removal rates to infer the reduction in concentration and radiative forcing that would occur in the absence of emissions". I thought the hatched bar shows the reduction in radiative forcing that would occur in the absence of emissions. [Joyce Penner]	Section dropped
2-820	A	19:22	19:23	Replace "each year" by "each 5-year-period", since the figure referred to shows only data for 5 year intervals [Ralf Koppmann]	Section dropped
2-821	A	19:22	19:29	This is a nice analysis. [Jerry Mahlman]	Section dropped
2-822	A	19:30	19:30	There is a decided absence of discussion on the absorption of solar radiation by atmospheric gases. There shoud be a separate section that deals with this topic. About 20% of the incident solar radiation is absorbed within the atmosphere, and from an energy balance perspective, it is important to know what gases are doing the absorption, and whether they might be changing. Water vapor is the principal absorbing gas that varies greatly, but there is also absorption by O3, O2, O2-O2, N2-O2, CO2, CH4, N2O, NO2, in addition to cloud and aerosol absorption. Gases with fixed (unchanging) atmospheric concentrations are perhaps of little interest when it comes to climate change. But they are, nevertheless, contributors to the global energy balance. NO2 is being monitored by SAGE II and GOME. It is seasonally variable, an is important in biomass burning areas, and it absorbs a non-negligible amount of solar radiation. Perhaps a table could be included listing the different gases that absorb solar radiation and the range of absorption attributable to each gas. [Andrew Lacis]	Partalli accepted. No new section but solar absorption will be mentioned
2-823	A	19:31	22:27	Ozone is one of the more important GHGs in global climate change, both as a diagnostic and as an acive participant. Figure 2.3.8 is useful, but not sufficient since it does not	Rejected. Limited figure space. Earlier IPCCs have illustrated this point.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				illustrate the importance of the vertical distribution of ozone change. The authors should borrow more illustrative figures of ozone change either from Hansen et al. (2002) or Hansen et al. (2005). Ozone radiative forcing has a characteristic dependence on height (e.g., Lacis et al., 1990; Hansen et al., 1997), and the height dependence of the solar and thermal components is also different. The largest ozone RF contribution comes from the tropopause region, which is difficult to monitor accurately either from space or from the ground. Hence it is important to emphasisze accurate knowledge of changes in the vertical distribution of ozone. Ozone also has significant longitudinal variability for both stratospheric and tropospheric ozone, with the longitudinal distribution of tropospheric ozone being significanlty impacted by anthropogenic activity. [Andrew Lacis]	
2-824	A	19:31		Section 2.3.7.1. I did not find in this section an explanation to the statement given in the executive summary saying that RF of stratospheric ozone is expected to decrease in the future if the ozone hole shows signs of recovery. [Philippe Tulkens]	Accepted. Section reworded
2-825	A	19:33		Section 2.3.7.2 comments prepared with the help of Samuel Oltmans (samuel.j.oltmans@noaa.gov) [Anne Thompson]	Noted
2-826	A	19:39	19:39	remove "from" [Graham Feingold]	accepted
2-827	A	19:39	19:40	"from ? Is displayed" source of measurments is missing [Eleanor Highwood]	accepted
2-828	A	19:39	19:39	a word missing or too much in this line [Reto Knutti]	accepted
2-829	A	19:39	19:47	I find this paragraph very unconvincing. I think that this paragraph needs further scrutiny before it is to be an important part of this chapter. [Jerry Mahlman]	Accepted. Text reworded
2-830	A	19:39	19:39	The reference to "ozone anomalies" is missing. [Steven Massie]	accepted
2-831	A	19:39	19:39	PROOFREADING TYPE'COMMENT: current text reads "... Column ozone anomalies from is displayed".. There seems to be missing the source Fioletov et al. (2002)? [Malte Meinshausen]	accepted
2-832	A	19:39	19:39	"ozone anomalies from is dispayed". Something is missing from this sentence - a range of years? [Dylan Millet]	accepted
2-833	A	19:39	19:39	delete "from"	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Rolf Philipona]	
2-834	A	19:39	19:40	Something is missing in the sentence [Eugene Rozanov]	accepted
2-835	A	19:39		Delete: "from" [Michael Coffey]	accepted
2-836	A	19:39		delete "from" after "anomalies" [Hartmut Grassl]	accepted
2-837	A	19:39		delete "from" [Junying Sun]	accepted
2-838	A	19:41	19:41	What does "good agreement" mean quantitatively? [Eleanor Highwood]	quantified
2-839	A	19:42	19:44	I think the Shindell and Faluvegi idea is a plausible suggestion rather than a statement of fact. [Joanna Haigh]	accepted
2-840	A	19:42	19:42	"shown" … "suggested" would be better [Keith Shine]	accepted
2-841	A	19:43	19:43	The increase of 7.2 DU between the first and second half of the 20th century is not well established as is implied. [Stefan Brönnimann]	Accepted - reworded
2-842	A	19:43	19:44	However this can only be correct, if H2O has indeed increased over this time period which is not clear given the discussion in 2.3.8.1 [Rolf Müller]	Accepted - reworded
2-843	A	19:47	19:47	Talking about ozone in the first half of the 20th century, the probably most important feature is the extreme anomaly of total ozone in the early 1940s, which for Arosa was of similar magnitude as the trend between 1975 and 2000 (Brönnimann, S., J. Luterbacher, J. Staehelin and T. M. Svendby (2004) An extreme anomaly in stratospheric ozone over Europe in 1940–1942. Geophys. Res. Lett. 31, L08101, DOI: 10.1029/2004GL019611) [Stefan Brönnimann]	Rejected. Not necessary
2-844	A	19:49	19:55	Recent additional findings are in: Krizan P., J. Lastovicka (2005), Trends in positive and negative ozone laminae in the Northern Hemisphere, J. Geophys. Res., 110, D10107, doi:10.1029/2004JD005477 shows evidence for a trend change around 1997, which is mainly atributed to dynamical changes. [Hugo De Backer]	Accetped. Reference added
2-845	A	19:49	19:55	Recently, Yang E.-S., D. M. Cunnold, M. J. Newchurch, R. J. Salawitch (2005), Change in ozone trends at southern high latitudes, Geophys. Res. Lett., 32, L12812, doi:10.1029/2004GL022296, showed that also some signs of possible recovery at high	Reference not needed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				lattitudes in the southern hemisphere are seen. [Hugo De Backer]	
2-846	A	19:49	19:49	Recommend changing "show lowest value" to "with the lowest values" [Dylan Millet]	accepted
2-847	A	19:49	19:50	This focusses strongly on one of the minima seen figure 2.3.8. Indeed it is the lowest value of the three minima observed by SBUV (blue curve) in figure 2.3.8. For other instruments this might be different, e.g. for the merged series. Anyway, the other two minima, in 1995 and 1997, are equally pronounced so it is a bit strange to strongly focus exclusively only on 1993. [Peter Van Velthoven]	accepted
2-848	A	19:49		correct "show lowest value" to "shows lowest value" [Hartmut Grassl]	accepted
2-849	A	19:54		A rebuttal to the Steinbrecht et al. article by Cunnold et al. (2004) and Steinbrecht's response should be included in the references here. [Derek Cunnold]	accepted
2-850	A	20:1	20:42	This summary of some of the recent research is interesting and informative. In my view, however, it does not seem to connect well with the purpose of Chapter 2, i.e., to provide definitive contributions to the integrated quantification of RF. If a careful rewrite of key part of this section would negate some of these concerns, all of us would benefit. [Jerry Mahlman]	accepted
2-851	A	20:4		The period discussed in WMO (2003) was 1997-2001 not 2000-2003 [Derek Cunnold]	Rejected – true of sroc, not here
2-852	A	20:6	20:8	What is the reason of different seasonality for both hemispheres? [Xiaobin Xu]	Beyond scope of RF discussion
2-853	A	20:8	20:10	The response of total ozone is visible in the observation data (WMO, 2003), but it is shifted by 2-3 years over the Southern Hemisphere. [Eugene Rozanov]	Rejected – incorrect interpretation
2-854	A	20:8		add: "about" before "6% year round" [Hartmut Grassl]	Alrady states this
2-855	A	20:10	20:13	This conclusions is drawn from the experiments with Chemistry-Transport model driven by assimilated circulation, which indirectly includes ozone changes due to chemical processes. Therefore, we cannot conclude that the obtained changes reflects purely dynamical processes. It well could be that mentioned changes of the tropopause height is the result of ozone destruction by volcanic aerosol. [Eugene Rozanov]	Conclusion based on more than this
2-856	A	20:14	20:14	ozone forcing…" -> "stratospheric ozone forcing…	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Xiaobin Xu]	
2-857	A	20:20	20:21	Is "roughly" used deliberately to describe the casual nature of this estimate? It seems rather informal for the rest of the chapter. [Eleanor Highwood]	Accepted. reworded
2-858	A	20:25	20:25	"not known" - does this mean that no-one (even Hansen) has done this calculation? I know my group should have! [Keith Shine]	It does and you should
2-859	A	20:27	20:33	Not sure why this paragraph is needed [Keith Shine]	Accepted paragraph dropped
2-860	A	20:33	20:33	Insert "to" between "dropped" and "close". [Dylan Millet]	accepted
2-861	A	20:35	20:35	Artic -> Arctic [Reto Knutti]	accepted
2-862	A	20:35	20:35	Change "artic" to "arctic" [Brian Magi]	accepted
2-863	A	20:35		Artic to Arctic [Junying Sun]	accepted
2-864	A	20:36	20:36	Perhaps it is worth pointing out here that not only the variability of ozone is stronger in the Arctic but that also the climatological value of Arctic column ozone is much larger (that is for pre-ozone hole conditions) than the value for the Antarctic. [Rolf Müller]	Rejected, detail not needed
2-865	A	20:39	20:39	"lowe temperatures accelerate ozone depletion" - oh no they don't. They may well do in the polar lower stratosphere, but elsewhere, including the non-polar lower stratosphere, they lead to an increase in ozone - see e.g. Zeng and Pyle GRL 2002 [Keith Shine]	Accepted. Text reworded
2-866	A	20:44	20:57	There is much here that is complex, incompletely understood, and quantitatively marginal, relative to the RF-grounded central theme of this chemical-composition dominated section. If this is to be retained, and I suggest that it should be, it almost certainly needs to be shortened with considerable discipline applied. I recognize that this is easier said, than done. However, to preserve the focus and integrity of this Chapter, it needs to be tackled, with the mentality that this is an Assessment for the world to read and understand. Scientific minutiae needs to be carefully filtered out so that this Assessment's critical messages can come out loud and clear. Our much valued speaking to our own colleagues has to be severely curtailed in this Chapter, so that our truly important climate warming messages come out, loud and clear. [Jerry Mahlman]	The RF caused by incraesed tropspheric ozone is abour 25% of the total RF since pre-ind. Times, so we don't agree that this is quantitatively marginal. With a long term future focus it will a smaller fraction of the RF, but this section also serevs as a documentation of current RF for detection and attribution purposes.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-867	A	20:44		Section #2.3.7.2. Grammar and sentence structure quite poor throughout this section. [Dylan Millet]	Accepted.
2-868	A	20:45	20:45	from ozonesondes and surface observations' should be 'between' [Tami Bond]	Text retained. The differences are regional and not systematic between sondes and surface observations.
2-869	A	20:46	20:49	Sentences need to be reworded, "the CTM model results…" is stated before we know which "the" refers to, then in the next sentence we infer this means the OXCOMP results. [Mark Lawrence]	Ok, remove "the"
2-870	A	20:46		I think a new paper of Oltmans et al. has been submitted and it would be valuable to include this paper in the assessment (whenever this is still possible) [Johannes Staehelin]	Accepted
2-871	A	20:48	20:49	If the conclusions of the OXCOMP exercise were interesting then tell the reader what it was and some brief conclusions. [Joanna Haigh]	Section rephrased.
2-872	A	20:48	20:49	A brief summary of the results from the OXCOMP model exercise would be helpful for the readers. [Xiaobin Xu]	Space limitations, must refer to TAR and Gauss et al.
2-873	A	21:1	21:13	Seems a shame not to have some figures showing these trends. [Keith Shine]	Agree, but space is very limited.
2-874	A	21:1	22:26	I miss the connection between the very nice summary of observed trends and the models. The model discussion only deals with preindustrial ozone. However several studies have provided calculated trends in ozone over the last decades. It would be nice to mention these studies and make the link between the observed trends and the calculated trends! Do they agree ? The model section is much very oriented towards the Mickley et al. studies. Not that I dislike this nice study but it should be kept in mind that Mickley used a quite simple model (low resolution, simplified chemistry, ...) to do that and didn't even consider the FDH (please note that you provide almost exclusively references to estimates with the GISS model, all the Mickley, Shindell, even Seinfeld references). To me these studies (and the Mickley in particular) are a bit overemphasized. This section would benefit more from a discussion of the main issues raised by the ACCENT experiments performed by more detailed models including the troposphere and stratosphere. [Didier Hauglustaine]	We don't agree that there are several papers that provide calculated trends. There are some time-slice calculations, but generally the model simulations lack the effect of interannual variability in atmospheric circulation and also the interannual variability in key sources (i.e. biomass burning). Therefore the proposed comparison (which should be done in a proper study), will be hampered by caveats and probably without a clear message. More than 50% of models in fig. 2.3.9 are from ACCENT. RF in Fig. 2.3.9 are scaled to account for FDH in all cases. "This section would benefit more from

No.	Batch	Page:line From	Page:line To	Comment	Notes
					a discussion of the main issues raised by the ACCENT experiments": Agree, more emphasis is made on the ACCENT results.
2-875	A	21:1	22:57	There is much here that is complex, incompletely understood, and quantitatively marginal, relative to the RF-grounded central theme of this chemical-composition dominated section. If this is to be retained, and I suggest that it should be, it almost certainly needs to be shortened with considerable discipline applied. I recognize that this is easier said, than done. However, to preserve the focus and integrity of this Chapter, it needs to be tackled, with the mentality that this is an Assessment for the world to read and understand. Scientific minutiae needs to be carefully filtered out so that this Assessment's critical messages can come out loud and clear. Our much valued speaking to our own colleagues has to be severely curtailed in this Chapter, so that our truly important climate warming messages come out, loud and clear. [Jerry Mahlman]	Trop. Ozone is a major warming agent so the text is retained. Cf. Reply to comment 866.
2-876	A	21:5		The paragraph dealing with published studies of background ozone trends over Europe is not complete and needs clarification. Suggested addendum: …. or slightly negative" in the planetary boundary layer (Naja et al., 2003). However, significant upward trends in ozone were observed during the 1990s at the background site in Mace Head (West coast of Ireland) (Simmonds, et al., Atmos. Environ., 38, 4769-4778, 2004, and Simmonds et al., Atmos. Environ, 39, 2513-2517, 2005) and at the high mountain site of Jungfraujoch (Switzerland) (Broennimann, et al, Atmos. Environ., 36, 2841-2853, 2002). These trends are not well understood keeping in mind the decreasing trend in ozone precursor emissions in Europe and North America. This background ozone increase might have influenced the only small decrease in ozone maxima found in the Swiss plateau (Ordoñez, et al., Atmos. Chem. Phys., 5, 1187-1203, 2005). [Johannes Staehelin]	This paragraph discusses free trop. Ozone changes based on ozone sonde data. Surface trends (eg. Simmonds et al is included in a separate section.
2-877	A	21:9	21:9	after line 9, add: While the trend at Japanese stations over the entire length of the ozonesonde records shows an increase in the ozone, the most recent 15-year period shows little change (Oltmans et al., 2005) despite continued increases in East Asian emissions (Richter et al., 2005). [Anne Thompson]	OK. Will include ref to Oltmans et al.,
2-878	A	21:15	21:15	After first sentence, add (encompasses comments 11-13): The preferred instrumentation is the electrochemical concentration cell (ECC) ozonesonde. Only at Natal, Brazil (6S, 35.5W) have ECC sondes operated regularly for more than 25 years. Since 1998, the SHADOZ (Southern Hemisphere Additional Ozonesondes) network of 12 tropical and subtropical ozonesonde stations has been launching ECC sondes weekly [Thompson et	Too detailed for this kind of assessment.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				al., 2003]. SHADOZ provides data that can be compared to soundings from 1986-1992 at four locations: Ascension, Irene, Natal, American Samoa. [Anne Thompson]	
2-879	A	21:15	21:15	Continuation of previous comment (#11): In the case of Irene (26S, 28E; a suburb of Pretoria, South Africa), a comparison of ozonesonde profiles from 1998-2002 and a series at the same location a decade earlier showed an increase in lower tropospheric ozone, presumably from an increase in regional pollution [Diab et al., 2004]. However, as Diab et al. [2003] note, averages of tropospheric ozone profiles (even within a season), have limited statistical significance. Tropospheric ozone is better classified in terms of characteristic profiles that correlate with synoptic situations. Then it is appropriate to evaluate trends in terms of whether the distribution among 'prototype' profiles changes over time. [Anne Thompson]	Diab et al. document mainly a local/regional increase in the PBL due to urbanization in Pretoria.
2-880	A	21:15	21:15	Continuation of previous comment (#12): Besides the South African data, there is evidence for tropospheric ozone trends in the SHADOZ record. At Ascension, Natal, Samoa, SHADOZ observations since 2000 display much greater frequency of low ozone episodes in the upper troposphere (see TABLE-AT-1) compared to the pre-1992 record. These low-ozone features are broad, suggestive of convective outflow. They maximize between 150 and 300 hPa and are not found at mid-troposphere pressures [Solomon et al., 2005]. Perturbed dynamics is one explanation but for the very lowest ozone layers (< 5 ppbv) chemical factors cannot be discounted. [Anne Thompson]	Too detailed. Discussions of processes can't be included. However, a sentence stating the uncertainty in the simple averaged trends has been included.
2-881	A	21:15	:17	The TOMS instrument is rather insensitive to tropospheric ozone, and it must be questioned if TOMS data should be used to infer trends. I recommend adding after "during 1979-1992" "although tropospheric ozone retrieval from TOMS data is difficult owing to the limited instrument sensitivity". The upward ozone trend over the tropical and sub-tropical Atlantic Ocean inferred by Lelieveld et al. (2004) is consistent with the ozone trend in the tropical upper troposphere inferred from MOZAIC measurements by Bortz and Prather (Bortz, S.E. and M.J. Prather, Ozone, water vapor, and temperature in the upper tropical troposphere: variations over a decade of MOZAIC measurements. Submitted to Geophys. Res. Lett.). These results are in conflict with the zero-trend inferred from TOMS. Note: if you would like to mention the Bortz-Prather study, please ask the authors for permission. [Jos Lelieveld]	Reference to Bortz and Prather is included.
2-882	A	21:19	21:27	There's a lot more to be said about near-surface ozone trends. There are several more "regional background" sites (e.g., Jungfraujoch, Zugspitze). [Stefan Brönnimann]	Surface ozone is an important environmental issue, but the contribution to RF is relatively small.

No.	Batch	Page:line From	Page:line To	Comment	Notes
					An extensive review of surface ozone trends is not possible in this report, however, when we find that the surface trends can represent large scale features and /or trends in the free troposphere it has been included in this assessment. Some additions done, cf. Replies to comments above
2-883	A	21:19	21:27	The results of Parrish et al. [2004] should also be mentioned here. (Parrish et al., JGR, VOL. 109, D23S18, doi:10.1029/2004JD004978, 2004) [Dylan Millet]	The Parrish et al. paper discusses processes explaining the findings in Jaffe et al. (which was already referenced). Although very scientifically interesting, this is too detailed for the current assessment.
2-2700	B	21:20	21:20	delete comma [Olivier Boucher]	OK
2-884	A	21:20	21:20	Jaffe et al. (2003), derived…" -> "Jaffe et al. (2003) derived… [Xiaobin Xu]	OK
2-885	A	21:22	21:22	In the middle of line 22, add: Continuous records from a number of other sites in the United States do not show evidence, however, for significant changes over the United States in the past 15 years (Oltmans et al., 2005). [Anne Thompson]	Accepted
2-886	A	21:23	21:23	after line 23, add: Two other North Atlantic island locations (Izaña and Bermuda) also indicate increased ozone (Oltmans et al., 2005). [Anne Thompson]	Accepted.
2-887	A	21:29	21:29	Should read "The new-generation models include…" [Graham Feingold]	Accepted
2-888	A	21:32	21:32	'STE' is an example of an abbreviation that could usefully be spelled out [Tami Bond]	OK, STE is not longer used.
2-889	A	21:35	2:51	The Stevenson et al model intercomparison paper (JGR, in press, see Chapter 7 for the full reference) really needs to be included here! This is the state of the art amongst these studies. It appears that some of the text is already related to this study, but it is hard to be sure (perhaps the citation was avoided since it was previously only submitted; now it is in press). [Mark Lawrence]	Gauss et al. describes the pre-industrial vs. present ozone experiments from ACCENT (the same models as used in Stevenson et al.) Stevenson et al look at future ozone and is not relevant here.
2-890	A	21:35	21:37	I do not want to over emphasized one of my own studies but in Hauglustaine and Brasseur	OK, Estimate from H&B is added.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				(J. Geophys. Res., 2001) we also provide a new (since TAR) estimate of the FDH tropospheric ozone forcing since pre-industrial times of 0.43 W.m-2 which should be fit nicely in your Figure 2.3.9. [Didier Hauglustaine]	
2-891	A	21:44	21:45	You will notice that in Hauglustaine and Brasseur (2001) we reach a fairly good agreement with pre-industrial ozone measurements without tuning our emissions as was done by Mickley. The main reason is that we used for the first time an estimate of the pre-industrial NO soil emissions. These emissions were calculated by a soil model excluding the effect of fertilizers. Doing this we estimated a NO soil emission lower by a factor of 2 compared to present day emission. The impact on ozone is very important. [Didier Hauglustaine]	Agree, is included in the text
2-892	A	21:44	21:44	If you want to raise this use, it is important to warn the reader that the pre-industrial measurements are very uncertain. Some experimentalists would even argue that these should be called "proxies" instead of real measurements. Tuning the models to reach an agreement with these preindustrial ozone estimates do not really make sense. [Didier Hauglustaine]	We agree that there is a consensus in the literature that ozone "observations" in the 19'th century should not be used for quantitative comparisons with models. This has the implication that the RF from Mickley et al (2001) in their "tuned" case is not included in out central estimate for RF by tropospheric ozone. However, we include this in the uncertainty estimate for trop. Ozone assuming a log-normal uncertainty distribution. The text and figures are changed accordingly.
2-893	A	21:44	21:51	The disccusion of the failure of models to capture the obserrved preindustrial ozone amounts should also state that there is considerable uncertainty as to the quantitative reliability of those data, and that in fact several papers (e.g. Pavelin et al., Atm. Env., 1999) have concluded that the early measurements are not a meaningful quantitative test of models at all. [Drew Shindell]	See comment 892. Conclusion in 892 is based Pavelin et al. (among others).
2-894	A	21:44	22:8	Ozone "observations" of the 19th century should not be used for comparison with numerical simulations. Strong evidence exists that Schoenbein measurements are only semi quantitative showing most probably much too low values as published in the Ozone Assessment 1994 (see N.R.P. Harris et al: Ozone measurements, in Scientific Assessment of Ozone Depletion: 1994, UNEP/WMO, World Meteorol. Organ., Global Ozone Res. and Monit. Project, Report 37, Geneva, 1995). You find on page 1.34: "Ground-based measurements were made during the last (i.e. 19th) century mostly with the Schoenbein	See comment 892

No.	Batch	Page:line From	Page:line To	Comment	Notes
				method (eg. Marenco et al., 1994), which is subject to interferences from wind speed and humidity (Linvill et al., 1980). Kley et al. (1988) concluded that these data are only semi-quantitatiive in nature and should not be used for trend estimates." (comp N.R.P. Harris, et al., J. geophys. Res., 102, 1571-1590 (1997). The measurements of the Montsouris observatory near Paris also show low mean values around 10 ppb. However, the planetary boundary layer measurements include a substantial fraction of very low values (less than 7 ppb, see Fig. 8 on page 84 in J. Staehelin, Atmos. Environ., 28, 75-87 (1994). These data can be hardly viewed as representative for the free troposphere (they might have been caused either by an incomplete removal of values contaminated by SO2 from Paris or by strong dry deposition). [Johannes Staehelin]	
2-895	A	21:48	21:51	One thing I liked about the Hauglustaine and Brasseur (2001) study is that we estimated an upper limit on the tropospheric ozone forcing. In order to do that we used a stratospheric ozone tracer (no photochemical production at all but destruction was possible) for the pre-industrial ozone. Since the uncertainty comes from the estimate of natural emissions, excluding the photochemical production provides a lower limit on ozone. Using these ozone levels we obtain an upper limit of the forcing of 0.77 W.m-2. [Didier Hauglustaine]	This is an upper limit for this particular model. We feel that discussing this requires too much detail, and we thus leave it out.
2-896	A	21:48	21:48	after line 48, add: The 19th century observations need to be viewed (evaluated) on the basis of their geographic representativeness and more primitive measurement techniques (Staehelin et al., 1994). [Anne Thompson]	See comment 892
2-897	A	21:51	21:51	after line 51, add: Observations made in the middle of the 20th century suggest that most of the increase in tropospheric ozone over Europe came in the second half of the 20th century (Staehelin et al., 1994). [Anne Thompson]	The time evolution of ozone in Europe is scientifically interesting, but is too detailed for this assessment. This also depends on how the 19'th century "observations" are interpreted.
2-898	A	21:54	21:56	Please give some justification for reducing the biomass sources by 90% [Eleanor Highwood]	We agree that this a key point, but a discussions is deemed too detailed. Readers should refer to the original papers. Text retained.
2-899	A	22:0	23:	I am very confused about the water anthropogenic vapor forcing. First, am I correct in reading that the only really known trend is that due to water vapor change in the stratosphere due to methane changes? If yes, one has to discuss the anthropogenic versus natural methane and what fraction of which goes to strat. Second, do we really know enough about other large lever arms such as soil moisture changes etc to say what the water vapor forcing really is? Lastly, the error bars in the last figure (the key figure from	Noted, the section rewritten and it is taken into account that it is only the anthropogenic fraction of CH4 that contributes to the strat water vapour RF. In our key figure we will

No.	Batch	Page:line From	Page:line To	Comment	Notes
				this chapter) will have to greatly thought-out again. I do not believe that the chapter has justified the assigned bar and especially the limits. The anthropogenic water vapor issue comes out of nowhere and it really requires some fleshing out with background, etc. [A. R. Ravishankara]	change the name from anthropogenic water vapour to stratospheric water vapour.
2-900	A	22:1	22:2	The discussion on such interaction between atmospheric chemistry and the biogeochemical cycles belongs to Chapter 7. if you want to keep this in this section please note that we recently estimated the pre-industrial levels of biogenic emissions of VOCs with a vegetation model in Lathiere et al., GRL, 2005. [Didier Hauglustaine]	This is just a description of the assumptions behind the ACCENT model simulations. The sentence is shortened and a cross ref. to section 7.4.2. is added.
2-901	A	22:2	22:2	"biogenic HC". Acronym undefined. [Dylan Millet]	Text removed, discussed in chapter 7.
2-902	A	22:6	22:26	It is difficult to follow the reasoning on uncertainty estimates and how you end up with 0.4 +/- 0.15. Could this explanation be improved? [Jan Fuglestvedt]	The mean estimate and the uncertainty range has been changed due to the omission of models tuned to 19'th century ozone observations from the sample of model estimates (cf. comment 892). The text describing how we arrive at the uncertainty estimate has been revised.
2-903	A	22:6	22:6	Change "uncertainties in the estimated RF by tropospheric ozone, originates…" to "uncertainties in the estimated RF by tropospheric ozone orginate…" [Dylan Millet]	OK.
2-904	A	22:7	22:22	remove "of" [Graham Feingold]	Unclear which "of", text retained.
2-905	A	22:8	22:11	This sentence is not well formulated. Suggestion: "In addition, the models that include stratospheric chemistry have modelled a significant reduction in the tropospheric ozone at high latitudes, as a result of decline in the stratospheric ozone, and this affected the range of results. [Xiaobin Xu]	OK
2-906	A	22:8		The sentence of "lack of consistence between observed and calculated preindustrial ozone levels" is very misleading (see above). Either you need to clarify the problems of the quality of the measurements in more detail (see comment above) or (what I recommend) cancel all discussion of the comparison with "observations" completely. This also has requires adjustments in the following paragraph. (p. 22, lines 15-20). [Johannes Staehelin]	Agree, See comment 892
2-907	A	22:9	22:10	write: "ozone at high latitudes as a result of the decline in stratospheric ozone" [Hartmut Grassl]	Text changed according to comment 2-905.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-908	A	22:9	22:11	"In addition ... results", this sentence is incomplete [Ralf Koppmann]	Text changed according to comment 2-905.
2-909	A	22:9		write: "addition models including stratospheric chemistry show" [Hartmut Grassl]	Text changed according to comment 2-905.
2-910	A	22:15	22:16	Again, Mickley et al. were not the only one to indicate that these uncertainties in ozone levels could substantially affect the forcing. This is excatly why we provide an upper estimate of the forcing in Hauglustaine and Brasseur (2001) and a discussion of these uncertainties a well. [Didier Hauglustaine]	Accepted, uncertainty section rephrased.
2-911	A	22:15	22:26	This discussion on uncertainties is a bit loose. The climate feedback of lightning should be discussed in Chapter 7. I found several typos in this section as well. The only reference to Marenco seems odd as well. I do not find the drining ideas in this section and found it unconclusive. [Didier Hauglustaine]	Accepted, uncertainty section rephrased.
2-912	A	22:15	22:26	In this section we also miss a discussion on the RF per DU. We had that in TAR and a figure or a table showing these (in particular in parallel to your fig. 2.3.9) would be interesting. [Didier Hauglustaine]	Mean RF/DU from ACCENT experiments, and for others referred to in figure 2.3.9 that report RF and trop. DU change is included in the text.
2-913	A	22:21	22:21	increase [Graham Feingold]	OK
2-914	A	22:21	22:21	Correct the sentence "An uncertainty whih can increases…" [Ramachandran Srikanthan]	OK
2-915	A	22:21	22:21	…which can increases…" -> "which can increase… [Xiaobin Xu]	OK
2-916	A	22:21		correct: "increases" to "increase" [Hartmut Grassl]	OK
2-917	A	22:24	22:25	The phrase "However, .. than RF." is illegible. [Peter Van Velthoven]	Accepted, text changed.
2-918	A	22:24		add "a" after "to be more" [Hartmut Grassl]	OK
2-919	A	22:25	22:25	"a feedback mechanism", [Graham Feingold]	OK
2-920	A	22:28	23:37	The water vapor section appears to be addressing observed trends that belong in Chapter 3 [Robert E. Dickinson]	Rejected, Chapter 3 trends extensively refered to. However, Section shortened
2-921	A	22:28		No mention at all of other anthropogenic changes in water vapour, such as land use changes, fuel combustion, etc	Section rewoked. Stratospheric water vapour only here

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Vincent Gray]	
2-922	A	22:30		Section 2.3.8. The water vapor concentration changes are looked at only from the anthropogenic sources. The reason for not mentioning the other sources could be specified here. Section 3.4.3 (in Chapter III) covers all observations of water vapor but as Chapter II refers to the radiatively important gases and the overall radiative forcing, both the natural and the anthropogenic sources could be referred to. [Philippe Tulkens]	Accepted. Section reworked and context provided
2-923	A	22:30		Section 2.3.8.1. The paragraph could be rearranged to avoid repetitions. For instance, paragraph from p. 2-22. L. 36 to 42 and p.2-23. L. 9 to 20. seems to repeat each other to some extent. [Philippe Tulkens]	Accepted - reworded
2-924	A	22:30		Section 2.3.8. Chapter III, states that water vapor increased by about 5% over the oceans in the atmosphere (p. 3-26. L. 1-2). However, according to section 2.3.8, the radiative forcing associated with water vapor increase is small. Do these results fully match? Some further explanation (either in Chapter II or in Chapter III) on the increase of water vapor versus the corresponding radiative forcing would be helpful. [Philippe Tulkens]	Section meaning clarified
2-925	A	22:36	22:43	It is important to express the change in stratospheric water vapor in terms of changes in water vapor concentration (ppmv) since this is a more fundamental quantity than changes in W/m2. The computation of radiative forcing by stratospheric water vapor is sensitive to radiative transfer parameterizations commonly used in GCMs (e.g., Oinas et al., 1991), and is also a sensitive function of height (Rind and Lacis, 1993). [Andrew Lacis]	Accepted
2-926	A	22:36	22:42	It seems that this paragraph belongs at the end of section 2.3.8.1. [Dylan Millet]	Partially accepted. Delted paragraphSectiion reworded
2-927	A	22:36	22:53	This dismisses the trend in stratospheric H2O from the SPARC assessment with no justification, with no citation of SPARC, nor of any contradictory work. This is an insult to the large number of authors and referees of the SPARC report [Howard Roscoe]	Only Chapter 3 section 3.2.4.4 now citedSPARC referenced but science has moved on
2-928	A	22:36	22:36	Why no references in this paragraph? [Keith Shine]	Paragraph deletedAccepted – references added
2-929	A	22:39	22:40	Some clarification would be helpful to reconcile this statement with those made on page 2-23, lines 2-3 and page 2-23, lines 19-20 from a conclusion standpoint. Furthermore it is not clear what can be concluded about operations in the upper troposphere versus stratosphere. [Lourdes Maurice]	Accepted – aviation only mentioned once- reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-930	A	22:39	22:39	PROOFREADING TYPE' COMMENT: Current text reads "... with methan changes is estimated by two studies to be ..." Which two studies? [Malte Meinshausen]	Accepted - reworded
2-931	A	22:39	22:39	which two studies are being referred to here? [Joyce Penner]	Accepted
2-932	A	22:39	22:39	Please, put the references to the mentioned two studies. [Eugene Rozanov]	Accepted
2-933	A	22:39	22:39	give references of these two studies [Peter Siegmund]	Accepted
2-934	A	22:39	22:39	Correct the sentence "estimated by two studies to be …" [Ramachandran Srikanthan]	Accepted - reworded
2-935	A	22:39	22:40	What is the reference for this statement? New subsonic aircraft designs are being designed for higher flight altitudes, and for some parts of the year a large fraction of the flights in the northern hemisphere are in the lower stratosphere. I think Baughcum has a couple of articles that show that 20% of the fuel burn from aviation is currently in the lower stratosphere. I think this merits a little more care -- or a clear reference to support dismissing it. [Ian Waitz]	Accepted - reworded
2-936	A	22:39	22:39	Please cite the references of the two studies. [Xiaobin Xu]	Accepted
2-937	A	22:40	22:40	from aviation [Graham Feingold]	Accepted
2-938	A	22:40	22:40	Typo. "form" should be "from" [Patrick Hamill]	Accepted
2-939	A	22:40	22:40	"RF from other proposed mechanisms..." Include a reference or list them. [Patrick Hamill]	Accepted
2-940	A	22:40	22:40	Replace "form" by "from" (spelling error) [Peter Van Velthoven]	Accepted - reworded
2-941	A	22:40		form to from [Junying Sun]	Accepted - reworded
2-942	A	22:47	22:48	What is the indirect evidence? Is this from the fact that the observed cooling is larger than can be accounted for? [Joyce Penner]	No longer mentioned – Section 3.2.4.4 citedAccepted - reworded
2-943	A	22:49		Not obvious why more water vapor in the lower stratosphere is expected to causes cooling, since it absorbs SW and acts as a greenhouse gas. Suggest deleting this sentence, or providing a more useful reference if this is actually correct.	No longer mentioned – Section 3.2.4.4 citedAccepted - reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Roger Davies]	
2-944	A	22:51	22:53	The notion that a blip covering 4 years negates 20 to 40 years of observations is bizarre [Howard Roscoe]	Section 3.2.4.4 cited to expalin thisAccepted - reworded
2-945	A	22:53	22:53	"may not" … maybe "may or may not"! [Keith Shine]	Accepted - reworded
2-946	A	22:53	22:53	The Chipperfield reference is hardly primary literature (and has an incomplete author list, but that is a different matter). [Keith Shine]	Accepted – referenc edeletedreference changed
2-947	A	22:57	22:57	'while' should be inserted after 'as a forcing,…'. Otherwise this is a run-on sentence. [Tami Bond]	Accepted - reworded
2-948	A	22:57	23:1	So which of these different mechanisms would be a forcing and which a feedback? What is the reasoning behind any given choice? [Joyce Penner]	Accepted - reworded
2-949	A	23:0	2:37	CO2 is recognized as the leading cause of RF, yet it receives only 4 pages of treatment and explanation in this cahpter, while aerosol forcing (Section 2.4) receives 4 times as much coverage. It would seem that the development of the different sections of this chapter should parallel one another and that some statements should be made regarding the advances in knowledge since the 3rd assessment, advances in observations, and advances in modeling for the important gases. For example, there is no mention made of the satellite remote sensing that has been accomplished for CO2 [Chedin and coworkers using TOVS and AIRS] or methane [Buchwitz and coworkers using SCIAMACHY] to parallel Section 2.4.3.1, nor is there any mention of the GOSAT and OCO projects under development to address greenhouse gas measurements from space. [Charles Miller]	Partially accepted. CO2 section will be expanded. However, it is not our job to reinforce measurement programs – especially ones that are not flying. Noted... a major cause in the error of total anthropogenic RF is due to uncertainties with the aerosols hence their lenghty treatment cf the LLGHG which are much better known. Satellite observations eg Frankberg 2005 will be added to the methane section.
2-950	A	23:1	23:38	There is much here that is complex, incompletely understood, and quantitatively marginal, relative to the RF-grounded central theme of this chemical-composition dominated section. If this is to be retained, and I suggest that it should be, it almost certainly needs to be shortened with considerable discipline applied. I recognize that this is easier said, than done. However, to preserve the focus and integrity of this Chapter, it needs to be tackled, with the mentality that this is an Assessment for the world to read and understand. Scientific minutiae needs to be carefully filtered out so that this Assessment's critical messages can come out loud and clear. Our much valued speaking to our own colleagues has to be severely curtailed in this Chapter, so that our truly important climate warming messages come out, loud and clear. [Jerry Mahlman]	Shortened as suggested
2-951	A	23:2	23:3	The statement that "Aviation gives a direct RF by emitting water vapour directly into the stratosphere" is true but a bit misleading since trajectory studies (e.g. by Schoeberl and	Section reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
				co-workers) (J. OF GEOPHYSICAL RESEARCH, VOL. 108, NO. D3, 4103, doi:10.1029/2002JD002614, 2003 and J. OF GEOPHYSICAL RESEARCH, VOL. 105, NO. D9, PAGES 11,833–11,840, 2000 and J. OF GEOPHYSICAL RESEARCH, VOL. 103, NO. D9, PAGES 10,817–10,826, 1998) show a very short lifetime for conventional aircraft flying in the lowermost stratosphere. [Steven Baughcum]	
2-952	A	23:2	23:3	"Aviation gives a a direct RF..." But above it says that this effect is insignificant. [Patrick Hamill]	Noted. reworded
2-953	A	23:2	23:3	Some clarification would be helpful to reconcile this statement with those made on page 2-22, lines 39-40 and page 2-23, lines 19-20 from a conclusion standpoint. Furthermore it is not clear what can be concluded about operations in the upper troposphere versus stratosphere. [Lourdes Maurice]	Noted. reworded
2-954	A	23:2	23:2	"aviation" - but aviation was dismissed as a source on page 22 line 40 [Keith Shine]	Noted- reworded
2-955	A	23:3	23:4	Slightly odd that on lines 6 and 7 you talk about "other mechanisms relate to changes in tropoause temperature" which is precisely the mechanism by which volcanic eruptions may impact on stratospheric water vapor - a reader might be confused into thinking it is direct injection of water (and perhaps it might be for some very large eruptions, but not Pinatubo) [Keith Shine]	Noted. reworded
2-956	A	23:9	23:9	wondered whether injection of H20 into upper trop/lower stratosphere by deep convective clouds should be mentioned. [Graham Feingold]	Acccepted
2-957	A	23:11	23:11	used estimated': delete one word [Reto Knutti]	Acccepted
2-958	A	23:11	23:11	"used estimated" needs correction [Andrew Lacis]	Acccepted
2-959	A	23:11	23:11	Correct the sentence "Smith et al. (2001) used …" [Ramachandran Srikanthan]	Acccepted
2-960	A	23:11	23:11	Smith et al. (2001) used estimated a…" -> "Smith et al. (2001) estimated a… [Xiaobin Xu]	Acccepted
2-961	A	23:11		delete: "used" [Hartmut Grassl]	Acccepted
2-962	A	23:11		delete "used"	Acccepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Joanna Haigh]	
2-963	A	23:15	23:15	You are happy to refer to the stratospheric water vapour changes as an RF, when these might more obviously be called a feedback than some of the cloud forcings that you call feedbacks! [Keith Shine]	Noted. Feedbacks/forcing clarified
2-964	A	23:16	23:16	The radiatve forcing estimates for stratospheric water vapor are subject to large uncertainties due to approximations commonly used in GCM radiation codes. For estimating RF for stratospheric water vapor changes based on more precise raditive transfer modeling, Oinas et al. (1991) provide an accurate parameterized formula that gives 0.2 W/m2 forcing for a 1 ppmv change in stratospheric water vapor compared to the 0.29 W/m2 obtained by Forster and Shine (2002). [Andrew Lacis]	Disagree but noted
2-965	A	23:18	22:19	Such a large difference (5-10 times) should be explained or commented. [Eugene Rozanov]	Noted. No longer there
2-966	A	23:18	23:18	"these two estimates" - these two estimates hardly seem to be independent ones, for a point that gets quite some attention [Keith Shine]	Noted. reworded
2-967	A	23:19	23:20	Some clarification would be helpful to reconcile this statement with those made on page 2-22, lines 39-40 and page 2-23, lines 2-3 from a conclusion standpoint. Furthermore it is not clear what can be concluded about operations in the upper troposphere versus stratosphere. [Lourdes Maurice]	Noted. reworded
2-968	A	23:20	23:20	aviation again - now mentioned three times in this section! [Keith Shine]	reworded
2-969	A	23:22	23:37	It may help to further dispel a not rarely encountered confusion by emphasizing that, even though combustion of hydrocarbons, e.g. methane, produces as much as twice as many H2O as CO2 molecules, the resulting H2O flux is a few orders of magnitude less than the natural H2O fluxes - i.e. the atmospheric residence time of H2O is a several days rather than several decades to centuries. [Robert KANDEL]	Rejected, we have already stated in the end of the section the following 'The emission of water vapour from fossil fuel combustion is significantly lower than the emission from changes in land use.'
2-2701	B	23:23	23:37	May I suggest to rewrite as "….The effect of irrigation on surface temperature was dominated by evaporative cooling rather than by the excess greenhouse effect. … The Gordon et al … study ALSO estimates … lower than the emission from CHANGES IN LAND USE." [Olivier Boucher]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-970	A	23:26	23:35	This discussion could be made far more concise. What is the difference between Gordon and Boucher estimates? From the discussion as presented, it is difficult even to understand what the major uncertainties are, which terms sum to provide a net RF, and what the factor of uncertainty is. [Tami Bond]	Accepted, an additional sentence is included to describe the main source of uncertainty.
2-971	A	23:29	23:29	up to 1% [Graham Feingold]	Accepted
2-972	A	23:29	23:29	Change "up to a 1%" to "up to 1%". [Dylan Millet]	Accepted
2-973	A	23:29		delete: "a" before "1%" [Hartmut Grassl]	Accepted
2-974	A	23:39	23:50	This section should be omitted since radiative forcing is not really an observable quantity. Flux changes at TOA and ground surface are of course observable, but they are also subject to major ambiguity since many contributing factors to the radiative fluxes may be undergoing changes that obscure the signal being sought. In part, the topic can be included as part of a comment on closure studies such as that by Turner et al, (2004) for surface observations of longwave fluxes. Remote sensing of GHG gas variability such as methane is possible, and can perhaps be successful, from a satellite platform, but this requires specialized spectral measurements, and requires dedicated radiative transfer modeling. [Andrew Lacis]	Accepted, section moved to ch3
2-975	A	23:41	23:49	This section on long-lived GHG appears sparse and with inadequate frameing. Perhaps is out of place - or not needed? [Robert E. Dickinson]	Accepted, section moved to ch3
2-976	A	23:41	23:49	Should so much be made of the Harries et al and the Philipona et al results ? They go in the right direction, but they relate results from extremely limited samples both in space and in time. [Robert KANDEL]	Accepted, section moved to ch3
2-977	A	23:43	23:43	is experimental evidence [Graham Feingold]	Noted, section moved to ch3
2-978	A	23:47	23:49	I guess, that they finally showed that the obtained increase in LW radiation is local and reflects water vapor increase, which was not homogeneous over the Europe. I do not think these results can be used to illustrate global greenhouse effect [Eugene Rozanov]	Noted, section moved to ch3
2-979	A	23:49	23:49	Please change: …with model calculations … to … with radiative transfer model calculations …	Noted, section moved to ch3

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Rolf Philipona]	
2-980	A	23:49	23:51	Please add this sentence: In an extended study that includes the evolution of temperature and integrated water vapor over Europe (Philipona et al., 2005) they show that 70% of the clear sky longwave radiation increase is due to positive water vapor feedback strongly enhancing greenhouse warming in Central and Northeastern Europe. [Rolf Philipona]	Noted, section moved to ch3
2-981	A	23:51	36:55	2.4. Aerosols. In this chapeter carbonaceous aerosols are divided into organic carbon and black carbon. From the chemical point of view, organic carbon and elemental carbon are often used. However, black carbon and elemental carbon is not equal. The black carbon is usually defined as "optically black carbon", which sometimes include "black organic carbon". Therefore I worry about some confusion among black carbon, elemental carbon, and organic carbon, and I hope to add some descriptions on this matter, if possible. [Tadahiro Hayasaka]	Noted, organic carbon absorption mentioned 2.4.5.2
2-982	A	23:51	42:8	The comments will concern more specifically the section on "Aerosols" [Savitri GARIVAIT]	Noted
2-983	A	23:51	42:8	All aspects related to aerosols and their role in RF have been included. Lack of reliable data are still obvious, especially for Asian countries, regarding biomass burning or acid deposition for example. In order to compendate this insufficience, it is suggested to set a section on concluding remarks and future research priorities, e.g. ground observation in parallel with remote sensing to cross check information. [Savitri GARIVAIT]	Noted, IPCC does not provide research priorities
2-984	A	23:51	42:8	This part is well documented and constructed. Many important findings and progresses, and also uncertainties have been included. The extreme complexity of aerosol direct and indirect effect on climate change leads the list of gaps in our understanding of climate change. It should be noted that many available results listed in this part are from case observational studies or modeling studies. The lack of available aerosol data in global scale and long time period might greatly limit our studies. In addition, it should be pointed out that the quite different physical processes and parameterizations of cloud formation used in the current climate models might lead to very large differences in RF. Although the further determination of these processes by observation is necessary, the completeness and verification of cloud physical processes in current climate models are important since many models still use prescribed or ideal cloud processes. The effect of different precipitation types on aerosol removal by wet deposition process should be assessed since this process might lead to seasonable and geographical changes of aerosol effect. [Xueliang Guo]	Noted, Different precipitation types are taken into account in models, (snow,large scale, convective precip)

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-985	A	23:51		Section 2.4. This is the topic which I know the best, and so most of my scientific comments will be made on the contents here. Frankly, I found this section quite disappointing. I believe that substantial progress has been made since the TAR, as the authors assert. The extensive field and satellite observations are summarized. But very little of the progress is portrayed when it comes to estimating RF of individual species or total aerosol. A preliminary table of uncertainties and their sources appeared in TAR. One would expect this table to be filled in after five years of extensive research. Instead, uncertainties are mainly handled by hand-waving, postulating but not confirming differences between models. These uncertainties were already present in TAR. [Tami Bond]	Noted, we give now the standard deviation rather than just a range.
2-986	A	23:51		Section 2.4 is too detailed. The detailed Tables 2.4.1 to 2.4.6 should be published in papers, but not in AR4. This also applies to Figs. 2.4.3. and 2.4.4. [Peter Siegmund]	Rejected, many positive comments about the tables. Aerosol forcing's importance demands this material.
2-987	A	24:1	24:1	The direct effect is the mechanism…' The direct effect is not a mechanism. It is the name given to the physical phenomenon you describe in this paragraph. [Tami Bond]	Rejected, disagree - it is a physical mechanismn.
2-988	A	24:1	24:39	The "semi-direct effect" is shown in Figure 2.4.1, but not described in text. [Hongbin Yu]	Accepted, sentenece added.
2-989	A	24:6	24:6	The scattering phase function and the asymmetry factor do not provide the same amount of information. The "or" between these two properties is misleading. [Alcide di Sarra]	Accepted, sentence will clarified by dropping the reference to the asymmetry factor as this is not referred to later in the text
2-990	A	24:7	24:7	… in the horizontal and vertical (include "coordinates" or "direction" or anything similar) which varies … [MARCOS S. P. GOMES]	Rejected
2-991	A	24:8	24:10	In the text scattering and absorbing aerosols are mentioned. Absorbing aerosols as such (only absorbing) do not exist. The term absorbing aerosol is jargon. Therefore the text is not correct. There aerosols that predominantly scatter and aerosols that have an important absorption component. Text shall be corrected to reflect this. [Pepijn Veefkind]	Accepted: "partially" inserted.
2-992	A	24:11	24:11	…desert, snow/ice or if the aerosol is above cloud…' Remove 'or if the aerosol is above'; the meaning is the same. [Tami Bond]	Rejected, cloud is not a surface
2-993	A	24:12	24:13	The emphasis in this chapter is upon radiative forcing at the top of the atmosphere, abbreviated as 'RF'. It should be noted that for absorbing aerosols like mineral dust and black carbon, the radiative forcing at the surface is substantially larger in magnitude than the TOA value (e.g. Miller and Tegen JAS 1998, Miller et al JGR 2004, Wang JGR	Noted, see section 2.9.5 and changes will be made to 2.4.1. Reference to Miller et al 2004 will now be included in mineral dust section.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				2005). Thus, radiative forcing at TOA is an incomplete measure of the effect of absorbing aerosols upon evaporation and the hydrologic cycle. Miller, R. L., and I. Tegen (1998), Climate response to soil dust aerosols, J. Clim., 11, 3247-3267. Miller, R. L., I. Tegen, and J. Perlwitz (2004), Surface radiative forcing by soil dust aerosols and the hydrologic cycle, J. Geophys. Res., 109, D04203, doi:10.1029/2003JD004085. Wang C. (2004), A modeling study on the climate impacts of black carbon aerosols, J. Geophys. Res., 109, D03106, doi:10.1029/2003JD004084. [Ron Miller]	
2-994	A	24:12	24:12	SHORTWAVE irradiance [Keith Shine]	Accepted
2-995	A	24:13	24:15	Define 'significant magnitude', 'large', and 'substantial concentrations'. [Tami Bond]	Rejected, this is an introduction.
2-996	A	24:15	24:15	Mutiple scattering by clouds and aerosols at thermal wavelengths is typically ignored in GCM radiative calculations. Dufresne et al. (2002) show that multiple scattering by dust makes a significant (30%) enhancement to the dust greenhouse effect. [Andrew Lacis]	Rejected, this is an introduction.
2-997	A	24:17	24:17	Same comment as direct effect, above. Indirect effects are the label given to a set of physical phenomena that involve aerosol-cloud interactions. [Tami Bond]	Rejected, we disagree. The series of indvidual mechanismns in figure 2.4.1 make up the indirect effect
2-998	A	24:17	24:36	I would recommend to include in this explanatory paragraph a short description of the semi-direct effect. [Alcide di Sarra]	Noted, this is re-directed to and described in Chapter 7
2-999	A	24:19	24:20	effectiveness… is a function of the… geographic distribution'. Effectiveness is a function of the environment. Geographic distribution determines the environment, but is only an indirect cause. [Tami Bond]	Accepted: inserted "Ambient environment"
2-1000	A	24:20	24:20	It is not clear what is intended with "geographical distribution". Differences in the source properties are already accounted for by the other properties. [Alcide di Sarra]	Accepted: inserted "Ambient environment" instead of geographical distribution.
2-1001	A	24:24	24:26	I don't understand how the term "cloud albedo effect" is more representative of the microphysical processes that occur when anthropogenic aerosols interact with clouds. You can change the cloud albedo by changing the liquid water path in the cloud, but this is not included in calculations that fix the liquid water content of the cloud. I also think "change in precipitation efficiency" is a better descriptor of what is commonly referred to as the 2nd indirect effect. This is because these 2nd effects include change in liquid water	Noted, definitions which are kept across chapters have been suggested by chapter 7 (Ulrike Lohmann). We stick with this definition but will seek ways to improve the text here.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				path, change in cloud fraction, and change in cloud height as well as the change in cloud lifetime. Unless you mean to exclude these other indirect effects. [Joyce Penner]	
2-1002	A	24:26	24:33	I still don't get this feedback/forcing distinction. On the basis that you can calculated the Twomey effect diagnostically, and therefore it is a forcing, I could argue, therefore, that all the radiative forcing due to ozone is a feedback, as I certainly couldnt do that diagnostically. I think you should revisit this - my suggestion is that you distinguish between feedbacks and forcing on the grounds that some are mediated by surface temperature change (which I would call a feedback) and some go on regardless of surface temperature change. I think this is cleaner, and more in tune with classical feedback analyses. [Keith Shine]	Noted, A clearer description of the definition of what falls into the "radiative forcing" category and what not will be included in the early part of chapter 2. However, note that this text refers to the TAR. Note feedback/s can occur without Tsfc changing.
2-1003	A	24:32		It is necessary to introduce Figure 2.4.1 ealier, i.e., in the beginning of section 2.4.1. [Hongbin Yu]	Accepted, and moved.
2-1004	A	24:33	24:33	Feedback mechanisms do occur…' It would be useful to have 'feedback' defined at this point. [Tami Bond]	Noted, A clearer description of the definition of what falls into the "radiative forcing" category and what not will be included in the early part of chapter 2.
2-1005	A	24:33	24:33	Necessarily is a poor word, how about invariably [Robert Levy]	Accepted.
2-1006	A	24:38	24:38	Increse the font size within figure 2.4.1 [MARCOS S. P. GOMES]	Accepted.
2-1007	A	24:40	24:40	"Advances" since the TAR. Replace the loaded word "advances" with the neutral word "developments". This is an assessment, not a scientific promotional piece. [Theodore Anderson]	Accepted.
2-1008	A	24:40	25:6	Advance in theoretical/conceptual understanding should be added in Section 2.4.2. For example, it has been shown that increasing aerosols enhance not just cloud droplet concentration but also relative dispersion because of competion for available water etc, and the enhanced dispersion excerts a warming indirect aerosol effect (Liu and Daum, Nature, 419, 580-581, 2002; Peng and Lohmann, Geophys. Res. Lett., 30, DOI10.1029/2003GL017192. 2003; Rotstayn and Liu, J. Climate, 16, 3476-3481, 2003; Wood, J. Atmos. Sci., 59, 2681-2693; Liu et al, An analytical expression for relative dispersion of the cloud droplet size distribution, Geophys. Res. Lett., revised, 2005). Therefore, the concept of the first and second indirect aerosol effect should be changed from the number effect only to number effect plus dispersion effect. [Yangang Liu]	Noted, section 2.4.2 has been removed because of space constraints.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1009	A	24:40	27:50	There have been major advances in aerosol measurement, esp from satellites. However, it is not clear that these research advances have had time to move into an 'operational' phase, which is required for climate change purposes. Moreover, we need to recognise that we'll need a better understanding of aerosol composition in the future. I think that the section could add a note of caution, as well as herald the achievements. [Michael Manton]	Noted, section 2.4.2 has been removed because of space constraints.
2-1010	A	24:42	25:5	The list is confusing. The title is Advances since the TAR, but the list contains descriptions that are not really advances but rather increases in the amount of observations. The list should be broken up into 1) real advances in understanding aerosol effects and 2) increases in observational capability. This list should include quantitative advances pulled from the sections following it, instead it contains mostly vague statements. [Ellsworth Welton]	Noted, section 2.4.2 has been removed because of space constraints.
2-1011	A	24:46	24:54	Are these necessarily "advances?" or are we just adding MORE, MORE, MORE observations? There is no mention of "validation" to any of these observations [Robert Levy]	Noted, section 2.4.2 has been removed because of space constraints.
2-1012	A	24:46	24:54	The following advances should be added: (1) Improved satellite instruments better capable of making aerosol observation, (2) Increased number of operational sunphotometer measurements (e.g. AERONET) [Pepijn Veefkind]	Noted, section 2.4.2 has been removed because of space constraints.
2-1013	A	24:46	24:54	(1) Need to add some details about the advances; (2) It is not evident from the report what observational advances have been achieved for emissions and trends; (3) A space-borne lidar. i.e., GLAS, is already acquiring cloud and aerosol profiles. It is missing here; (4) Should add improved retrievals of surface reflectance (wavelength dependence and anisotropy) from satellite instruments. Recent studies have shown that surface albedo has a visible effect on the aerosol RF and an adequate characterization of the wavelength dependence and anisotropy of surface reflection is necessary. This part has been totally left out in this report. If we don't get surface albedo right (presumably easier than the characterization of aerosol properties), how can we get the RF right and explain satellite-model or model-model discrepancies? [Hongbin Yu]	Noted, section 2.4.2 has been removed because of space constraints.
2-1014	A	24:46	25:5	This part should be better narrated. As my colleague said, this part reads like his "weekly progress report" to his employee. [Mian Chin]	Noted, section 2.4.2 has been removed because of space constraints.
2-1015	A	24:46	25:5	Suggetsed additions to the list with start on page 46, line 46: • Recent advances concluding that it is the properties, including morpology and state of mixture, of the individual airborne particles at a given size or within a given size range	Noted, section 2.4.2 has been removed because of space constraints.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				which are of importance of the model estimates on radiative forcing and not just some integral or average property over a large number of particles as would be determined by bulk analysis. • Advances in the understanding the properties of the biogenic/organic aerosol and its relative importance to the inorganic aerosol. • A greater appreciation of the importance of aerosol vertical profiles- both improvements on in-situ observations and on modelling. [Caroline Leck]	
2-1016	A	24:46	25:5	There is no mention of observation/observation or observation/modeling SYNERGY! [Robert Levy]	Noted, section 2.4.2 has been removed because of space constraints.
2-1017	A	24:47	24:47	Change "via dedicated retrievals" to "via advanced instrumentations and dedicated retrievals". [Mian Chin]	Noted, section 2.4.2 has been removed because of space constraints.
2-1018	A	24:47	24:47	Improved accuracy is in contract to what is written in the middle of page 2-26 [Robert Levy]	Noted, section 2.4.2 has been removed because of space constraints.
2-1019	A	24:47	24:47	This statement is in contrast with parts later in the chapter. (see following comments) [Felicita Russo]	Noted, section 2.4.2 has been removed because of space constraints.
2-1020	A	24:47	24:47	This list bullet contradicts the statement made on page 26, line 24. The summary list of advances here should match the text in the rest of the section. [Ellsworth Welton]	Noted, section 2.4.2 has been removed because of space constraints.
2-1021	A	24:48	24:48	"Longer satellite records" does not mean advances or improvements. Of course as time goes by the records get longer. Delete this bullet. [Mian Chin]	Noted, section 2.4.2 has been removed because of space constraints.
2-1022	A	24:48	24:48	This is an example of comment #1. Longer satellite records does not constitute an advance in understanding, just the natural progression of time. [Ellsworth Welton]	Noted, section 2.4.2 has been removed because of space constraints.
2-1023	A	24:51	24:51	"Further work on emissions and trends" – What are the advancements there? [Mian Chin]	Noted, section 2.4.2 has been removed because of space constraints.
2-1024	A	24:51	24:51	The text does not seem to go into a discussion of the improvements in estimates of aerosol emissions. A really critical factor to be considering that does not seem to be mentioned is the issue of the changes in height of emission of SO2 during the 20th century. Early in the 20th century, most of the SO2 that led to sulfates was emitted near the surface (from homes, factories, etc.), so below most of the water vapor and mixed with other ashy components of the aerosol that would be absorbing; at this level, the sulfate lifetime was quite short and the reflective efficiency of the aerosols was likely quite low. By mid-century, most of the SO2 emitted in North America and Europe was being emitted from	Noted, However, the AeroCom emission inventory accounts for present day SO2 effective vertical injection height for industry, biomass burning. Information added in modeling section 2.4.54. Emission trends specifically included 2.29 for sulphate, 2.30 organic carbon, 2.31 black carbon.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				tall stacks that led to the sulfates forming up in the troposphere (above the boundary layer) and the typical lifetime of the aerosol particles was extended from a couple of days to a week or two--with dispersion taking the aerosols over much more extensive areas of the Northern Hemisphere (and extending the deposition by acid rain). One would hope that the improvements to the emissions inventories and the model treatment of sulfates was being improved to account for this change. That there is apparently no mention of this aspect in this or earlier IPCC assessments is unfortunate--it should perhaps be mentioned as a shortcoming unless some account of this is being taken. [Note: The comment is made at this point because it is not clear where in this section mention of this should be made--there does not appear to be a section on emission trends.] [Michael MacCracken]	
2-1025	A	24:52	24:52	This item of the list is an example of the vague descriptions given in this section. "More focus on aerosol optical parameters" does not convey any quantitative information. It does not mean any advance has occurred, just more attention was given to an area of research. [Ellsworth Welton]	Noted, section 2.4.2 has been removed because of space constraints.
2-1026	A	24:56	24:56	Most models do not explcitetly contain nitrogen aerosols (nitrite, ammonium). Hence I do not agree with the statement that all major aerosol species are contained in improved models. [Christiane Textor]	Noted, section 2.4.2 has been removed because of space constraints.
2-1027	A	25:0		Figure 2.4.2. There is a discussion in the text about being able to see continental pollution in the figure. However, one cannot see this pollution because there are many white boxes (indicating field stations) over North America and Europe. [Scot Martin]	Noted, However the purpose of the figure is to show also major observation site locations.
2-1028	A	25:1	25:1	suggest "hygroscopicity and absorption of aerosol mixtures" be changed to "hygroscopicity, phase transitions, and absorptions of aerosol mixtures" in relation to longer comment on chapter 2, page 30, start line 24 (see below) [Scot Martin]	Noted, section 2.4.2 has been removed because of space constraints.
2-1029	A	25:1	25:1	I do not agree with this statement. Despite improvemements since TAR, these parameters are exactly those showing very large uncertainty as shown by the AeroCom inititiative. AeroCom has shown that a) the parameterizations for hygroscopic growth and the water content is one of the major uncertainties in aerosol modelling (Textor et al.: Analysis and quantification of the diversities of aerosol life cycles within AeroCom, Atmos. Chem. Phys. Discuss., 5, 8331-8420, 2005), and b) that diverging simulated compositions lead to large uncertainty in absorption (Kinne et al.: An AeroCom initial assessment – optical properties in aerosol component modules of global models, Atmos. Chem. Phys. Discuss., 5, 8285-8330, 2005) [Christiane Textor]	Noted, section 2.4.2 has been removed because of space constraints.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1030	A	25:2	25:2	What is the evidence that modeling of aerosol vertical profiles has improved? The discrepancy in modeled vertical profiles in AEROCOM-A is just as much, if not more, as those in IPCC 2001. [Mian Chin]	Noted, section 2.4.2 has been removed because of space constraints.
2-1031	A	25:2	25:2	AeroCom has shown that vertical profiles differ considerably among model (Textor et al.: Analysis and quantification of the diversities of aerosol life cycles within AeroCom, Atmos. Chem. Phys. Discuss., 5, 8331-8420, 2005). Model validation of the vertical distributions is difficult due to the lack of global data, it is restricted to LIDAR profiles (e.g., Guibert et al.: The vertical distribution of aerosol over Europe—synthesis of one year of EARLINET aerosol lidar measurements and aerosol transport modeling with LMDzT-INCA, Atmospheric Environment 39, 2933-2943, 2005.) [Christiane Textor]	Noted, section 2.4.2 has been removed because of space constraints.
2-1032	A	25:3	25:3	This statementcould be joined with that on page 24, line 56. In addition, I think it is too positive, since only one of the 16 AeroCom models contains NO3. [Christiane Textor]	Noted, section 2.4.2 has been removed because of space constraints.
2-1033	A	25:4	25:5	These terms need to be defined clearly here, and their significance needs to be highlighted and explained in a straightforward manner. [Jerry Mahlman]	Noted, section 2.4.2 has been removed because of space constraints.
2-1034	A	25:6	25:6	Significant progress has been made in the laboratory community, too. To complement the bullet points of "observations" and "modeling", I suggest to insert a follow-on section on "laboratory findings" with the following bullet points: Laboratory findings: - deliquescence and efflorsecence parametrizations of mixed aerosol species (both inorganic and organic) - reactive oxidative (OH, O3, NO3) aging of organic particles to alter hygroscopic and CCN properties - efficacy of different types of heterogeneous ice nuclei - yields, chemical species, and hygroscopic properties of biogenic secondary organic aerosol - physical properties of aerosol chemical species (optical constants, surface tension, water activity, and so on) - cloud droplet activation, e.g., theoretical advances of modified Kohler coupled to laboratory quantification and validation [Scot Martin]	Noted, section 2.4.2 has been removed because of space constraints.
2-1035	A	25:7	25:7	"Advances" in observations. Replace the loaded word "advances" with the neutral word "developments". This is an assessment, not a scientific promotional piece. [Theodore Anderson]	Accepted, but section 2.4.2 has been removed due to space constraints.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1036	A	25:7	25:21	It would be best to start with aerosols that have the best observational record avaialble since that sets the tone both for what is possible to measure and also what information is needed to compute accurate radiative forcing. Of all atmospheric aerosols, the volcanic stratospheric aerosol is the most accurately measured, at least in terms of radiative parameters that are needed to calculate accurate radiative forcing. Based on SAGE II and ground based measurements, Hansen et al. (2002) show the decadal trend of stratospheric aerosol optical depth and particle size. Radiative forcing by Pinatubo aerosols is shown in Hansen et al. (1996), also in Hansen et al. (1992) which successfully modeled and predicted the climate response to the Pinatubo volcanic eruption - which remains the best documented radiative forcing climate experiment to date. [Andrew Lacis]	Noted. Chapter discussion under 2.7.2 includes volcanic aerosol.
2-1037	A	25:7	25:7	Should this section be called "Advances in Observational Capability?" [Robert Levy]	Accepted, changed to obs developments
2-1038	A	25:7	26:32	There is very little discussion and/or references to remote retrievals of aerosol size distributions properties. Although the subject has been only recently addressed in global satellite studies (and as such there is little material available), this fact should be mentioned since size distribution information is a key parameters not only RF computations but also for validation of model predictions. [Santiago Gassó]	Rejected, information in text and table 2.4.1 is comprehensive enough in view of the current accuracy of this type of retrieved aerosol size info.
2-1039	A	25:7		Section 2.4.3. For comprehension, it would be useful to have a small table containing the available satellite measurements and retrieved parameters. Some abbreviations (instrument names) appear in the text and are never expanded nor identified as satellites. This might be forgivable if they appeared in a table. [Tami Bond]	Noted, see table 2.4.1
2-1040	A	25:9	25:9	Near global coverage of what? AOD? [Tim Bates]	Accepted, sentence changed for clarity.
2-1041	A	25:12	25:14	"Further detailed ..." There is a problem with the syntax of this sentence. Suggest re-writing for clarity. [Patrick Hamill]	Accepted, sentence changed for clarity.
2-1042	A	25:12		delete: "aerosol" [Hartmut Grassl]	Accepted, sentence changed for clarity.
2-1043	A	25:13	25:13	Please qualify that this is a satellite measurement based assessment as opposed to a an in-situ measurement based assessment. The in-situ measurement based assessment is described in Bates et al. 2005 (ACPD). [Tim Bates]	Accepted, sentence changed for clarity.
2-1044	A	25:16	27:2	The satelllite retrieval section 2.4.3.1 only covers the aerosol optical depth. It doesn't cover retrievals of the single scattering albedo, as available for example from TOMS.	Rejected, absorption measurements from satellite are not considered

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Because of the importance of aerosol absorption for the diirect forcing, a section shall be added that covers absorption measurements from satellites. [Pepijn Veefkind]	reliable.
2-1045	A	25:17	25:20	This paragraph/sentence greatly exaggerates the status and utility of satellite-based aerosol observations. Replace with: "Satellite retrievals of aerosol optical depth have been improved via new-generation sensors (Kaufman et al., 2002) and an expanded global validation program (Holben et al., 2001). Advanced aerosol retrieval products such as aerosol fine-mode fraction and effective particle radius have been developed and offer great potential for improving knowledge of direct forcing by anthropogenic aerosols (see section 2.4.5), as discussed by Kaufman et al. (2002) and implemented by Bellouin et al. (2005). However, validation programs for these advanced products have yet to be developed and some initial assessments indicate systematic errors (Levy et al., 2003; Chu et al., 2005; Anderson et al., 2005b). References (in alphabetical order, excluding Kaufman which was already cited): Anderson, T. L., Y. Wu, D. A. Chu, B. Schmid, J. Redemann and O. Dubovik (2005b) Testing the MODIS satellite retrieval of aerosol fine-mode fraction, J. Geophys. Res., 110, doi:10.1029/2005JD005978. Bellouin, N., O. Boucher, J. Haywood and S. Reddy (2005) Global estimate of aerosol direct radiative forcing from satellite measurements, Nature, in press. Chu, D. A., L. A. Remer, Y. J. Kaufman, B. Schmid, J. Redemann, K. Knobelspiesse, J.-D. Chern, J. Livingston, P. Russell, X. Xiong and W. Ridgway (2005) Evaluation of aerosol properties over ocean from Moderate Resolution Imaging Spectroradiometer (MODIS) during ACE-Asia, J. Geophys. Res., 110, in press, doi:10.1029/2004JD005208. Holben, B. N., D. Tanré, A. Smirnov, T. F. Eck., I. Slutsker, N. Abuhasan, W. W. Newcomb, J. S. Schafer, B. Chatenet, F. Lavenu, Y. J. Kaufman, J. Vande Castle, A. Setzer, B. Markham, D. Clark, R. Frouin, R. Halthore, A. Karneli, N. T. O'Neill, C. Pietras, R. T. Pinker, K. Voss and G. Zibordi (2001) An emerging ground-based aerosol climatology: Aerosol optical depth from AERONET, J. Geophys. Res., 106, 12067-12097. Levy, R. C., L. A. Remer, D. Tanré, Y. J. Kaufman, C. Ichoku, B. N. Holben, J. M. Livingston, P. B. Russell and H. Maring (2003) Evaluation of the Moderate-Resolution Imaging Spectroradiometer (MODIS) retrievals of dust aerosol over the ocean during PRIDE, J. Geophys. Res., 108, (D19), 8594, doi:10.1029/2002JD002460. [Theodore Anderson]	Accepted. With minor modification.
2-1046	A	25:17	25:20	I would maintain that satellite retrievals are invaluable for constraining AOD and DRE but their ability to differentiate between natural and anthropogenic aerosol and hence	Accepted, sentence changed. "the

No.	Batch	Page:line From	Page:line To	Comment	Notes
				constrain RF are still very limited. [Tim Bates]	routine differentiation between natural and anthropogenic aerosols from satellite retrievals remains very challenging."
2-1047	A	25:17	25:18	Dierect radiative effect of aerososl does not really belong in the same category as observations. It is clearly a modeling results subject to numerous unsubtantiated modeling assumptions and approximations. [Andrew Lacis]	Rejected, Viewed this way, AOD would be a modelling result as well.
2-1048	A	25:17	25:17	Satellites have also tried to determine size distribution (aka MODIS) and SSA (TOMS) [Robert Levy]	Accepted, in part, sentence changed. Absorption retireval from satellites are not reliable.
2-1049	A	25:17	25:20	Has this "constraining" been accomplished? If so, what are the conclusions concerning its success, or lack thereof? [Jerry Mahlman]	Noted, new synthesis section added.
2-1050	A	25:17		change "Angstrom coefficient" to "Angstrom exponent". They are different! [Hongbin Yu]	Accepted.
2-1051	A	25:23	25:23	"over both land and ocean" – there are large blank areas over land in Figure 2.4.2. Explanations are needed here. [Mian Chin]	Noted, mentioned in table 2.4.1 and page 25 lines 35-37
2-1052	A	25:24		My package was missing Table 2.4, and Tables for the rest of Chapter 2; the figures were fine. [Jerry Mahlman]	Noted, Sorry
2-1053	A	25:28	25:28	The MODIS instrument only retrieves optical depths during cloud free conditions. This should be perhaps included in the text. This should be recognized since aerosols are also present during cloud presence. [Richard Fernandes]	Noted, stated explicitely already in section, new sentence
2-1055	A	25:29	25:29	Change "biomass-burning" to "biomass burning" [Brian Magi]	Accepted.
2-1056	A	25:34	25:34	what is the source of industrial dust? [Ron Miller]	Noted, see section 2.4.5.6
2-1057	A	25:35	25:37	It is perhaps also of note that MODIS estimates of AOD are conditional on assumptions regarding single scattering albedo and particle size distribution of aerosols as well as vertical profiles of aerosols. This leads to substantial uncertainties in aerosol retrievals in a relative sense although not in an absolute sense. I note that this is discussed in the next paragraph. Perhaps it is appropriate to keep comments on uncertainties/limits in retrieval methods together.	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Richard Fernandes]	
2-1059	A	25:35	25:36	"There are several regions where the MODIS instrument cannot perform retrievals; …" I suggest to replace this with "The MODIS aerosol algorithm is currently unable to make retrievals over highly reflective surfaces such as deserts, snow cover, ice, and ocean glint, as well as over high latitude regions when the solar insolation is insufficient." The reason I suggest this is that the MODIS instrument itself is rich in data resources, and can probably allow retrievals over some of those areas when fully explored. In fact, I am aware of ongoing research to develop some new plug-ins to the MODIS aerosol algorithm to enable retrieval over some of those surfaces (particularly over deserts). If you want to mention the ongoing research, you may cite: Hsu, N. C., S. C. Tsay, M. D. King, and J. R. Herman, 2004: Aerosol properties over bright-reflecting source regions. IEEE Trans. Geosci. Remote Sens., 42, 557-569. [Charles Ichoku]	Accepted. We do not reference the Hsu paper because, while very interesting, it is not relevant to the figure that is shown and our space constraints are tight.
2-1060	A	25:35	25:37	It might be noted that highly reflective regions prohibiting retrieval by MODIS include deserts where the dust load can be large. [Ron Miller]	Accepted, Explicitely stated page 25, line35-37
2-1061	A	25:39	25:39	The TOMS instrument is followed up by the OMI instrument that was launched in July 2004 on the NASA EOS-Aura satellite. This should be added (maybe as a footnote) to the table 2.4.1 [Pepijn Veefkind]	Accepted. Footnote inserted.
2-1062	A	25:41	25:41	I suggest redo this figure. See my comments on figures at the end. [Mian Chin]	Noted, It is difficult with our space constraints to simply re-do the figures. We have decided that they are a little too busy though and will remove the lettering denoting the measurement campaigns and replace them with stars.
2-1063	A	25:43	25:55	A description of SeaWiFS aerosol product should also be added on Page 2-25, Lines 43-55. [Xuepeng Zhao]	Noted..
2-1064	A	25:43	26:30	The description of satellite retrieval of aerosol optical depth should include recent results showing how a proper selection of viewing geometry can significantly improve (by a factor of 2 to 3) the accuracy of the satellite retrieval of aerosol optical depth (P. Chylek, B. Henderson, M. Mishchenko: Aerosol radiative forcing and the accuracy of satellite aerosol optical depth retrieval, J Geophys. Res., 108, doi:10.1029/2003JD004044, 2003). For example the MODIS accuracy of aerosol optical depth retrieval changes from about 0.12 to 0.04 with the change of the scattering angle from 160 to 100 degrees (P. Chylek, B. Henderson and G. Lesins: Aerosol optical depth retrieval over the NASA Stennis	Accepted. The first of these refrences is now included.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Space Center: MTI, MODIS and AERONET, IEEE Transactions on Geosciences and Remote Sensing, 43, 1978-1983, 2005). [Petr Chylek]	
2-1065	A	25:43		An important reference here is Husar et al 1997 [Yoram Kaufman]	Accepted.
2-1066	A	25:48	25:48	OCTS is described amongst instruments that have delivered long-term datasets. OCTS doesn't have a long-term record, and therefore should be removed from this paragraph. [Pepijn Veefkind]	Accepted. OCTS is now included in the later paragraph.
2-1067	A	25:49		"A" is not defined. [Hongbin Yu]	Noted, we have strict space constraints. Interested readers will have to go to the reference quoted.
2-1068	A	25:50	25:52	one limitation for TOMS is its very large footprint. Please add. [Hongbin Yu]	Accepted.
2-1069	A	25:52	25:55	It might be helpful to include a figure from Geogdzhayev et al. (2002) showing the decadal variability (or lack thereof) of maritime aerosols derived from AVHHR measurements. [Andrew Lacis]	Rejected, this was included in an earlier draft, but is really only relevant for the volcanic episodic optical depths. Unfortunately the space constraints are really tight!
2-1070	A	25:53	25:53	remove "from those" [Graham Feingold]	ACCEPTED
2-1071	A	25:53	25:54	… compared to those (remove "from those") from dedicated satellite … [MARCOS S. P. GOMES]	ACCEPTED
2-1072	A	25:53	25:54	delete one 'those from' [Reto Knutti]	ACCEPTED
2-1073	A	25:53	25:53	Change "those from those" to "those" [Brian Magi]	ACCEPTED
2-1074	A	25:53	25:53	Omit "from those" (syntax error) [Peter Van Velthoven]	ACCEPTED
2-1075	A	25:54		delete: "from those" at the end of the line [Hartmut Grassl]	ACCEPTED
2-1076	A	25:56	25:	add "The aerosol optical depth (AOD) products generated from the AVHRR (Mitshchenko et al. 1999) and TOMS (Torres et al. 2002) were compared and their synergy was explored (Jeong and Li 2005). While the two products exhibit common spatial features, considerable discrepancies exist in the magnitude of AOD. Taking advantage of different sensitivities of the two products to aerosol particle size and absorption, the global aerosols	Noted. This is too detailed for inclusion in the IPCC report. We are concentrating on 'old' instruments as they give us a long time series and on 'new' instruments as they have some considerable advantages in terms of the

No.	Batch	Page:line From	Page:line To	Comment	Notes
				were classified into 8 types. Applying different spectral conversation functions for different aerosol types, a new integrated AOD at a common wavelength was generated over both ocean and land (Jeong and Li 2005)." [Zhanqing Li]	realism. However, the Jeong et al reference is now included as requested.
2-1077	A	26:0		The discussion of satellite-derived aerosol optical depths is toofocused on the MODIS instrument. The paragraph beginning on line 24 should give a more quantitative estimate of the uncertainty of optical should give a more quantitative estimate of the uncertainty of optical depth retrievals, which last I checked was more than a factor of two depth retrievals, which last I checked was more than a factor of two based on differences between POLDER, MODIS, and MISR climatologies. based on differences between POLDER, MODIS, and MISR climatologies. The uncertainty estimates made by the MODIS team of +/- 5% are notThe uncertainty estimates made by the MODIS team of +/- 5% are not believable in my opinion, yet these are the only uncertainty figures believable in my opinion, yet these are the only uncertainty figures quoted here in the text. [Steven Sherwood]	Noted. We remove the uncertainty estimates from MODIS as we do not want to highlight this instrument above the others.
2-1078	A	26:1	26:22	The section (and table 2.4.1) on advances in satellite retrieval contains very selective information. For example the French work on aerosol retrieval is not mentioned altogether. Furthermore, the ATSR-2 image that has been used in many presentations and publications is not mentioned. This is the most important pioneering work done with ATSR-2 and one of the first aerosol retrievals over land. Therefore the following reference should be included in this section and in Table 2.4.1: Robles-Gonzalez, J.P. Veefkind and G. de Leeuw, GRL 27, 955-959, 2000 (this is the paper with the much-used aerosol optical depth over Europe figure). The reference to the paper where the retrieval method is described is in: J.P. Veefkind, G. de Leeuw, and P.A. Durkee, Retrieval of aerosol optical depth over land using two-angle view satellite radiometry, Geophys. Res. Letters 25, 3135-3138, 1998. The dual-view algorithm uses both the spectral as well as the directional information contained from forward and nadir view of the ATSR-2 data. To my knowledge this was the first time that multi-angle retrievals over land were successfully performed, even before Polder and MISR. Several other pappers have been published since, using this retrieval algorithm. [Pepijn Veefkind]	Noted. We have to include only the most brief details due to space constraints. The alternative is to remove the satellite section altogether.We refute the alegation that the French work is not quoted. There are a great number of references to the French work on both POLDER and MODIS. However, the ATSR-2 references are now included.
2-1079	A	26:4	26:7	This appears to be an incomplete or inconclusive statement. [Andrew Lacis]	Noted.
2-2702	B	26:5	26:5	Tanre has an accent on the e [Olivier Boucher]	Accepted.
2-1080	A	26:7	26:10	Wasn't LANDSAT supposed to solve this problem? Why didn't it?	Noted. The problem is that the

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Jerry Mahlman]	atmospheric column contains Rayleigh scattering, aerosols and a land surface – unambiguous separation of land surface and aerosols is hard ......
2-1081	A	26:9	26:9	do you mean +/-0.05 or 0/-0.2 taer? [Joyce Penner]	Noted. Specific uncertainties removed so that we don't push MODIS too far compared to the other retrievals some of which are just as sophisticated.
2-1082	A	26:12		Reference of Kaufman et al 2005 refers not to the referenced paper on aerosol cloud interaction (that is actually referenced later) but to: Kaufman Y. J., O. Boucher, D. Tanré, M. Chin, L. A. Remer & T. Takemura, Aerosol anthropogenic component estimated from satellite data, Geoph. Res. Lett., VOL. 32, L17804, doi:10.1029/2005GL023125, 2005 [Yoram Kaufman]	Accepted.
2-1083	A	26:12		Add "Jeong and Li, 2005" after "Kaufman et al., 2005" [Zhanqing Li]	Noted. Again, we are concentrating on the new instruments here.
2-1084	A	26:16		correct: "desert" to "deserts" [Hartmut Grassl]	Accepted.
2-1085	A	26:19		a full stop is missing after the first brackets [Hartmut Grassl]	Accepted.
2-1086	A	26:24	26:30	This paragraph needs to be much more quantitative. How large are the differences in taer? What does 'extensive' validation mean-- how often have satellites been compared with ground-based measurements? There are even challenges in comparing AERONET and satellite data. When satellites 'show good agreement on a case-by-case basis', what does this mean-- how frequent are the cases and how good is the agreement? [Tami Bond]	Noted. We have reworded this section and will tie up the satellite based and model based assessments of the radiative forcing better in the next draft.
2-1087	A	26:24	26:30	After reading this paragraph, I am left wondering why I should trust any of the satellite retrivals, even from the dedicated instruments. This is a problem given the emphasis on their use for validating aerosol models. I would prefer some further justification on this point. [Eleanor Highwood]	Noted. Reworded.
2-1088	A	26:24	26:29	Ouch! Strong statement that observations cannot be compared. There is certainly: A) Validation with AERONET B) Case by case studies during field experiements. [Robert Levy]	Noted. Reworded.
2-1089	A	26:24	26:29	If the observations cannot be objectively quanitified, can we say that there is a "medium" level of understanding? [Robert Levy]	Accepted. We have reassessed the LOSU at low.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1090	A	26:24	26:29	How well do we need to quanitify the global aerosol distribution in order to determine aerosol effects and forcings? [Robert Levy]	Noted. Penner et al in the TAR have given an error assessment which we cannot improve upon given the space constraints ....
2-1091	A	26:24	26:28	Other major difference in satellite retrievals = cloud screening and surface screening [Robert Levy]	Accepted. Cloud screening is now mentioned explicitly.
2-1092	A	26:24	26:30	Huh? It seems that, either this patching has been unsuccessful, or that the satellites have not come close to achieving what was originally promised. [Jerry Mahlman]	Noted. We have been more objective in the assessment of the satellite and model based assessments of the radiative forcing.
2-1093	A	26:24	26:25	The statement starting with "Despite the increased sophistication and realism…." is in contrast with page 24 line 47 [Felicita Russo]	Noted. We have reworded and reemphasised the text.
2-1094	A	26:24		...discrepancies do exist… Some quantitative assessment of the magnitude of these discrepancies should be given. [Stephen E Schwartz]	Noted. This paragraph has been reworded to provide a more objective statement.
2-1095	A	26:25	26:27	It should be pointed out that perhaps one of the largest contributions to the differences between the results from different satellite-based aerosol retrievals comes from the differing degree of "clearness" of the input radiances; the cloud masks used in the pre-processing steps are usually different. [Istvan Laszlo]	Accepted. Cloud screening is now mentioned explicitly.
2-1096	A	26:25	26:25	Add the main papers on model intercomparison/comparison to observations of aerosol optical depth: a) Penner, J.E., Zhang, S.Y., Chin, M., Chuang, C.C., Feichter, J., Feng, Y., Geogdzhayev, I.V., Ginoux, P., Herzog, M., Higurashi, A., Koch, D., Land, C., Lohmann, U., Mishchenko, M., Nakajima, T., Pitari, G., Soden, B., Tegen, I., and Stowe, L.: A comparison of model- and satellite-derived aerosol optical depth and reflectivity, Journal of the Atmospheric Sciences, 59 (3), 441-460, 2002. b) Kinne, S., Lohmann, U., Feichter, J., Schulz, M., Timmreck, C., Ghan, S., Easter, R., Chin, M., Ginoux, P., Takemura, T., Tegen, I., Koch, D., Herzog, M., Penner, J., Pitari, G., Holben, B., Eck, T., Smirnov, A., Dubovik, O., Slutsker, I., Tanre, D., Torres, O., Mishchenko, M., Geogdzhayev, I., Chu, D.A., and Kaufman, Y.: Monthly averages of aerosol properties: A global comparison among models, satellite data, and AERONET ground data, J. Geophys. Res., 108 (D20), 4634, doi:10.1029/2001JD001253, 2003. c) Kinne et al.: An AeroCom initial assessment – optical properties in aerosol component modules of global models, Atmos. Chem. Phys. Discuss., 5, 8285-8330, 2005 [Christiane Textor]	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1097	A	26:25		After "Myhre et al. 2005", add "Jeong and Li 2005, Jeong et al. 2005". [Zhanqing Li]	The reference to Jeong et al is now included.
2-1098	A	26:26	26:30	How large are the discrepancies? Do the discrepancies (presumably between different satellite measurements) also exist when compared with surface based column and insitu measurements? [Tim Bates]	Noted and reworded.
2-2703	B	26:26	26:26	Myhre et al (2005): is the correct paper referenced? [Olivier Boucher]	Accepted. ACPD -> ACP.
2-1099	A	26:26	26:26	I suggest to replace "view" by "viewing" [Peter Van Velthoven]	Noted.
2-1100	A	26:26		after "in the aerosol models", add "(Jeong et al. 2005)". [Zhanqing Li]	Noted.
2-1101	A	26:29	26:29	"…it is difficult to comment objectively on the accuracy of a particular retrieval..." is also in contrast with page 24 line 47 [Felicita Russo]	Accepted. We have reworded here.
2-1102	A	26:29	26:30	Can we use AERONET measurements of AOT to "objectively comment on the accuracy of a particular retrieval relative to the other"? [Hongbin Yu]	Noted. This section has been reworked.
2-1104	A	26:29		Do you mean "the relative merit of any particular retrieval"? [Joanna Haigh]	Noted. This section has been reworked.
2-1105	A	26:29		It is wrong to say "It is difficult to comment objectively on the accuracy of a particular retrieval relative to the other (e.g., Myhre et al., 2005)". Actually, comparisons of these satellite aerosol retrievals to the surface AERONET observations do provide an opportunity to objectively evaluate as well as improve the accuracy of these satellite retrievals. As shown in Fig.10 of Myhre et al. (2005), the ensemble AERONET validation clearly indicates the more advanced aerosol retrieval from MODIS multi-channel algorithm and MISR multi-angle algorithm perform better than the other simple retrieval algorithms. Zhao et al. (2005a, b) further prove the MODIS aerosol retrieval based on the dynamic aerosol models are better suited for simultaneously measuring the regional variations in aerosol optical properties and their global mean values than AVHRR type aerosol retrievals based on a fixed aerosol model. I suggests remove "It is difficult to comment objectively on the accuracy of a particular retrieval relative to the other (e.g., Myhre et al., 2005)" and add "Comparison of these satellite aerosol retrievals to the surface AERONET observations provide an opportunity to objectively evaluate as well as improve the accuracy of these satellite retrievals (Zhao et al., 2004; Myhre et al., 2005). Zhao et al. (2005a, b) further indicate the MODIS aerosol retrieval based on the dynamic aerosol models are better suited for simultaneously	Accepted. We have used alot of this text in our revision.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				measuring the regional variations in aerosol optical properties and their global mean values than the simple AVHRR type aerosol retrieval based on a globally fixed aerosol model. (Full references listed in separate supplemental doc file) [Xuepeng Zhao]	
2-1106	A	26:31		add "Although the retrievals from the dedicated aerosol instruments such as the MODIS and POLDER are more reliable than those from the historical sensors such as the AVHRR and TOMS, their compatibility is essential in establishing a long record for studying the long term trend. At present, the various AOD products share similar features in its spatial distribution and, to some extent, in its seasonal variation. However, their magnitudes differ even more than the spatial and temporal variability (Jeong et al. 2005, Jeong and Li 2005, Myhre et al. 2005). While the causes for the differences are understood reasonably well, reconciliation of the differences and generation of consistent and integrated products are still far from being achieved. It is highly desired to first reconcile differences among the AOD products derived from the modern sensors (e.g. MODIS and MISR) and then use them as anchors to bridge the long-term products from historical sensors (e.g. AVHRR and TOMS)." [Zhanqing Li]	Noted. The IPCC cannot be prescriptive in that it cannot recommend what should and should not be funded. It would therefore be inappropriate to make these recommendations even though I am in agreement with the referee's sentiments.
2-1107	A	26:32	26:50	While I welcome the initial clear definition of the radiative effect as opposed to the radiative forcing of an aerosol, this paragraph discusses estimate of both which becomes rather confusing unless you read it very carefully. Could they be more firmly distinguished in the text? [Eleanor Highwood]	Accepted. A separate section on the direct radiative effect and the direct radiative forcing has been introduced.
2-1108	A	26:32	26:50	Delete, or move to a different section, perhaps section 2.4.5.7. DRE is not an observational result. It is a model derived quantity of questionable accuracy because of the numerous unsubtantiated modeling assumptions and approximations that have to be made in order to perform the radiative calculations. [Andrew Lacis]	Noted. However, aerosol optical depth from satellites can be viewed in the same manner as this argument (although the assumptions should be better). Also, some of the prodcuts are significantly different such as the CERES product which is essentially a DRE derived from a regression of TOA radiances against optical depth. Granted, the BDRF must be adequately described, but we believe that observational estimates of the DRE are of at least as good a grounding as the atmospheric models ......

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1109	A	26:32		Yes, the models and observations maybe "consistent" but what are the quantifyable differences? [Robert Levy]	Accepted. A separate section on the direct radiative effect and the direct radiative forcing has been introduced.
2-1110	A	26:33	26:50	I find this to be quite confusing, and not obviously connected to the concerns about the quantification, or lack thereof, of the various aerosol measurements, successful or otherwise. [Jerry Mahlman]	Accepted. A separate section on the direct radiative effect and the direct radiative forcing has been introduced.
2-1111	A	26:33	27:2	This section is titled DRE but the section mixes DRE and RF. Is the last section referring to RF efficiency or DRE efficiency? How do these compare between models? [Tim Bates]	Accepted. A separate section on the direct radiative effect and the direct radiative forcing has been introduced.
2-1112	A	26:33		DRE is not well defined. It appears to be a change in TOA net shortwave irradiance due to the presence of aerosol.It is not clear in the text or table whether the quantities quoted are local instantaneous or 24-hour average (which would be around a factor of 2 less). [Stephen E Schwartz]	Accepted. A separate section on the direct radiative effect and the direct radiative forcing has been introduced.
2-1113	A	26:34	26:34	If RF considers only the anthropogenic component, then the volcanic aerosol should not even be discussed in section 2.7.2. Again, the RF should be clearly and better defined at the beginning of this document. In addition, the DRE in Yu et al 2005 are all at the TOA, not at the tropopause. [Mian Chin]	Accepted. The radiative forcing section has been expanded at the beginning of the chapter.
2-1114	A	26:34	27:2	" . . . the definition of RF which considers the anthropogenic components only." This again underscores the need for clear definitions. Atmospheric physics does not distinguish between anthropogenic and non-anthropogenic components. If there is to be an anthropogenic radiative forcing contribution, then perhaps it should be labeled as ARF. The point being is that radiative forcings need to be computed for the full atmospheric amounts of radiative constituents since the radiative transfer results are not necessarily that linear. Attribution to anthropogenic causes should be done after the fact once all of the radiative forcings have been properly evaluated. Just from a purely holistic perspective of understanding climate, it is important to know the total radiative forcing (TRF) attributable to a particular greenhouse gas or aerosol as it exists (in its entirety) in the atmosphere. If some portion ARF of TRF is to be attributable to anthropogenic causes, let that fact be clearly identified and the amount justified. [Andrew Lacis]	Accepted. . The radiative forcing section has been expanded at the beginning of the chapter.
2-1115	A	26:38	26:38	Please quantify "fair degree of agreement". [Tim Bates]	Accepted. Mean and standard deviation are now quoted.
2-1116	A	26:38	26:39	The table shows that the central value is about -5 W/m2. The text sounds as if the agreement (variability) is only to 5 W/m2-- which would not be good agreement at all. Perhaps the text could give the range of the estimates.	Accepted. Mean and standard deviation are now quoted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Tami Bond]	
2-1117	A	26:39	26:40	Only one study shows the consistency?! This is a big general statement but only one reference is listed. The model results in Yu et al 2005 show a very large range of DRE estimates, and they cannot be all consistent with satellite based estimates. [Mian Chin]	Noted. However, we DO include the numbers from the study of Yu et al. (2005) in a table. The reader can therefore make up their own mind.
2-1118	A	26:39	26:40	This sentence is confusing in light of the statement made on page 36, line 10. In general, descriptions of the accuracy of different models appear to contradict themselves if from different subsections of section 2.4 [Ellsworth Welton]	Noted. Sentence removed.
2-1119	A	26:39	26:40	This statement is inconsistent with that in page 36, line 10. Significant differences exist between satellite retrievals and recent model simulations, and the reason for this discrepancy is not clear! [Hongbin Yu]	Noted. Sentence removed.
2-1120	A	26:42	26:47	Here the RF and DRE are all mixed up. All the satellite based estimates are DRE, nor RF. [Mian Chin]	Accepted. A separate section on the direct radiative effect and the direct radiative forcing has been introduced.
2-1121	A	26:42	26:47	Give justifications of the assumption of no aerosol direct RF in cloudy regions. [Mian Chin]	Noted. Not possible to include this detail here owing to space constraints. This item is discussed in detail for e.g. biomass burning aerosol etc and forward reference is now made.
2-1122	A	26:42	26:42	Adding "aerosol" between "... there is no" and "contribution..." would help understanding the sentence. [Alcide di Sarra]	Accepted.
2-1123	A	26:43		after "Loeb and Manalo-Smith (2005) assume that there is no contribution to the direct RF from cloudy …" add "The aerosol DRF under cloudy conditions may not be ignored for absorbing aerosols which enhance atmospheric absorption and reduce or even reverse the aerosol cooling effect of the atmosphere-surface system, noting that absorbing aerosols under cloudy conditions have a warming effect at the TOA and cooling effect at the surface (Li and Trishchenko 2001)" [Zhanqing Li]	Forward reference is now made to the effects of absorbing aerosol above cloud.
2-1124	A	26:47	26:47	What is the uncertainty in these estimates? [Tim Bates]	Uncertainty ranges in these estimates now included.
2-1125	A	26:49		I would argue that the contribution to the RF from aerosols below clouds is also not negligible. [Hongbin Yu]	Noted. Sentence now removed.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1126	A	26:50		Additional reason is the large uncertainty in dust generation. Dust is generated in the models using reanalysis winds that were shown over the dust sources to be inaccurate within factor 2 (Koren et al 2005) Koren, I. and Y. J. Kaufman Direct wind measurements of Saharan dust events from Terra and Aqua, Geophys. Res. Lett., 31, Doi:10.1029/2003GL019338, 2004 [Yoram Kaufman]	Noted, but probably not relevant here.
2-1127	A	26:56	26:57	How does the forcing efficiency remove the dependence on the retrieved tau? It should be directly dependent on tau according to the definition. [Mian Chin]	Rejected. Tau is assumed to have an inherent error from transport and optical parameter modelling
2-1128	A	26:56		need to define "RF efficiency" here. Also note that papers cited here derive DRE, but not RF. [Hongbin Yu]	Accepted.
2-1129	A	27:0		Section 2.4.3.2. Contents of surface based retrievals are generally weak in comparison with Section 2.4.3.1. It could be more subdevided into sulphate aerosol, black carbon, mineral dust, biomass burning aerosol, for example. [Yukitomo Tsutsumi]	Rejected. Surface based retrievals are necessarily of the complete aerosol component, not speciated.
2-1130	A	27:1	1:2	It is hard to understand "the RF efficiency is non-linear....". We should say "the RF is not a linear function of AOT at high AOT range". If the RF is defined as the direct effect by anthopogenic aerosols, we should not mention "large mineral dust events" here. [Hongbin Yu]	Accepted. Have added other high optical depth events.
2-1131	A	27:1	27:2	Explain non-linearity in this context. [Jerry Mahlman]	Accepted.
2-1132	A	27:2		The nonlinearity is also in the case of pollution in China or some in South America with AOT of 2-3. [Yoram Kaufman]	Accepted.
2-1133	A	27:4	27:4	Change "retrievals" to "measurements" since some of the data (IMPROVE, tau from AERONET) are not retrieved products but direct measured ones. [Mian Chin]	Rejected.
2-1134	A	27:5	27:16	This paragraph doesn't belong in this section which is about surface based retrievals. [Tim Bates]	Accepted.
2-1135	A	27:5	27:16	I don't find this paragraph very satisfying. It sounds as if you are saying that there is perhaps some key information but this is not summarized at all. For example, how has understanding of variability provided by in-situ measurements affected model development and results? If I were making funding decisions based on this paragraph, I'm not sure I would consider in-situ measurements very favorably. Yet those types of measurements are the only reason we know anything about chemical composition.	Noted. We are not allowed to make policy prescriptive statements.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Tami Bond]	
2-1136	A	27:5	27:14	No references to "validation" of global in-situ measurements. How are they used? [Robert Levy]	Noted. It is not possible to go into detail here owing to the space constraints.
2-1137	A	27:5	27:8	NOAA funds routine aerosol in-situ measurements at several network sites. See http://www.cmdl.noaa.gov/aero/net/index.html for a listing of aerosol stations, references, and contacts. [Ellsworth Welton]	Accepted – this is added in replacement for the IMPROVE network.
2-1138	A	27:6	27:6	Insert the word "a" between "as" and "long-term" [Patrick Hamill]	Noted. 'long-term measurement sites'
2-2704	B	27:7	27:7	Why single out IMPROVE here? [Olivier Boucher]	Accepted. A web refernce is now used.
2-1139	A	27:7	27:7	I wonder what is the uniqueness of IMPROVE that it is specifically mentioned here. It seems there is no need to point out any specific network in this paragraph. [Mian Chin]	Accepted. A web refernce is now used.
2-1140	A	27:8	27:10	This statement is dubious. In-situ measurements can provide essential validation for global models, but such activity is not occuring on a routine basis for most, if not all, models. The sentence gives the impression that it is. If I am in error, then references are required. [Ellsworth Welton]	Noted. This statement gives reasons for why there are not comprehensive validation efforts based on the surface measurements.
2-1141	A	27:8		separate "e.g." with commata [Hartmut Grassl]	Accepted.
2-1142	A	27:11	27:13	Are ground based in-situ measurements always representative of conditions at the surface? AERONET retrievals for example give something in comparison to a bulk column quantity, whilst LIDAR can certainly distinguish between vertical profiles. I think this is an unfair simplification. [Eleanor Highwood]	Accepted. In-situ measurements are different from remote sensing measurements made from teh surface. The paragraph has been moved to reflect this better.
2-1143	A	27:12	27:12	are complicated by the effects of meteorology and ...(This is awkward usage). Why not use "Weather Conditions?) [Jerry Mahlman]	Accepted.
2-1144	A	27:14		correct: "are" to "is" [Hartmut Grassl]	Rejected.
2-1145	A	27:27	27:27	if the taer is high enough…' You should state how high and how often this criterion is met. [Tami Bond]	Accepted. How often this criteria is met would be too detailed.
2-1146	A	27:27	27:27	should read "the column-averaged aerosol…"	Accepted. Column-avereaged added a

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Graham Feingold]	couple of times.
2-1147	A	27:27	27:27	Aerosol Optical Depth - Tau aer - aer shold be a subscript [MARCOS S. P. GOMES]	Accepted.
2-1148	A	27:27	27:27	"aer" should be subscript. [Xiaobin Xu]	Accepted.
2-1149	A	27:27		aer lower case [Junying Sun]	Accepted.
2-1150	A	27:27		tau aer" should be "tau aer(subscripted) [Xuepeng Zhao]	Accepted.
2-1151	A	27:29	27:32	I recommend emphasizing the need for validation of AERONET retrieved products by changing from "Whilst these inversion products have not been comprehensively validated a number of studies show encouraging agreement when compared against in-situ measurements from aircraft measurement campaigns for different aerosol species (e.g., Dubovik et al., 2002; Haywood et al., 2003a; e.g., Reid and et al., 2003; Osborne et al., 2004)." to "Although there is encouraging agreement when these inversion products are compared against in situ aircraft-based measurements (e.g., Dubovik et al., 2002; Haywood et al., 2003a; e.g., Reid and et al., 2003; Osborne et al., 2004) and when the products are compared to models (Reddy et al., 2005b), the AERONET inversion products have not been comprehensively validated." [Brian Magi]	Rejected. We have to be careful not to make statements that could be seen as research proposals.
2-1152	A	27:29	27:32	This is a weak statement. Reword, or rethink. [Jerry Mahlman]	Noted. We have added that the size distribution and the single scattering albedo both compare favorably.
2-1153	A	27:30	27:30	"Show encouraging agreement". Can you be more quantitative? [Tim Bates]	Noted. These stuies show a variety of comparisons, and it is difficult to be fully quantitative in the space allocated. We now include the fact that both the size distributions and the absorption properties have been shown to be in reasonable agreement.
2-1154	A	27:31	27:31	… measurements from aircraft (remove "measurement") campaigns … [MARCOS S. P. GOMES]	Accepted. Reworded.
2-1155	A	27:32	27:43	Since much of this paragraph refers to works-in-progress (Sato et al 2003 was updated with the Reddy et al. 2005b reference and the Schuster et al 2005 is an early attempt to derive more information from AERONET data), I recommend deleting the following sentences: "Sato et al. (2003) determined the aerosol absorption optical depth from	Accepted. We have moved this to a later section which compares the model and the measurements.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				AERONET measurements and suggested that aerosol absorption simulated by global aerosol models is underestimated by a factor of between 2–4. Schuster et al.(2005) estimate the black carbon loading over continental scale regions. Prima facia the results suggest that the model concentrations and absorption optical depths of black carbon from models are lower than those derived from AERONET. Some of this difference in concentrations could be explained by the assumption that all aerosol absorption is due to black carbon (Schuster et al., 2005), while a significant fraction may be due to absorption by organic aerosol and mineral dust (see Sections 2.4.5.2, and 2.4.5.6). Furthermore, Reddy et al. (2005a) show that comparison of the aerosol absorption optical depth from models against those from AERONET must be performed very carefully, reducing the discrepancy between their model and AERONET derived aerosol absorption optical depths from a factor of 4 to a factor of 1.2 by careful co-sampling of AERONET and model data." [Brian Magi]	
2-1156	A	27:32	27:43	I suggest to remove the last part of this paragraph (line 32 "Sato ..." untill line 43 ".. data"). This part talks about model shortcomings, rather than the surface based retrieval results. This part is oout of scope and contains unnecessary detail. [Pepijn Veefkind]	Accepted. We have moved this to a later section which compares the model and the measurements.
2-1157	A	27:34	27:34	… underestimated by a factor (remove "of" or "between") 2-4. [MARCOS S. P. GOMES]	Accepted. We have moved this to a later section which compares the model and the measurements.
2-1158	A	27:35	27:35	What is "Prima facia"? [Eugene Rozanov]	Accepted. We have moved this to a later section which compares the model and the measurements.
2-1159	A	27:35		correct: "facia" to "facie" [Hartmut Grassl]	Accepted. We have moved this to a later section which compares the model and the measurements.
2-1160	A	27:39	27:43	The message of this sentence is not clear, but it could be very important. Is the estimated discrepancy of 2-4 (Sato) simply due to sampling differences? This suggests that the community needs to put a lot more effort into statistical methods! Please clarify the message. [Tami Bond]	Accepted. We have moved this to a later section which compares the model and the measurements.
2-1161	A	27:39	27:43	This seems to actually be saying that the problem is unsolved and unclosed, without actually saying so. [Jerry Mahlman]	Accepted. We have moved this to a later section which compares the model and the measurements.
2-1162	A	27:45	27:50	Discussion of what has been learned for the lidar networks should be included. [Tami Bond]	Noted. Space constraints mean that we cannot go into this here.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1163	A	27:45	27:46	I am the MPLNET PI. Suggested rewrite: The MPLNET Lidar network currently consists of eleven lidars worldwide, nine are co-located with AERONET sites and provide complementary vertical distributions of aerosol backscatter and extinction. Eleven additional MPLNET sites have supported major aeorsol field campaigns (e.g., Campbell et al. 2003; e.g., Reid et al. 2003). NOTE: The MPLNET website is http://mplnet.gsfc.nasa.gov for further reference. [Ellsworth Welton]	Accepted.
2-1164	A	27:45	27:46	Reference for comment #10: Campbell, J. R., E. J. Welton, J. D. Spinhirne, Q. Ji, S. Tsay, S. J. Piketh, M. Barenbrug, and B. N. Holben, Micropulse Lidar observations of tropospheric aerosols over northeastern South Africa during the ARREX and SAFARI-2000 Dry Season experiments. J. Geophys. Res., 108, 8497, doi:10.1029/2002JD002563, 2003. [Ellsworth Welton]	Accepted.
2-1165	A	27:45	27:46	Reference for comment #10: Reid, J. S., J. E. Kinney, D. L. Westphal, B. N. Holben, E. J. Welton, Si-Chee Tsay, D. P. Eleuterio, J. R. Campbell, S. A. Christopher, P. R. Colarco, H. H. Jonsson, J. M. Livingston, H. B. Maring, M. L. Meier, P. Pilewskie, J. M. Prospero, E. A. Reid, L. A. Remer, P. B. Russell, D. L. Savoie, A. Smirnov, and D. Tanre´, Analysis of measurements of Saharan dust by airborne and ground-based remote sensing methods during the Puerto Rico Dust Experiment (PRIDE), J. Geophys. Res., 108, 8586, doi:10.1029/2002JD002493, 2003. [Ellsworth Welton]	Accepted.
2-1166	A	27:45		This paragraph could be deleted to shorten the chapter, unless the MPLNET data are being used somewhere else in the chapter. [Daniel Murphy]	Noted. We chose to keep it as it ties in with the remote sensing measurements made in Figure 2.4.2.
2-1167	A	27:46	27:48	In the EARLINET project there are more than 20 lidar stations, as reported in the corresponding reference on page 82 line 54. [Felicita Russo]	Noted. However, at the time of writing only fifteen sites are operational.
2-1168	A	27:52	29:27	This section (2.4.4) is a bit unfocused with too much detail about and emphasis on the AEROCOM exercise. It should summarize (a) advances in modeling, (b) determining factors in obtaining RF from the models (e.g., emission, mass burden, tau, single scattering albedo, mixing state…), and (c) uncertainties in RF from the available models. [Mian Chin]	Accepted
2-1169	A	27:52	36:56	In sections 2.4.4 and 2.4.5 extensive use is made from AEROCOM studies. The most important references are to Schultz et. al. 2005 and Kinne et al, 2005. However, these are only manuscript that have been submitted. As such, based upon the peer review process, results and conclusions from these manuscript may change. Also, these manuscripts are not readily available for the IPCC reviewers. Thus, I strongly object against the usage of	Taken into account. Details of the AeroCom exercise are found in the published papers Texter et al 2005 and Kinne et al 2005. They had been available on ACPD for the review

No.	Batch	Page:line From	Page:line To	Comment	Notes
				submitted manuscripts as key references. [Pepijn Veefkind]	process. A table with references to the models used here has been taken out when preparing the FOD, additional references are added in tables to identify the models from which radiative forcing estimates are derived.
2-2705	B	27:52		Shouldn't this session also address advances in modelling of the aerosol indirect effect? [Olivier Boucher]	Taken into account. Indeed the section would suggest this. However, also the 'advances in observations" concern rather the direct effect. A sentence is added that the two sections on "advances" concern aerosol properties, and not cloud-aerosol interactions. The aerosol-cloud interactions section (2.4.6) does address "new evidence" and "post-TAR" GCM results.
2-1170	A	27:52		Section 2.4.4: this section is poorly balanced, it is focused entirely on aerosols without even indicating that there have also been advances in gas phase modeling, especially with regards to the understanding/evaluation/intercomparison of these models, especially via two major efforts that should be cited here: For the troposphere, Stevenson, D.S., F.J. Dentener, M.G. Schultz, K. Ellingsen, T.P.C. van Noije, O. Wild, G. Zeng, M. Amann, C.S. Atherton, N. Bell, D.J. Bergmann, I. Bey, T. Butler, J. Cofala, W.J. Collins, R.G. Derwent, R.M. Doherty, J. Drevet, H.J. Eskes, A.M. Fiore, M. Gauss, D.A. Hauglustaine, L.W. Horowitz, I.S.A. Isaksen, M.C. Krol, J.-F. Lamarque, M.G. Lawrence, V. Montanaro, J.-F. Müller, G.Pitari, M.J. Prather, J.A. Pyle, S. Rast, J.M. Rodriguez, M.G. Sanderson, N.H. Savage, D.T. Shindell, S.E. Strahan, K. Sudo, and S. Szopa, 2005: Multi-model ensemble of present-day and near-future tropospheric ozone. Journal of Geophysical Research, in press; and for the stratosphere the CCMVal work discussed in Eyring et al., A Strategy for Process-Oriented Validation of Coupled Chemistry–Climate Models, Bull. Amer. Meteorol. Soc., 86, 1117–1133. [Mark Lawrence]	Rejected: The section tries to assess work done purely on the aerosol modeling. Other sections in chapter 2 deal with gasphase chemistry.
2-1171	A	27:54	27:56	'convenient' and 'readily' are relative terms and could even be read as condescending. I don't think there is anything easy about differentiating natural and anthropogenic terms. [Tami Bond]	Accepted, 'convenient' and 'readily' are deleted
2-1172	A	27:54	27:55	This is ironic – the global models provide estimates of RF either "at the top of the atmosphere" or "at the surface". What about the RF at the tropopause (page 7) that this	Accepted, a sentence included that state that the difference in the TOA and

No.	Batch	Page:line From	Page:line To	Comment	Notes
				report asks for? Can the models provide those values? Again and again, the use of RF should be consistent with its definition, or the definitions should be revised. [Mian Chin]	tropopause RF is small in the case of aerosols
2-1173	A	27:54		Section 2.4 lacks any definitive conclusions that could help guide policy makers. The discussion of models points to strengths and weaknesses, but does not answer the question "is this good enough to base a decision on." [Lourdes Maurice]	Noted, see the synthesis section (2.9) for indications of our judgments on the overall uncertainties.
2-1174	A	27:56	27:57	This is a run-on sentence. Also, not all models include all species, so the sentence is misleading. [Tami Bond]	Accepted, a stop is added. 'All major species' is replaced by 'The most important species'
2-1175	A	27:56	27:56	I think this statement is too positive, since only one of the 16 AeroCom models contains NO3. [Christiane Textor]	Accepted, 'All major species' is replaced by 'The most important species'
2-1176	A	28:1	29:28	I don't know where to start. This reads like an end-of-experiment report to the teams of participating experimenters. It is not at all obvious that the IPCC TAR-significant punchlines have been properly conveyed to the expected "consumers" of this Chapter 2 contribution to this IPCC assessment. There are many aspects of this discussion that I could comment on, but with low probability of penetrating the opacity and density of the writing. Suffice it to say that this section needs to be carefully rethought in the context of what it can really produce in terms of an understandably relevant information for contribution to the goals of this IPCC Chapter 2 report. [Jerry Mahlman]	Noted
2-1177	A	28:2	28:2	It would probably be safe to also add the Angstrom exponent and single scattering albedo to the list of key diagnostic model output parameters. [Andrew Lacis]	Rejected, only AOD available from the listed platforms
2-1178	A	28:6	28:10	The description of the AeroCom initiative is not sufficient. Explain abrevation (AeroCom: Global Aaerosol Model Intercomparison). Move sentence in line9/10 to line 7, explain the three experiments. [Christiane Textor]	Taken into account, abrevation included. The sentence is not moved
2-1179	A	28:7	28:8	This statement is too positive. One model (KYU-SPRINTARS) had a resolution of 1.1x1.1 degree, three had about 1.8x1.8, and one a 1x1 zoom over Europe and the US. Only one model had 40 vertical levels (UIO_CTM). Most of the models have between 20 and 30 levels, see Textor et al., Atmos. Chem. Phys. Discuss., 5, 8331-8420, 2005. The sentence should be 'Several models have used a resolution below 2x2 degree in the horizontal, and between 20 and 30 levels in the vertical.' [Christiane Textor]	Taken into account. Increased resolution of models over TAR is a fact.
2-1180	A	28:9	28:17	A, B, PRE are identified as runs and then A, B, C are defined.	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Tami Bond]	
2-1181	A	28:9	28:9	change to Schulz et al. 2004 [Christiane Textor]	Accepted
2-1182	A	28:9		a full stop is missing after "TAR" [Hartmut Grassl]	Accepted, stop added
2-1183	A	28:10	28:10	there seems to be a mix of the names of the experiments (A, B, PRE) and in other places, (A, B, C). (See also Fig. 2.4.3.) [Graham Feingold]	Accepted
2-1184	A	28:10	28:10	remove hyphen after the colon [Christiane Textor]	Accepted
2-1185	A	28:16	28:16	change 'years' 'year' [Christiane Textor]	Accepted,
2-1186	A	28:16		correct: "years" to "year" [Hartmut Grassl]	Accepted
2-1187	A	28:17	28:17	change 'years' 'year' [Christiane Textor]	Accepted
2-1188	A	28:17		see: above [Hartmut Grassl]	Accepted
2-1189	A	28:20	28:20	change 'life times' to 'residence times' [Christiane Textor]	Rejected
2-1190	A	28:21	28:21	why 'e.g.' Textor? Is there another paper? [Christiane Textor]	Accepted, 'e.g' removed
2-1191	A	28:23	28:25	Suggestion: "The diagnostic parameters appear in a wide rage of values, specially in the case of natural aerosols originated by dust and sea salt … the high scatter "in" emission fluxes." [MARCOS S. P. GOMES]	Noted, sentence changed
2-1192	A	28:23	28:24	Is this proper english? Or better 'The simulation results showed a large scatter for several of the parameters that were investigated, especially in the case of ....' [Christiane Textor]	Noted, sentence changed
2-1193	A	28:23	28:57	The FOD discusss a wide range in natural aerosol (including dust) emissions (p. 28 lines 23-34) in more detail than the range in natural aerosol burden (p. 28 lines 52-57). The burden is more important for radiative forcing than the emissions/deposition flux for reasons mentioned in the report. (Emissions depend strongly on the size cutoff used, while burdens are "self-correct" since the large particle fall out). Moreover, satellite and remote-sensing measurements can only directly constrain burden, not emissions/deposition fluxes. The most recent study to review published mineral dust emission and burden	Accepted text changed to emphasize and discuss range of dust loads.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				estimates is Zender et al. (2004). We showed that dust burden varies by a factor of _four_ (from 8--36 Tg) amongst studies published since 2001. Yet Line 53 on p. 28 says that the maximum disparity among AEROCOM burdens is 20-30% for fine mode aerosols and 80% for sea-salt. This is inconsistent with Zender et al. (2004, Table 1) who show that dust burden varies by a factor of four amongst published models. The dust burden is approximately equally split between PM2.5 and larger particles (Zender et al., 2003, Table 8). Hence, both fine mode and coarse mode dust burdens vary more widely in most of the published literature than in the AEROCOM simulations described in the FOD. The result is that the FOD _understates_ the inter-model disparity in natural aerosol burden, which is the diagnostic most relevent to aerosol radiative effects. This implies that the uncertainty in anthropogenic dust burden, and thus RF, is more than the FOD describes. What Zender et al. (2004) find most troubling about the inter-model disparity in dust burden (and optical depth), is that it is the quantity most well constrained by satellites. Models agree much more on dust emissions (only a factor of two spread among models) even though emissions and deposition are not directly constrained by any global observations. Prescribing emissions (as AEROCOM does) for numerical experiments is fine but we know much more about burden (from satellite-retrived optical depth) than we know about emissions of natural aerosol. Because the estimated RF from dust is derived largely from AEROCOM studies with fixed emissions, I think it probably underestimates the true range of dust RF. In summary, both of my comments on the FOD point to a need to increase the range of uncertainty for dust RF. The two reasons given are independent of eachother. Zender, C. S., R. Miller, and I. Tegen (2004), Quantifying Mineral Dust Mass Budgets: Terminology, Constraints, and Current Estimates, Eos Trans. AGU, 85(48), 509-512. Zender, C. S., H. Bian, and D. Newman, Mineral Dust Entrainment And Deposition (DEAD) model: Description and 1990s dust climatology, J. Geophys. Res., 108(D14), 4416, doi:10.1029/2002JD002775, 2003. [Charles Zender]	
2-1194	A	28:24	28:25	Zender et al EOS 2004 document the wide range of emission estimates by recent dust models. Zender, C. S., Miller, R. L. and Tegen, I: Quantifying Mineral Dust Mass Budgets:Terminology, Constraints,and Current Estimates, Eos, v.85, No.48, 30 November 2004 [Ron Miller]	Accepted, see above
2-1195	A	28:24	28:24	I am not sure what this sentence means. Maybe: 'Diverging contributions of coarse aerosols to the emitted aerosol mass of these aerosol types are responsible for the high scatter in the simulated emissions fluxes. [Christiane Textor]	Noted, sentence changed.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1196	A	28:25	28:25	change "scatter" to "variability" [Graham Feingold]	Accepted
2-1197	A	28:25		What is "high scatter" of emission fluxes ? [Robert Levy]	Noted, changed as suggested above
2-1198	A	28:26	28:32	"The higher order dependence of the source strength on wind speed adds to the problems in computing natural aerosol emissions. Dust emissions have been found to vary by a factor of two due to the difference in the high-end tail of the wind distribution e.g., this occurs for two operation modes (nudged and climatological) of the same climate model, ECHAM4 (Timmreck and Schulz, 2004). The major reason for the larger difference in dust emissions as compared to the fine mode components is, however, the range in source strength maps established by the different groups (Balkanski et al., 2004)." These sentences do not include all the sensitivies of dust emissions that have been established in the literature, and go into more detail than seems appropriate for the wider audience. There are large uncertainties from the basic parameterization (not just the tail end of the wind speeds) as well as the meteorological wind set used (Luo et al., 2003). Much of this is summarized in Zender et al., 2004. I would propose the following sentences: "The higher order dependence of the source strength on wind speed adds to the problems in computing natural aerosol emissions. Dust emissions for the same time period can vary by a factor of or larger depending on details of the dust parameterization (Luo et al., 2003; Timmreck and Schulz, 2004; Balkanski et al., 2004; Zender et al., 2004), and even on which reanalysis dataset used (Luo et al., 2003)." One could also, just cite Zender et al., 2004 instead of the first 4 references. references: Luo, C.,N. M.Mahowald, and J. del Corral (2003), Sensitivity study of meteorological parameters on mineral aerosol mobilization, transport, and distribution, J.Geophys. Res.,108, 4447, doi:10.1029/ 2003JD003483. Zender, C. S., R. Miller, and I. Tegen (2004), Quantifying Mineral Dust Mass Budgets: Terminology, Constraints, and Current Estimates, Eos Trans. AGU, 85(48), 509–512. [Natalie Mahowald]	Accepted, text was changed, see also above.
2-1199	A	28:28	42:10	AEROCOM: Much more of this needs to be summarized in tables. It is very confusing to the reader because there are so many experiments performed. [Robert Levy]	Rejected, space constraints do not allow us this and we refer to the publications wherr more details can be found
2-1200	A	28:30	28:32	Cakmur et al (JGR 2006 in press) show that the global burden of emission of mineral dust in optimal agreement with a wide range of observations varies between 1500 and 2500 Tg per year, depending upon how the source region is prescribed. Cakmur,†R. V. and R. L. Miller†and J. Perlwitz†and I. V. Geogdzhayev†and P. Ginoux and D. Koch and K. E. Kohfeld, 2006, Constraining the Magnitude of the Global Dust Cycle By Minimizing the Difference Between a Model	Accepted. The work was incorporated in mineral dust section.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				and Observations, J. Geophys. Res., 111, doi:10.1029/2005JD005791 (in press: I'll forward a copy to the lead authors) [Ron Miller]	
2-1201	A	28:30	28:32	I do not agree with this statement. Balkanski et al. pointed out a major problem in dust simulations, but does not proove that the source strength maps are the major reason for the model diversity. Uncertainties in dust modelling are due to the description of the particle sizes, because the consideration of large particles in some of the models leads to a very high emission flux, and consequently to differences in the deposition flux as well. In addition, the parameterization of the emission flux as a function of surface properties and wind speed is a problem. Another important reason are the differences in the simulated wind speeds and surface properties. [Christiane Textor]	Taken into account. Text has been modified to give a better view on progress in dust modelling.
2-1202	A	28:32	28:34	What does this sentence mean? [Christiane Textor]	Noted, the sentence is clear
2-1203	A	28:36	28:50	Please mention that 2 thirds of dust and sea salt are removed by dry deposition, and that the removal of the fine aerosols is dominated by wet deposition [Christiane Textor]	Rejected, too detailed
2-1204	A	28:37	28:37	Please explain "for all five aerosol components". [Caroline Leck]	Accepted, 'considered in AEROCOM' included in the end of this sentence
2-1205	A	28:39	28:41	We note that the PBL-burden…' Presumably this statement is in terms of percentages-- otherwise it seems impossible. What does 'varies more' mean? How much more? [Tami Bond]	Accepted, table which had contained the numbers was left out in the FOD.
2-1206	A	28:39	28:39	PBL is defined as the layer below 1 km [Christiane Textor]	Accepted
2-1207	A	28:39	28:40	The burden in the PBL of all species (except for SO4) varies more than the total burden. [Christiane Textor]	Accepted
2-1208	A	28:40	28:41	I agree that the high diversity of the simulated humidification is one of the major uncertainties in aerosol modelling with important implications on radiative forcing. However, there are more reasons for it than the vertical aerosol.distribution. The differences in the parameterizations and in the simulated relative humidity are as important for aerosol water uptake as the vertical aerosol distribution. [Christiane Textor]	Noted
2-1209	A	28:40	28:41	Please move the sentences about aerosol water uptake to line 51, it is confusing in the middle of the discussion about vertical aerosol distributions. [Christiane Textor]	Rejected, important to keep it here for explaining the main cause for the differences
2-1210	A	28:43	28:44	…variation of upper troposphere mass fraction is much more important…' I do not	Accepted, table which had contained

No.	Batch	Page:line From	Page:line To	Comment	Notes
				understand this phrase. What is importance? In terms of burden, aerosol cycling, radiative forcing? Also, please be more quantitative: what is 'much more'? [Tami Bond]	the numbers was left out in the FOD
2-1211	A	28:43	28:43	please define what you mean by "upper tropospheric" [Eleanor Highwood]	Accepted, (defined as the layer above 5km) added
2-1212	A	28:43	28:44	This statement about the variation in the upper troposphere seems inconsistent with the claim on page 25 line 2 that the vertical distribution of aerosols is improved [Joyce Penner]	Accepted, p2512 was removed since it was considered too general.
2-1213	A	28:43	28:44	Not clear, see also comment on lines 39/40. change sentence to 'Although a large diversity has been found for the simulated aerosol masses in the PBL, the models agree better on the fraction of total mass in the PBL. Highest diversities for the mass fractions are found above 5 km height in the upper free troposphere and in the tropopause region. [Christiane Textor]	Noted.
2-1214	A	28:44	28:44	… more important "than" that of the PBL … [MARCOS S. P. GOMES]	Accepted
2-1215	A	28:44	28:44	Replace "more important THEN …" with "more important THAN …" [Charles Ichoku]	Accepted
2-1216	A	28:44	28:44	more important then..." -> "more important than… [Xiaobin Xu]	Accepted
2-1217	A	28:44		then to than [Junying Sun]	Accepted
2-1218	A	28:45	28:47	I am not sure if this accumulation at greater heights, which also concerns SO4, is caused by the wet deposition schemes. It can also be due to vertical dispersivity of the global model, see Textor et al. ACPD 2005. It is not possible within AeroCom to distinguish a the major reason.In addition, POM is not insoluble, but in most models transferred from insoluble to soluble via the aging process. The species accumulated at greater heights are the fine aerosols (or the fine fraction in the case of dust). [Christiane Textor]	Accepted
2-1219	A	28:46	28:46	POM has not been introduced before and should be defined here. What is the difference between POM and OC? If they are the same, one terminology should be used; if not, it should be explained. [Mian Chin]	Accepted, description of POM added early in this section.
2-1220	A	28:47	28:47	suggest to change "sea salt and sulphate" to "sea salt, sulphate, and nitrate". This change would emphasize one of the points in the executive summary that nitrate is now included in models. Otherwise, page 28 only mentions the historical aerosol classifications and not the progress made on other aerosol species, notably nitrate.	Rejected, nitrate is not included in the AEROCOM results

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Scot Martin]	
2-1221	A	28:47	28:49	Is it meant here that sulfate dominates the upper troposphere compared to sea salt, or compared with all other aerosols? [Ina Tegen]	Noted, sentence clarified. Yes, sulphate becomes the dominant aerosol component in the upper troposphere >10 km.
2-1222	A	28:47	28:50	This sentence should be completely changed. The main difference between SO4 and SS is their size, which is the main reason for the difference in their vertical distribution. SO4 has higher mass fractions at higher altitudes (above 5 km) than all other species in most of the models, despite is high solubility, because it is produced at higher altitudes from chemical oxidation of pre-cursor gases. The contributions for volcanoes (mainly SO2) as well as those from biomass burning, which are both partly located above the PBL, might be another reason for the higher contribution of SO4 to the aerosol conc. at higher altititudes, but this is also true for other biomass burning aerosols. Therefore, the main reason is the chemical prodution above the PBL. [Christiane Textor]	Accepted
2-1223	A	28:49	28:50	Not true: SO4 does not dominated the aerosol in the upper troposphere in all models, see Textor et al. ACPD 2005. In general, the SO4-contribution to the AER composition becomes increasingly important with height due to the removal of DU and SS, and due to chemical SO4-production at greater altitudes within the atmosphere. In several models, SO4 dominates the aerosol composition above 10 km, but not in all models. [Christiane Textor]	Accepted. "upper troposphere" is not well defined.
2-1224	A	28:50		a full stop is missing after "models" [Hartmut Grassl]	Accepted
2-1225	A	28:52	28:56	The sentence beginning 'These variations…' does not make physical sense to me. For the most part, there is a linear relationship between emissions and burden in an individual model. It is not a question of linearity. The fact that models differ in terms of lifetime means that individual processes differ, and this is probably the reason for the 'effectiveness' difference for organic and black carbon. The models have simply specified different removal rates. However, this notion of 'effectiveness in increasing aerosol burden' could be read in a misleading fashion; it sounds like a small change in black carbon (for example) could lead to a relatively large change (75%) in aerosol burden, and that is not true because BC is such a small fraction of the aerosol emissions. I believe I understand what you mean, but I'm not sure that it is clear to any reader. [Tami Bond]	Noted, text is clarified
2-1226	A	28:53	28:53	How are these percentages defined? Do they give the range around the all-models-mean? [Christiane Textor]	Accepted, clarified
2-1227	A	28:54	28:54	change 'how linearly the models' to 'how linearly the ensemble of models'	Accepted, clarified

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Christiane Textor]	
2-1228	A	28:54		... how linearly the models relate emissions to aerosol burden ...Makes no sense. [Stephen E Schwartz]	Accepted, clarified
2-1229	A	28:55	28:55	56 and 75% of what? Unclear meaning. [Ina Tegen]	Accepted, clarified
2-1230	A	28:55	28:55	change 'eventually' to 'finally' [Christiane Textor]	Accepted
2-1231	A	28:55	28:55	change organic matter to POM (did you define these abreviations above?) [Christiane Textor]	Accepted, included in the start of the section
2-1232	A	28:55	28:56	How is the effectiveness of emission to burden calculated? Is it the ratio of residence times? Please rephrase [Christiane Textor]	Accepted
2-1233	A	28:56	28:56	… effective "than" sulphur emisions … [MARCOS S. P. GOMES]	Accepted
2-1234	A	28:56	28:56	Replace "effective THEN …" with "effective THAN …" [Charles Ichoku]	Accepted
2-1235	A	28:56	28:57	Why should the effectiveness of emissions to be translated into burden be "due to co-variations of the spatial emission distribution with spatial differences in longevity"? [Christiane Textor]	Accepted, sentence clarified
2-1236	A	28:56		change "then" to "than" [Hongbin Yu]	Accepted
2-1237	A	29:0	30:	What is the difference between Direct Radiative Forcing and Direct Radiative Effect? [Robert Levy]	Accepted, this is defined now in section 2.4.1.1.2
2-1238	A	29:1	29:3	Are you using "soot" to refer to black carbon? Or does this refer to OM? Confusing, I would expect a longer residence time if it is less soluble. [Joyce Penner]	Accepted, 'soot' changed to 'black carbon'. Regarding longer life time of BC then OC: Yes indeed some few models show a slightly longer life time of POM than BC on global average. Clarified
2-1239	A	29:1	29:3	Is this not expected? I.e., if soot particles are less soluble than organics, their residence time should be longer as is seen in the models. [Ina Tegen]	Noted Regarding longer life time of BC than OC: Yes indeed some few models show a slightly longer life time of POM than BC on global average. Clarified
2-1240	A	29:1	29:1	change 'black carbon' to 'BC' and 'particulate organic matter' to 'POM' (everywhere after defining them once) [Christiane Textor]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1241	A	29:2	29:2	parameterised to parameterized [Christiane Textor]	Accepted
2-1242	A	29:2	29:2	do not use the word 'soot' as a synonym for BC without proper definition [Christiane Textor]	Accepted, 'soot' changed to 'black carbon'
2-1243	A	29:3	29:3	… slightly longer "than" those … [MARCOS S. P. GOMES]	Accepted
2-1244	A	29:3	29:6	This statement is not correct. If different emission pattern would be the only reason for the longer residence time of POM in some models, this feature would vanish in AeroCom experiment B, when all models used the same emissions. This is however not the case. In exp B all models have longer BC- then POM-residence times, except for four of the models with longer POM- then BC-residence times, and these four models show this feature also in exp A. Therefore emission patterns together with differences in the scavenging parameterizations, the spatial and temporal coincidence of precipitation and BC or POM, as well as different vertical dispersivity are responsible for the residence times. Within the framework of AeroCom it is not possible to determine the major reason. [Christiane Textor]	Accepted
2-1245	A	29:5	29:5	… aerosol burdens "than" fossil … [MARCOS S. P. GOMES]	Accepted
2-1246	A	29:5		then to than [Junying Sun]	Accepted
2-1247	A	29:8	29:11	If these models are using the same emission inventories are they really independent? Are they including only primary OC aerosol? How are they distinguishing between natural and anthropogenic (is dust natural? Are forest fires natural?)? [Tim Bates]	Noted. Models are never totally independent. clarified.
2-1248	A	29:8	29:10	It's not clear that AEROCOM really includes all possible aerosol distributions that are consistent with the observations, especially given 'peer pressure' upon modeling groups not to appear as an outlier. I suggest adding this sentence at the end of the paragraph: 'An alternative approach is to take the same model and see what range of aerosol mass is consistent with the observations. Cakmur et al (JGR 2006 in press) show that the global burden of mineral dust in optimal agreement with a wide range of observations varies by twofold, depending upon how the source region is prescribed.' [Ron Miller]	Taken into account, Cakmur work is considered now.
2-1249	A	29:8	29:11	Move the whole paragraph in a modified form to page 30, line 18, and explain how the forcing has been calculated from exp B and PRE! [Christiane Textor]	Rejected. Aerocom B-PRE calculation requires explanation at this place since it was used several times in the following chapters.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1250	A	29:8	29:9	Change sentence to 'The AeroCom model simulations of experiments B and PRE with unified emissions may be viewed as expert realizations of an ensemble of aerosol distributions for pre-industrial and present-day conditions, respectively. Here, the models are assumed to be independent from each other, and a mean ' [Christiane Textor]	Taken into account, clarified.
2-1251	A	29:9	29:9	While there is currently little choice than take the different model realizations as independent, in fact they are not - as many use very similar parameterizations for atmospheric aerosol processes and similar emission fields that do not neccessarily cover the range of possible values - this should at least be noted in the text. [Ina Tegen]	Taken into account, clarified.
2-1252	A	29:9	29:9	The model simulations are not independent, since for instance the UiO-CTM and UiO-GCM (table 2.4.3 on page 2-99) probably have strong resemblance. There ae also 3 GISS models.I suggest to replace "Here, they are taken to be independent and a mean result constructed"by "Tentatively, a calculating a mean result" [Peter Van Velthoven]	Rejected. Uios are NOT similar at all, GISS yes for transport
2-1253	A	29:10	29:10	Suggestion: " … models allows one way for arriving …" [MARCOS S. P. GOMES]	Accepted
2-1254	A	29:13	29:14	"Aerosol RF depends on anthropogenic emissions… Their computation remains uncertain" contrasts with a medium level of understanding. [Felicita Russo]	Accepted, LOSU changed to low
2-1255	A	29:13	29:27	move this whole paragraph to line 52 as it concern the spatial aerosol distribution [Christiane Textor]	Rejected and noted. Paragraph shall discuss consequences for radiative forcing. The paragraph was clarified.
2-1256	A	29:13	29:14	Delete sentence starting with 'Aerosol RF depends ...', as it has little relation to the paragraph that follows. [Christiane Textor]	Noted, the paragraph was clarified.
2-1257	A	29:13	29:27	This section is unclear and contains unnecessary detail. Specific comments: line 2 "Their computation remains uncertain". This sentence is vague and I have no idea what the authors mean by this. Next the section goes into unnecessary detail only to conclude that the dispersion differs between the models. This is followed by a conclusion (line 21 "This suggests that ... component species"), that I don't understand. What do the authors mean? This section is a key section to goto section 2.4.4 where conclusion from the approach are presented. I find this section unclear and therefore undermines the results from section 2.4.5. [Pepijn Veefkind]	Noted, the paragraph was clarified.
2-1258	A	29:14	29:14	should read "compilation of model results allows.." [Graham Feingold]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1259	A	29:14	29:14	… compilation of models allows the evaluation … [MARCOS S. P. GOMES]	Accepted
2-1260	A	29:14	29:14	change 'The AEROCOM compilation of model allows evaluation...' to 'The AeroCom model simulations allow evaluation...' (is the english correct?) [Christiane Textor]	Accepted
2-1261	A	29:14	29:14	replace "models" by "model results" [Peter Van Velthoven]	Accepted
2-1262	A	29:15	29:17	Is it unclear to what the percentage of 10-60 perc. is related. As the mass fractions above 5km are different for the three anthrop. species, it would be better to give the range for each species. [Christiane Textor]	Taken into account, since text was indeed not clear.
2-1263	A	29:16	29:16	… aerosol compounds "such as" black carbon … [MARCOS S. P. GOMES]	Accepted
2-1264	A	29:17	29:18	… above 5 km in (remove the AEROCOM) experiments A and B … [MARCOS S. P. GOMES]	Accepted
2-1265	A	29:18	29:19	" … To first order … in lifetime and thus burdens." this sentence is unclear [MARCOS S. P. GOMES]	Noted, the paragraph was clarified.
2-1266	A	29:18	29:19	For the fine aerosol components, a weak correlation between aerosol dispersal and residence timeis found, as can be seen on the AeroCom web site, where the sink rates are plotted versus the mass fraction above 5 km for all experiments. The relation ship between aerosol dispersal and residence time is most pronounced for SO4 and decreasing for POM and BC. There is no such relation ship for SS and DU. [Christiane Textor]	Noted, the paragraph was clarified.
2-2706	B	29:19	29:19	dispersivity ==> dispersion [Olivier Boucher]	Noted, the paragraph was clarified.
2-1267	A	29:21	29:23	It is true that models have different removal rates and lifetimes (what you call specific dispersion property). It is not clear how this translates to the idea that combined aerosol effect is more pragmatic than effect of individual species. The percentage uncertainty due to lifetime will be approximately the same for total submicron species or due to individual submicron species. [Tami Bond]	Noted, the paragraph was clarified.
2-1268	A	29:21	29:23	The sentence is a bit unclear but a possible interpretation might be: " … This suggests that the RF result obtained when combining all the aerosol effects in one single model is more realistic than grouping the ones obtained individually by modeling each aerosol component species separately." [MARCOS S. P. GOMES]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1269	A	29:21	29:23	What are you trying to say here? [Joyce Penner]	Noted, the paragraph was clarified.
2-1270	A	29:21	29:23	This suggests...' I do not understand this sentence. [Christiane Textor]	Noted, the paragraph was clarified.
2-1271	A	29:22	29:22	what is a 'pragmatic result'? [Christiane Textor]	Noted, the paragraph was clarified.
2-1272	A	29:25		This paragraph needs to be split. As written it implies that the changes are directly attributable to radiance changes but this is not shown. Haigh's modelling, excellent as it is only explains about 20% of the observations. [Alan Rodger]	Accepted
2-1273	A	29:26	29:27	There have been many studies comparing model simulations with field data. For example: STEM has been extensively compared against the TRACE-P and ACE-Asia data, and is being tested against the ICARTT and INDOEX data. Detailed comparisons can be found in Carmichael et al. [2003a], Carmichael et al. [2003b], Horowitz et al. [2003], Tang et al. [2003], Bates et al. [2004], Tang et al. [2004] and Streets et al. [2005]. [Tim Bates]	Accepted, an assessment of model evaluation is kind of short in this report due to space constraints. Clarified, referenced.
2-1274	A	29:26	29:27	I was under the impression that AEROCOM did perform this type of verification? What is missing? [Ina Tegen]	Accepted, an assessment of model evaluation is kind of short in this report due to space constraints. Clarified
2-1275	A	29:26	29:27	The last sentence of section 2.4.4 suggest that the model results can not be trusted. However, the whole of section 2.4.5 is based upon it. How can we base the IPCC report on unvalidated model results? [Pepijn Veefkind]	Noted, an assessment of model evaluation is kind of short in this report due to space constraints. Clarified
2-1276	A	29:26		has yet …has not yet [Junying Sun]	Noted; An assessment of model evaluation is kind of short in this report due to space constraints. Clarified
2-1277	A	29:29	30:33	This is well focused, and well written. It speaks clearly to the quantitative aspects and value of the calculations of Direct Radiative Forcing due to sulfate aerosols. [Jerry Mahlman]	Noted, thanks.
2-1278	A	29:29	36:56	In the AERCOM studies the emissions are the same for all the models. For this reason the final result will be strong underestimation, because the uncertainty is only based on differences in the models. Thus, there is no error included for uncertainties of emissions. As an example, in section 2.4.5.2, line 43 it is mentioned that emissions are in the range 10-30 TgCyr-1 for organic carbon from fossil fuel burning. The final AERCOM result mentioned in page 2-31 line 29 is 0.25 +/- W/m2. This uncertainty is only due to model differences, the uncertainty of 50% at least is not in this estimate. This is an example but	Accepted, an error for the emissions will be included in the uncertainty estimate.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				it applies to the whole section 2.4.5, uncertainties are underestimated because the same emissions are used for all models. [Pepijn Veefkind]	
2-1279	A	29:29	36:56	The tables in section 2.4.5 (table 2.4.3, table 2.4.4, table 2.4.5) are extremely hard to understand. The authors should more digest the data and present them in better way using tables and figures. From the current tables the reader has to make his own analysis to understand what is presented. The current presentation suggests that the authors have not fully digested the data themselves. [Pepijn Veefkind]	Accepted,will be reformatted
2-1280	A	29:29		The determination of the aerosol RF as the average of the results from several models would deserve a somewhat larger discussion. In particular, a brief discussion of the possible assimilation of measured data, and a comparison of measured and modeled results, when possible,would help assessing the reliability of estimated RFs. [Alcide di Sarra]	Accepted, a new section is for combined aerosol forcing uncertainty estimate.
2-1281	A	29:29		Section 2.4.5.1 Emissions from EU are described in terms of TgS, USA are described in terms of a percentage change and Asia etc get "significantly increased". Please describe changes in all regions in the same units. [Eleanor Highwood]	Accepted
2-1282	A	29:29		section 2.4.5. comment: I would have liked to find some comments on the radiative forcing efficiency (RF/opt.depth, or RF/burden) in this section. [Christiane Textor]	Noted, Available in table and shortly commented already, 30:22.
2-1283	A	29:31		Section 2.4.5 Given that the range of models used in AEROCOM varied widely in which types of aerosol they include and to what level of sophistication, and that often the range of forcing values from AEROCOM spans the range of non-AERCOM models, is it really meaningful to distinguish between AEROCOM and non-AEROCOM models in the estimates? It feels like just extra numbers to confuse the issue. If there is a valid reason for separating the estimates in this way it should be stated clearly at the start of this section. [Eleanor Highwood]	Noted, AEROCOM results do not include the uncertainty in the emissions, so this has to be factored in. This is now stated explicitly.
2-1284	A	29:31		Evidently, but not explicitly stated, DRF (aerosol direct radiative forcing) is a global annual average, i.e., averaged over 24-hour day, not just daylight hours. It is not clear whether DRF is for cloud free sky and therefore that it needs to be multiplied by factor (1-C) where C is cloud cover fraction to get global mean direct forcing, or whether the reported forcing already takes this into account. These definitions must be explicitly stated. [Stephen E Schwartz]	Accepted, clarifying sentence is included
2-1285	A	29:32	29:33	are 'structural' and 'parametric' uncertainty defined somewhere?	Rejected, see Pan et al 1997.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Christiane Textor]	
2-1286	A	29:34	29:34	'e.g.' is unnecessary in this line. [Tami Bond]	Accepted
2-1287	A	29:35	29:38	Uncertainty calculation is recognized to be insufficient by the authors. With that acknowledgement, why is there not more effort to provide a better estimate? This is not a research project, but a problem requiring synthesis of existing results, which is appropriate to this assessment. [Tami Bond]	Noted. We provide a better assessment than in the TAR, where the direct radiative forcing of each aerosol species was 'guided by the range of published estimates'. Here the use of statistical standard deviations is a better approach.
2-1288	A	29:35	29:35	suggest to change "hygroscopicity, etc." to "hygroscopicity, phase transitions, etc.". This change is in relation to longer comment on chapter 2, page 30, start line 24 (see below) [Scot Martin]	Rejected, too detailed.
2-1289	A	29:36	29:36	what is meant with 'sufficient'? [Christiane Textor]	Accepted, sentence changed
2-1290	A	29:37	29:37	what is meant with 'relative uncertainty'? [Christiane Textor]	Accepted, sentence changed
2-1291	A	29:40	29:33	The sulfate aerosol amounts are typically obtained in the form of sulfate mass densities from chemistry-transport model simulations. A key factor that is poorly determined is the aerosol effective radius - which is essential information that is required in order to calculate the radiative forcing. Undoubtedly, a large fraction of the wide range for the radiative forcings that is reported must be attributable to the uncertainty in aerosol size. [Andrew Lacis]	Rejected. The models used different formulations for aerosol size distribution. Thus reporting the mean and standard deviation incorporates the uncertainty.
2-1292	A	29:40	29:57	This paragraph has little to do with radiative forcing but rather with the aerosol life cycle and should be move to the previous section 2.4.4. [Christiane Textor]	Rejected.
2-2707	B	29:40		I think Pham et al, JGR, 2005 would deserve a citation here. It examines how the patters of sulfate RF will change in the future. [Olivier Boucher]	Accepted
2-1293	A	29:41	29:44	This sentence has a few problems with it. For example, "pure… sulphate aerosol consists of… H2SO4, NH4HSO4, and (NH4)2SO4" does not make chemical sense. I suggest the following replacement sentence: "Atmospheric sulphate aerosol frequently has a chemical composition inbetween NH4HSO4 and (NH4)2SO4 and can be in either aqueous or crystalline form [Martin, 2000]. Sulfate is formed by oxidation of SO2 in gas-phase reactions with hydroxyl radical and in aqueous-phase reactions within cloud droplets." (Martin, S.T., "Phase Transitions of Aqueous Atmospheric Particles," Chemical Reviews,	Accepted, "pure" removed.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				2000, 100, 3403-3453.) [Scot Martin]	
2-1294	A	29:41	29:41	delete 'pure' when talking about ammonium sulfate, pure would be h2so4 to my understanding [Christiane Textor]	Accepted
2-1295	A	29:42	29:42	I do not think that this is the right reference. Better cite an earlier paper on the atmospheric behaviour of sulfur, or one from the COSAM model intercomparison project. [Christiane Textor]	Rejected, numbers in the paragraph are from this citation
2-1296	A	29:44	29:44	I was under the impression that the main source of atmospheric sulphate aerosol was from in-cloud processes. [Graham Feingold]	Accepted. Reordered.
2-1297	A	29:44	29:48	Where do these numbers come from? Give references. [Christiane Textor]	Accepted, reference added Haywood&Boucher 2000
2-1298	A	29:44		How is it known that 72% are from fossil fuel, and etc? (Reference?) [Robert Levy]	Accepted, reference added Haywood&Boucher 2000
2-1299	A	29:46	29:46	… aerosol are from dimethyl "sulfide" emissions … [MARCOS S. P. GOMES]	Accepted, sentence changed
2-1300	A	29:46		dimethyl to dimethyl sulfide [Junying Sun]	Accepted, sentence changed
2-1301	A	29:47	29:48	It is pointed out that emissions range between 66.8 and 92.4 Tg S per year. Is this range due to uncertainties in emission inventory estimates or just the range of emissions within the last two decades? [Johann Feichter]	Accepted, sentence clarified, it meant current emissions.
2-1302	A	29:47	29:47	Change to "estimates of global SO2 mean emissions…" [Joyce Penner]	Accepted, sentence clarified, it meant current emissions.
2-1303	A	29:47	29:47	I assume that it is ESTIMATES of the emissions that range from 66.8 to 92.4 rather than the emissions themselves! [Keith Shine]	Accepted, sentence clarified, it meant current emissions.
2-1304	A	29:47		Global SO2 mean emissions range from 66.8 to 92.4 TgS yr –1 for anthropogenic emissions and from 91.7 to 125.5 TgS yr –1 for total emissions. Should read: Estimates of global SO2 mean emissions range from 66.8 to 92.4 TgS yr –1 for anthropogenic emissions and from 91.7 to 125.5 TgS yr –1 for total emissions. [Stephen E Schwartz]	Accepted, sentence clarified, it meant current emissions.
2-2708	B	29:48	29:48	can we constrain this flux better now? [Olivier Boucher]	Noted, no, unfortunately not.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1305	A	29:48	29:48	Do these estimates of the European decreases include the shipping emissions? [Keith Shine]	Rejected, European emissions refer to land based sources.
2-1306	A	30:1	30:1	Everywhere: should it not be sulfate instead of sulphate? [Christiane Textor]	Rejected, Cambridge English.
2-1307	A	30:4	30:5	suggest to change "with theoretical and experimental data available on the relative humidity dependence of the specific extinction coefficient, fRH (e.g., Tang et al., 1995)" to "with theoretical and experimental data available on the relative humidity dependence of the specific extinction coefficient, fRH (e.g., Tang et al., 1995), of the aqueous sulfate and on the extinction of sulfate crystals (e.g., Tang et al., 1997) and on the relative humidity conditions leading to the crystallization of aqueous sulfate particles and the deliquescence of crystalline sulfate particles (Martin et al. 2003)." (Martin, S.T., Schlenker, J.C., Malinowski, A., Hung, H.M., and Rudich, Y., "Crystallization of atmospheric sulfate-nitrate-ammonium particles," Geophysical Research Letters, 2003, 30, 2102.) (Tang et al. 1997 is given on page 2-90.) [Scot Martin]	Rejected. Too much detail.
2-1308	A	30:5	30:9	The list of measurement campaigns that concentrate on industrial pollution exclude MINOS (2001 campaign), ACE-Asia (2001), APEX (2000-2003) and NEAQS (2003). The lack of discussion of more recent measurement campaigns here is concerning. Taken with the perfunctory comments about in-situ measurements discussed earlier (comment #35), this suggests that the authors are not conversant with the field measurements that have been done since the TAR. This is inauspicious given that this assessment should be reviewing advances in the last five years. [Tami Bond]	Accept, text is changed. APEX, CLAMS, NEAQ included in figure 2.4.2
2-1309	A	30:5	30:5	f(RH) usually refers to a scattering coefficient, and although I realise the authors refer to a non-absorbing aerosol, it would be more accurate to say "scattering" (rather than extinction) – particularly if f(RH) is used elsewhere in its original sense. [Graham Feingold]	Accepted. Changed to scattering.
2-1310	A	30:6	30:7	An important follow-up campaign to TARFOX examined industrial pollution as well. This was called the Chesapeake Lighthouse and Aircraft Measurements for Satellites (CLAMS) campaign and took place in July-August 2001 in the same location as TARFOX. Measurements there indicated that the dry accumulation mode aerosol was dominated by sulphate. I recommend changing from "INDOEX (Ramanathan et al., 2001b) continue" to "INDOEX (Ramanathan et al., 2001b), and CLAMS (Smith et al. 2005) continue" with an additional reference to: Smith Jr., W. L., T. P. Charlock, R. Kahn, J. V. Martins, L. A. Remer, P. V. Hobbs, J. Redemann and C. K. Rutledge, EOS Terra Aerosol and Radiative Flux Validation: An Overview of the Chesapeake Lighthouse and Aircraft Measurements for Satellites (CLAMS) Experiment, Journal of the	Accepted. Campaigns are mentioned in changed text. Refence as well.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Atmospheric Sciences, Vol. 62, No. 4, pp. 903–918, doi: 10.1175/JAS3398.1, 2005. [Brian Magi]	
2-1311	A	30:8	30:9	A study that also clearly shows the importance of sulphate aerosol in accumulation mode mass is one from the CLAMS measurement campaign (discussed in my comment for Ch 2, p. 30, lines 6-7). I therefore recommend changing "Ramanathan et al., 2001b; Quinn and Bates, 2005" to "Ramanathan et al., 2001b; Magi et al., 2005; Quinn and Bates, 2005" with an additional reference to: Magi, B.I., P.V. Hobbs, T.W. Kirchstetter, T. Novakov, D.A. Hegg, S. Gao, J. Redemann, B. Schmid, Properties and Chemical Apportionment of Aerosol Optical Depth at Locations off the U.S. East Coast in July and August 2001, Journal of the Atmospheric Sciences, Vol. 62, No. 4, pp. 919-933, doi: 10.1175/JAS3263.1, 2005. [Brian Magi]	Accepted. Reference added
2-1312	A	30:10	30:10	'internally/externally mixed to varying degrees' A substance is internally or externally mixed, or mixed to varying degrees-- not both. [Tami Bond]	Rejected. Sentence is felt to be clear.
2-1313	A	30:10	30:14	Run on sentence [Robert Levy]	Accepted. Sentence changed.
2-1314	A	30:14	30:14	suggest to change "size distribution, and hygroscopicity" to "size distribution, physical state and morphology, and hygroscopicity". This change is in relation to longer comment on chapter 2, page 30, start line 24 (see below) [Scot Martin]	Accepted.
2-1315	A	30:16	30:31	Particularly the statement 'The reason for these differences is unclear.' This state of affairs is unacceptable. Surely somebody must have investigated the reason for these differences. It is not difficult to compare model assumptions (though it is a bit time-consuming). Can we or can we not identify the effect of aerosol optical properties, hygroscopic growth, etc. on radiative forcing? These uncertainties were present in forcing estimates in the TAR, so it hardly matters that more models have been added if the uncertainties have not been carefully investigated and reduced. This paragraph, and the following identification of direct RF midpoint and uncertainties, suggest that no progress has been made since TAR. [Tami Bond]	Accepted. , a reference to a sensitivity paper is included
2-1316	A	30:19	30:19	It is not necessary to separate "AEROCOM" and "non-AEROCOM" results. "Model results" is more appropriate. [Mian Chin]	Rejected, AeroCom emissions are fixed and model results can be identified this way fur further statistic treatment.
2-1317	A	30:22	30:22	significant' has a statistical meaning, suggest 'substantial' [Reto Knutti]	Accepted
2-1318	A	30:24	30:24	suggest to change "hygroscopicity, etc." to "hygroscopicity, phase transitions, etc.". This change is in relation to longer comment on chapter 2, page 30, start line 24 (see below)	Rejected. Phase transitions do exist. However, they are difficult to quantify.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Scot Martin]	Hygroscopic growth factor measurements show in the majority of situations no hysterisis, when ambient aerosol is under investigation.
2-1319	A	30:24	30:24	Suggest to insert the following sentence before "The direct…". Sentence: "The effect of aqueous versus crystalline sulphate particles on global aerosol direct radiative forcing has recently been assessed and found to lead to up to -0.10 W m^-2 of forcing (Martin et al. 2004)." (Martin et al. 2004 is given on page 2-82.) This suggested sentence is an action item related to a letter I sent earlier and to which I received an excellent reply from Piers Forster. TSU Note: A copy of this email exchange has been cut from this comment and pasted into a review supplementary document. [Scot Martin]	Rejected; See answer to 2-1318
2-1320	A	30:24		Please define "NDRF" here. I don't want to look it up. [Robert Levy]	Accepted.
2-1321	A	30:26	30:27	I strongly recommend to use AEROCOM-A results instead of AEROCOM-B. AEROCOM-A exercise ensembles a large number of current models with results representing the diversity of the processes implemented in those models, representing the range of current knowledge in various processes that determining aerosol RF. In contrast, AEROCOM-B removes one of the most improtant factors responsible for the diversities – emission. In AEROCOM-B, all the models were required to use the same emission datasets. While AEROCOM-B is a very useful exercise to diagnose the factors other than emission causing the model discrepancies shown in exercise A, its results do not represent the current uncertainty range in aerosol RF, because it "artificially" reduces the standard deviation or model discrepancies by using one prescribed emission set in all models. At the minimum, the uncertainties in the current emission inventories should be discussed. [Mian Chin]	Rejected, Unfortunately not possible. Check difference in MEC and residence time between A and B.
2-1322	A	30:32	30:32	Again, the definition of RF is vague – the value is total sulfate DRE, not RF, since the natural sulfate aerosols are also included in the calculation. And the RF is at the top of the atmosphere, not at the tropopause! [Mian Chin]	Accepted. A clearer explanation will be included in the aerosol section introduction or earlier on in the chapter. anthropogenic is implicite
2-1323	A	30:32	30:33	I do not believe that the authors have justified the choice of the limits. Saying that " Based on these results.." is not sufficient. I would argue that the best thing the authors can say is the sulfate forcing (direct) is -0.4 W m-2, but encompass a range from -0.2 to - 1.0. [A. R. Ravishankara]	Rejected. We have used the standard deviation throughout to derive uncertainty estimate.
2-1324	A	30:37	30:54	This paragraph has little to do with radiative forcing but rather with the aerosol life cycle	Rejected. Emissions and burdens are

No.	Batch	Page:line From	Page:line To	Comment	Notes
				and should be move to the previous section 2.4.4. [Christiane Textor]	discussed as basis to derive uncertainty estimate.
2-1325	A	30:37	31:39	This is lengthy and dense, but it captures well the organic carbon aerosol from fossil fuels, and its quantification as a positive radiative forcing agent. The inferred RF seems to be very reasonable. [Jerry Mahlman]	Accepted
2-1326	A	30:41	30:41	Please provide order of magnitude "significant" as well as an explanation as to why this order of magnitude is viewed significant. [Lourdes Maurice]	Accepted
2-1327	A	30:43	30:44	Emission estimates of 10-30 Tg/yr for fossil fuel OC. Authors appear to be unaware of recent work on regional and global emissions of black and organic carbon (e.g. Streets et al. 2003, Bond et al. 2004). This would not be so bad (even though it is my work) except that (a) they differ greatly from previous estimate by Cooke and Liousse (~9 Tg/year as central estimate) and (b) these were the fossil emissions prescribed for the AEROCOM 2000 run. Later in the document, RF due to aerosol from fossil fuel combustion is estimated from AEROCOM results. Combining these with the earlier emission estimates which were not always used for AEROCOM betrays a lack of familiarity with the modeling in that exercise. This is another topic in which the authors display a marked lack of awareness of the work done since TAR. (References: Streets, D.G., T.C. Bond, G.R. Carmichael, S.D. Fernandes, Q. Fu, D. He, Z. Klimont, S.M. Nelson, N.Y. Tsai, M.Q. Wang, J.-H. Woo, and K.F. Yarber (2003). An inventory of gaseous and primary aerosol emissions in the year 2000. J. Geophys. Res. 108 (D21): 8809, doi:10.1029/2002JD003093; Bond, T.C., D.G. Streets, K.F. Yarber, S.M. Nelson, J.-H. Woo, and Z. Klimont (2004). A technology-based global inventory of black and organic carbon emissions from combustion. J. Geophys. Res. 109 : D14203, doi:10.1029/2003JD003697.) [Tami Bond]	Accepted. The references were indeed forgotten, but now included Emissions from Bond et al had been used to construct the AEROCOM emissions, and this refrence is now cited.
2-2709	B	30:43	30:43	Emissions of PRIMARY organic aerosols [Olivier Boucher]	Accepted.
2-1328	A	30:43	30:44	An updated organic carbon inventory was published in 2004 and offers not only updated emissions estimates, but a immense library of references. I recommend changing "Emissions of organic carbon from fossil fuel burning have been estimated to be 10 to 30TgCyr-1 (Liousse et al., 1996; Cooke et al., 1999; Scholes and Andreae, 2000)." to "Emissions of organic carbon from fossil fuel burning have been estimated to be 5 to 17TgCyr-1 (Bond et al., 2004)." This removes references to Liousse et al. 1996, Cooke et al. 1999, and Scholes and Andreae, 2000 and would require adding a new reference to: Bond, T.C., D.G. Streets, K.F. Yarber, S.M. Nelson, J. Woo, Z. Klimont, A technology-	Accepted in part. Note that the emissions strictly from fossil fuels are only ~2.4TgCyr-1.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				based global inventory of black and organic carbon emissions from combustion, JGR, Vol. 109, D14203, doi:10.1029/2003JD003697, 2004. The emissions estimate of 5-17 TgC yr-1 that I quote from the Bond et al 2004 study comes from Table 12. [Brian Magi]	
2-1329	A	30:43	30:44	Emissions of OC from FF burning by Bond et al. (J.G.R, 2004) are only 2.37 Tg/yr (see also Ito and Penner (Global Biogeoch. Cycles, 2005) [Joyce Penner]	Accepted.
2-1330	A	30:44	30:46	I rather doubt that inferring trends of FFOC from emissions of NMVOC's works. The sources are not necessarily the same and changes for OC are particularly important because of changes in burning technologies. [Joyce Penner]	Accepted. Sentence removed.
2-2710	B	30:45	30:45	non-methyl or non-methane [Olivier Boucher]	Accepted.
2-1331	A	30:45	30:45	non-methyl…" -> "non-methane… [Xiaobin Xu]	Accepted.
2-1332	A	30:46	30:48	Trends on organic carbon emissions are given for Europe. If similar information is available for other industrialized countries it should be given (USA, Japan for instance). On line 47, the reference to Europe should be repeated in the sentence:" Thus the reduction in organic carbon is less dramatic than that of sulfur dioxide". [Philippe Tulkens]	Noted. However the emission inventories are not readily available.
2-2711	B	30:47	30:47	what do you mean by "dramatic"? [Olivier Boucher]	Noted. Sentence removed.
2-1333	A	30:47	30:47	The reference to Europe should be repeated in the following sentence:"Thus the reduction in organic carbon is less dramatic than that of sulfur dioxide". [Philippe Tulkens]	Accepted, but sentence removed.
2-1334	A	30:47	30:47	"(Vestreng and al, 2004)" -> "(Vestreng et al., 2004) [Xiaobin Xu]	Accepted.
2-1335	A	30:48	30:52	The TARFOX campaign did not make measurements that could differentiate fossil-fuel generated organic aerosol from organic aerosol from other sources. During TARFOX, total organic aerosol concentrations were similar to sulfate aerosol concentrations. It is misleading to describe this as fossil aerosol. [Tami Bond]	Accepted. Modification of words.
2-1336	A	31:0	33:0	I am impressed by the current achievements in the RF quantification of black carbon from fossil fuels, and that of black carbon to come up with a quite small, but intuitively straightforward RF for the nitrate aerosol. Its short atmospheric lifetime virtually demand a small number, with generous error bars.	Noted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Jerry Mahlman]	
2-1337	A	31:1	31:39	There are very, very large uncertainties and unknowns in organic carbon aerosols. The composition has not been measured extensively and the new information on the secondary organic aerosols (SOA) has raised many new questions. These large uncertainties should be adequately acknowledged here. The uncertainty range given here is largely underestimated. The AEROCOM results of OC also include biogenic OC, which is not anthropogenic. By definition, the results here is not RF but DRE. [Mian Chin]	Noted, regarding the first part a sentence is added. In the AEROCOM B and PRE emission data biogenic OC is included and thus in the RF (difference between these simulation) it is not included
2-1338	A	31:7	31:7	Suggest to append this sentence: "The effects of organic material on the crystallization of sulphate particles have been recently investigated (e.g., Parsons et al. 2004)." This suggestion is in relation to longer comment on chapter 2, page 30, start line 24 (see above). (Parsons MT, Knopf DA, Bertram AK, Deliquescence and crystallization of ammonium sulfate particles internally mixed with water-soluble organic compounds, JOURNAL OF PHYSICAL CHEMISTRY A 108 (52): 11600-11608 DEC 30 2004) [Scot Martin]	Noted.
2-1339	A	31:8	31:11	The sentence is not clear and a reference is missing, proposed change: "Attempts have been made to formulate organic carbon composition by functional group analysis in some main classes of organic species (e.g., Decesari et al., 2000; Decesari et al., 2001; Maria et al., 2002; e.g., Ming and Russell, 2002).capturing some general characteristics in terms of e.g., refractive indices, hygroscopicity, and cloud activation properties : this would facilitate their implementation in climate models (Fuzzi et al., 2001)." [MARIA CRISTINA FACCHINI]	Accepted.
2-1340	A	31:11	30:11	Delete the second "e.g.," in this line. [Xiaobin Xu]	Accepted.
2-1341	A	31:12	31:18	There are results of mixed aerosol optics from laboratory work, not just theoretical (see Schnaiter, M., H. Horvath, O. Möhler, K.-H. Naumann, H. Saathoff, and O. Schöck (2003). UV-VIS-NIR spectral optical properties of soot and soot-containing aerosols. J. Aerosol Sci 34 (10): 1421-1444, and the rest of that JAS issue.) Again, I find it disconcerting that almost no work since TAR is cited here. [Tami Bond]	Accepted. Now cited.
2-1342	A	31:16	31:16	…Jacobson, 2001) However coating…" -> "…Jacobson, 2001). However, coating… [Xiaobin Xu]	Accepted.
2-1343	A	31:16		a full stop is missing after the bracket. [Hartmut Grassl]	Accepted.
2-1344	A	31:24	31:39	comment: at tropopause ? [Hartmut Grassl]	Noted. Now noted in 2.4.1 that the toa and the tropopause radiative forcing do

No.	Batch	Page:line From	Page:line To	Comment	Notes
					not differ by much.
2-2712	B	31:29	31:29	-0.40 or -0.37 ? [Olivier Boucher]	Accepted.
2-1345	A	31:30	31:32	No justification is given for the 0.25:0.75 split. It is not a bad split, in my opinion; but given that these numbers will probably be propagated all the way to the much-touted bar chart, far more justification should be given. The contribution of secondary biogenic carbon is not discussed; was it used at all? [Tami Bond]	Accepted, reference to Bond et al. (2004) now included.
2-1346	A	31:37	31:39	Given all the uncertainties of primary and secondary aerosol and the source apportionment I do not feel that this is an accurate estimate of the uncertainty. Which models are including secondary aerosol? [Tim Bates]	Accepted, Best estimate and uncertainty range changed
2-1347	A	31:38	31:39	'...owing to difficulties in constraining…' Presumably you mean that the relatively large uncertainty is due to the difficulties in constraint-- not your estimate. [Tami Bond]	Accepted. Reworded.
2-1348	A	31:43	36:38	This is even more lengthy and dense, but it tells an important story about the RF role of organic carbon and black carbon from fossil-fuel burning. [Jerry Mahlman]	Noted
2-1349	A	31:44	31:51	Here *no* emission studies are cited except the one by Novakov-- which was very simplistic. Even citing the older, original studies (Penner, Cooke, Liousse) would have been better than this discussion, but again there's no inclusion of present-day emission inventories (Bond 2004 etc.) as used in AEROCOM. Furthermore, the statement (line 50) '...owing to the rapid expansion of the Chinese and Indian economies...' could be politically charged. The U.S. and Europe also experienced strong economic expansion during this period. However, their period of early industrialization without emission controls occurred from 1900-1950. Emission controls occurred post-1950. [Tami Bond]	Accepted. The emission estimates summarised in Haywood and Boucher are presented, and the most recent study from Bond et al, 2004, and Ito and Penner 2005. The senetence that was "politically charged" has been re-written.
2-1350	A	31:44	31:53	This paragraph has little to do with radiative forcing but rather with the aerosol life cycle and should be move to the previous section 2.4.4. [Christiane Textor]	Rejected.
2-1351	A	31:44	31:44	insert "the" before "source" (syntax) [Peter Van Velthoven]	Accepted.
2-1352	A	31:45	31:51	The recent study by Ito and Penner (JGR, 2005) on historical emissions of BC and POM should also be mentioned here. Their results are somewhat different from those of Novakov et al. [Leon Rotstayn]	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1353	A	31:50	31:50	Change "2.2 to 6.7 TgBC" to "2.2 to 6.7 TgC" [Brian Magi]	Accepted.
2-1354	A	31:50	31:51	…(e.g. Streets and al, 2001)…" -> "…(e.g., Streets et al., 2001)… [Xiaobin Xu]	Accepted.
2-1355	A	31:51	31:53	What has the information about electron microscope images to do with the rest of the paragraph? [Tami Bond]	Accepted.
2-2713	B	31:51	31:51	et al [Olivier Boucher]	Accepted.
2-1356	A	31:51		correct: "and" to "et al." [Hartmut Grassl]	Accepted
2-1357	A	31:53	31:53	remove "down" [Graham Feingold]	Accepted
2-1358	A	31:53		delete: "down" [Hartmut Grassl]	Accepted
2-1359	A	32:2	32:15	Again, this paragraph summarizes information that was available at TAR or shortly thereafter. What progress has been made since then? At least, what can be learned from AEROCOM? [Tami Bond]	Noted. We feel that we have to include a brief summary of why BC aerosol is important re. Difference in the TOA and the surface forcing, the effect of aerosol absorption above cloud etc.
2-1360	A	32:2	32:35	Line 32 on Page 2-35: Liao et al. [2004] is missing from the reference list. [John Seinfeld]	Accepted, paper included in the reference list.
2-1361	A	32:17	32:17	Perhaps remind the reader of what the TAR estimate for BC was? [Keith Shine]	Accepted.
2-1362	A	32:21	32:24	In fact, the column loadings in AEROCOM (Table 2.4.4) appear to be *lower* than those of previous studies-- perhaps due to the lower emissions. But the radiative forcing is higher. If the mixing state is an important component of this finding, then the mixing states of the different models should be identified. [Tami Bond]	Rejected, AEROCOM BC RF is lower than previous studies
2-1363	A	32:21	32:21	…W m^-2, and 0.75+/-0.46 W m^-2 respectively." -> "…W m^-2 and 0.75+/-0.46 W m^-2, respectively. [Xiaobin Xu]	Accepted.
2-2714	B	32:24	32:24	You may also want to refer to the new Stier et al work (GRL, 2005, submitted) which shows interesting non-linearities in sulfate and BC RFs. [Olivier Boucher]	Rejected, this paper has not been made available to us within IPCC deadlines
2-1364	A	32:25	31:25	Source emission inventories" -> "Emission inventories	Accepted, taken into account

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Xiaobin Xu]	
2-1365	A	32:25	32:27	Source emission inventories don't 'continue to' suggest a 50:50 split between biomass and fossil-fuel burning sources. That is, no post-TAR references have been included which include both biomass and fossil fuel; Novakov reported only fossil fuel. So in the sense that the pre-TAR papers have not been re-published, the sentence is correct; those papers do continue to suggest a 50:50 split. Bond et al. (2004) suggest 40:40 for BC from fossil and biomass; so the equal split may continue to hold up. However, in this entire discussion, the contribution of biofuel is missing. It differs from biomass emissions and contributes about 20% of both BC and OC (if you believe Bond 2004). I believe it was a mistake to lump this source with biomass emissions in the TAR and it would be a mistake to do it again. [Tami Bond]	Accepted, taken into account
2-1366	A	32:26	32:26	Bond et al. (JGR, 2004) suggest only 38% of BC is fossil fuel, while Ito and Penner (GBC, 2005) have only 34%. [Joyce Penner]	Accepted, taken into account
2-1367	A	32:30	32:30	…Takemura et al., (2000)" -> "…Takemura et al., 2000) [Xiaobin Xu]	Accepted
2-2715	B	32:32	32:33	I think it is worth mentioning that this excludes the snow effect and the semi-driect effect [Olivier Boucher]	Ok
2-1368	A	32:35	33:39	There are several misleading issues in section 2.4.5.4, "Biomass burning aerosols". First, biomass burning aerosols include OC, BC, and sulfate. Their RF should be separated. Second, not all biomass burnings are anthropogenic. There are natural wildfires especially in the boreal regions. By definition, again, this is DRE, not RF. Third, biomass burning emission is probably the largest uncertainty in modeling biomass burning aerosols with a factor of at least 4 discrepancies across the emission estimates. The uncertain in total RF by biomass burning has to be more than that in the emission. [Mian Chin]	Noted, it is already included that sulphate is a compound in biomass burning aerosols. We disagree that RF should be taken separately since biomass burning aerosols mainly are internal mixed. We added a sentence on emission data for biomass burning aerosols. It is RF and DRE since the model estimates assume a pre-industrial emission of biomass burning aerosols
2-1369	A	32:35	33:55	a discussion of the temporal evolution of source strength and distribution is missing. [Johann Feichter]	Noted, a sentence on the pre-industrial level of emission is included
2-1370	A	32:35		Section: Biomass burning aerosols. There are two recently published review papers on the physical and optical properties of biomass burning aerosols, which should be cited in this chapter: J. S. Reid, R. Koppmann, T. F. Eck, D. Eleuterio A Review of Biomass Burning Emissions Part II: Intensive Physical Properties of Biomass Burning Particles Atmospheric Chemistry and Physics, 5, 799-825, 2005.	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				J. S. Reid, T. F. Eck, S. A. Christopher, R. Koppmann, O. Dubovik, D. Eleuterio, B. N. Holben, E. A. Reid, J. Zhang A Review of Biomass Burning Emissions Part III: Intensive Optical Properties of Biomass Burning Particles Atmospheric Chemistry and Physics, 5, 827-849, 2005. [Ralf Koppmann]	
2-1371	A	32:35		Section 2.4.5.4: A clear definition of "biomass burning" appears to be missing. Does this refer to burning of standing vegetation (forest/savannah burning), or also the burning of biofuels for e.g. cooking? While in this section there is detailed information on the radiative properties of biomass burning aerosols, there is still considerable uncertainty in the emission extimates. Also, there is much less information available on the change in biomass burming emissions over the past decades, in contrast to the quite detailed time-dependent emission inventories for the fossil fuel burning. As in this report radiative forcing is presented as the difference in radiation fluxes between 2004 and 1750, this problem should be discussed - here it is apparently implicitely assumed that the biomass burning is purely anthropogenic (which is certainly not the case for boreal forest fires) and would have been close to zero 250 years ago. This needs to be discussed. [Ina Tegen]	Good point
2-1372	A	32:36		Section 2.4.5.4 I am glad to see a justification for the way in which different components are combined differently for fossil fuel and biomass burning aerosols [Eleanor Highwood]	Noted, thanks
2-1373	A	32:39	32:41	The sentence is unclear [MARCOS S. P. GOMES]	Rejected
2-1374	A	32:43	32:44	This statement is not correct. It is generally not possible to reduce black carbon and organic carbon separately from a single source. [Tami Bond]	Rejected
2-1375	A	32:43	32:47	Add at the end of the sentence "uncontrolled in the case of open fire and primitive small scale stoves. However, the emissions of OC, BC and inorganic compounds are as well controlled as by fossil fuels in case of modern biomass boilers". [Ilkka Savolainen]	Rejected, too detailed
2-1376	A	32:57	32:57	al., 2003; e.g., Keil…" -> "al., 2003; Keil… [Xiaobin Xu]	Accepted
2-1377	A	33:1	33:14	It is not clear what RF is under discussion, global mean or local? RF (in general) was defined as global mean, but sometimes the authors use local RF. The same problem appears later on on page 34. [Eugene Rozanov]	Accepted, it has now been stated earlier in the aerosol section that RF is global mean TOA radiative forcing, otherwise not explicitly stated

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1378	A	33:4		before "Kirchstetter, 2005" add "Taubman et al, 2004" [Zhanqing Li]	Rejected, sufficent citations added
2-1379	A	33:4		after "Kirchstetter, 2005" add "Vant-Hull et al, 2005" [Zhanqing Li]	Rejected, sufficent citations added
2-1380	A	33:5		I suggest we include in this session the existence of remarkable clean air slots in southern Africa found during SAFARI 2000 and reported by Peter V. Hobbs. In addition to that is the previous finding by Cosijn and Tyson (1996) about semi-permanent absolutely stable layers occurring around 850, 700, 500 and 300 hPa levels in the troposphere of southern Africa. I think this can be taken in account when analysing the direct aerosol forcing in this region. [Antonio Joaquim Queface]	Rejected, we find this interesting but due to space constraint we are not including a description of this
2-1381	A	33:7	33:7	Other determinations of the single scattering albedo of biomass burning aerosol exist. For example, Meloni et al. (Meloni, D., A. di Sarra, G. Pace, and F. Monteleone, Optical properties of aerosols over the central Mediterranean. 2. Determination of single scattering albedo at two wavelengths for different aerosol types, Atmos. Chem. Phys. Discuss., 5, 4971-5005, 2005) derive in the Mediterranean average values of 0.82 at 415.6 nm, and 0.8 at 868.7 nm. [Alcide di Sarra]	Noted, too detailed
2-1382	A	33:7		after (Haywood et al., 2003b). add "Biomass burning aerosols produced by boreal forest fires tend to have weaker absorption than those from tropical fires (single scattering albedo greater than 0.9) (Li and Kou, 1998; Wong and Li 2001). During a peak month of biomass burning in the Canadian boreal forest region, the aerosol DRF at the surface accounts for one third of a total reduction of solar energy by both clouds and aerosols (Li and Kou 1998)." [Zhanqing Li]	Noted, too detailed
2-1383	A	33:12	33:12	... As for industrial aerosols which contain... [MARCOS S. P. GOMES]	Accepted
2-1384	A	33:13	33:13	... absorbing black carbon particles ... [MARCOS S. P. GOMES]	Accepted
2-1385	A	33:17	33:17	... and optical properties as well as the vertical ... [MARCOS S. P. GOMES]	Accepted
2-1386	A	33:18		After "Penner et al. 2003)" add "although different methods obtaining the aerosol optical and physical properties still suffer from substaintial inconsistency. For example, Vant-Huall et al. (2005) found that the single scattering albedo measured by an airborne in-situ instrument differ from that inferred from the ground-based retrieval, leading to differences in the estimates of AOT from MODIS by 22-40% and aerosol DRF at the surface by 10%.	Rejected, too detailed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Zhanqing Li]	
2-1387	A	33:20	33:20	sentence is unclear: " ... when biomass burning aerosol overlies cloud than ..." [MARCOS S. P. GOMES]	Rejected, we think the sentence is clear
2-1388	A	33:22	33:22	…In addition, to modelling" -> "…In addition to modelling [Xiaobin Xu]	Accepted
2-1389	A	33:25	33:25	... lifted above the clouds ... [MARCOS S. P. GOMES]	Accepted
2-1390	A	33:32	33:33	Suggestion " ... The biomass burning aerosol may exert a significant positive direct RF when dispersed above clouds. This is documented by ..." [MARCOS S. P. GOMES]	Accepted
2-1391	A	33:32	33:39	comment: too vague to give a mean ! [Hartmut Grassl]	Noted, sentence rewritten
2-1392	A	33:32	33:39	How confident are you that the forcing should be > 0?? It seems that there are only 2 models in table 2.4.2 that have actually calculated the biomass burning forcing with a value > 0. At least for the Reddy et al. model, the large positive forcing appears to be due to the fact that they treated BC as externally mixed from the POM associated with biomass aerosols. Moreover, you derive a positive forcing for biomass aerosol of 0.12 for the Chuang et al., 2002 model from your "formula" which I know is not correct when that model actually calculates the forcing for biomass directly (as was done in Penner et al., 1998). Thus, it seems to me that by using the formula to derive the forcing for biomass aerosols, you are biasing the numbers to be positive. While placement of biomass aerosols higher in the atmosphere can certainly lead to a less negative forcing, even the paper you quote (Penner et al., 2003) still found a negative forcing (-0.03 W/m2) from biomass aerosols when they were placed higher in the atmosphere. [Joyce Penner]	Accepted, the best estimate is changed
2-1393	A	33:32	33:33	Rephrase for better readability. [Christiane Textor]	Accepted
2-1394	A	33:35	33:35	Break the sentence in " ... deviation is 0.08 W m-2. Hence, even the sign ..." [MARCOS S. P. GOMES]	Accepted
2-1395	A	33:38	33:38	... significantly different than ... [MARCOS S. P. GOMES]	Accepted
2-1396	A	33:41	33:41	Considering the now rather large size of the nitrate aerosol forcing, I found this section somewhat weak and unconvincing. Unlike other sections (e.g. for sulphate) we are not told anything about the sources of nitrate aerosols or their uncertainties. It is also extremely unclear where your central value of 0.15 comes from, as it is so much higher than 2 of the available estimates. . The final sentence makes me wonder whether you	Accepted, best estimate changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				should back off a little on this - it has knock on consequences later in the synthethis, where the caveats may be lost. [Keith Shine]	
2-1397	A	33:42	33:55	You should realize that Jacobson (2001) used an equilibrium model to calculate nitrate in aerosol which is incorrect for larger particle sizes and Adams et al 2001 ignored the interaction of nitrate with large particles. I have a paper in review showing the problems with these approaches but it did not make it to submission before the deadline. [Joyce Penner]	Accepted, The best estimate is changed from -0.15 to -0.10
2-1398	A	33:46	33:46	Suggest to change "aerosol" to "aerosol, and of Martin et al. (2004), who report -0.043 to -0.076 W m^-2 for global mean RF by nitrate." (Martin et al. 2004 is given on page 2-82.) [Scot Martin]	Accepted
2-1399	A	33:47	33:47	suggest to change "hygroscopicity" to "hygroscopicity and phase transitions". This change is in relation to longer comment on chapter 2, page 30, start line 24 (see above). [Scot Martin]	Rejected
2-1400	A	33:48	33:49	The sentence is unclear: "None of the models .... making an initial global estimate possible (or impossible?) " [MARCOS S. P. GOMES]	Accepted, sentence changed
2-1401	A	33:48	33:49	The sentence beginning " None of the models participating in AEROCOM…. " does not make sense [Eleanor Highwood]	Accepted, sentence changed
2-1402	A	33:48	33:48	PROOFREADING TYPE COMMENT: Substitute "None" with "Nine"... [Malte Meinshausen]	Accepted, sentence changed
2-1403	A	33:48	33:48	The aerosol section is excellent. A typo - should 'None of the models' be 'Nine of the models'? [Susan Solomon]	Accepted, sentence changed. Thanks
2-1404	A	33:48	33:49	Should "None" be "Nine"? [Hongbin Yu]	Accepted, sentence changed
2-1405	A	33:49	33:49	Don't you mean '…making an initial global estimate IMpossible?' [Tami Bond]	Accepted, sentence changed
2-1406	A	33:51	33:51	… this estimate is necessarily … [Rolf Philipona]	Accepted
2-1407	A	33:51		correct: "estimate in" to "estimate is" [Hartmut Grassl]	Accepted
2-2716	B	33:53	33:53	Is this 1-sigma? It would be useful to say where and under which chemical composition the nitrate is found. [Olivier Boucher]	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1408	A	34:1	34:5	. I was surprised by the large values that TAR estimated for the RF due to mineral dust. It seems to be quite counter-intuitive. The new value given in this assessment document of-0.2 to +0.1W/m2 seems to be much more plausible, even though I find it hard to support a +0.1 W/M2 value, or any net globally positive value, for that matter. [Jerry Mahlman]	Noted.With regard to the +0.1 W/M2 value: Some models compute a positive forcing, because absorption of dust above reflecting surfaces such as desert may result in atmospheric forcing and thus positive TOA forcing.
2-1409	A	34:1	34:57	add one reference paper from WANG Hong et al., Chinese Science Bulletin 2004 Vol. 49 No. 20 2212—2219.Radiative forcing due to dust aerosol over east Asia-north Pacific region during spring, 2001 [Junying Sun]	Rejected, because of space cnstraints and a missing indication on how this paper sheds new light on the dust forcing in the region.
2-1410	A	34:1		In the mineral dust section no mention is made of the effect of the dust vertical distribution on the radiative forcing. The dust vertical distribution is in general very different from the commonly assumed climatological profiles, and this effect may be important. Meloni et al. (2005) discuss this aspect, showing that the aerosol radiative forcing at the surface has a very small dependency on the aerosol vertical pro?le. At the top of the atmosphere, the radiative forcing is weakly dependent on the vertical pro?le (up to 10% variation on the daily average forcing) for low absorbing particles; conversely, it shows a strong dependency (the daily radiative forcing may vary up to 100%) for absorbing particles. A similar analysis, for aerosols measured during INDOEX, has been carried out by Wagner et al. (2001). Reference: Meloni, D., A. di Sarra, T. Di Iorio, and G. Fiocco, Influence of the vertical profile of Saharan dust on the visible direct radiative forcing, J. Quant. Spectrosc. Radiat. Transfer, 93, 347-413, 2005. Wagner, F., D. Mueller, and A. Ansmann, Comparison of the radiative impact of aerosols derived from vertically resolved (lidar) and vertically integrated (Sun photometer) measurements: Example of an Indian aerosol plume, J. Geophys. Res., 106, 22,861–22,870, 2001. [Alcide di Sarra]	Acepted, uncertainty discussion on vertical distribution requires being added.
2-1411	A	34:1		The executive summary (p.5 l.19-21) notes the importance of aerosol radiative forcing to the hydrologic cycle. However, the present section on dust overlooks the importance of dust radiative forcing at the surface, which (as with forcing by other absorbing aerosols) can be much larger than the TOA value. Surface forcing by dust is shown to reduce evaporation and weaken the hydrologic cycle (Miller and Tegen 1998), although it can cause local increases in precipitation by acting as an elevated source of diabatic heating (Miller et al JGR 2004, Miller et al GRL 2004). Thus, forcing at the top of the	Noted, however this chapter is not supposed to assess the climate feedback through eg the hydrological cycle. See chapter 7 for the appropriate dicussion of this effect.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				atmosphere does not entirely determine the climate response. Miller, R. L., and I. Tegen (1998), Climate response to soil dust aerosols, J. Clim., 11, 3247-3267. Miller, R. L., J. Perlwitz, and I. Tegen (2004), Modeling Arabian dust mobilization during the Asian summer monsoon: The effect of prescribed versus calculated SST, Geophys. Res. Lett., 31, L22214, doi:10.1029/2004GL020669. Miller, R. L., I. Tegen, and J. Perlwitz (2004), Surface radiative forcing by soil dust aerosols and the hydrologic cycle, J. Geophys. Res., 109, D04203, doi:10.1029/2003JD004085. [Ron Miller]	
2-1412	A	34:1		subsection 2.4.5.6. comment: I find this section not well structured. Reorder for better readability. [Christiane Textor]	Noted
2-1413	A	34:2	34:4	Mineral dust from anthropogenic sources originates mainly from agricultural practices (harvesting, ploughing, desertification due to over-grazing etc) and industrial practices (e.g., cement production, transport etc.)." Somewhere here, water use needs to be mentioned. No one has quantified the impacts of water use on dust emissions, but it is obviously important in some regions (Caspian and Aral Sea, Lake Owen). The water usages aren't solely for agricultural practices, but also for population consumption, so it might be better to put water usage separate from agricultural practices. (Prospero et al., 2002). I would suggest the following: "Mineral dust from anthropogenic sources originates primarily from agricultural practices (e.g. harvesting, ploughing, over-grazing), changes in surface water (e.g. Caspian and Aral Sea, Owens Lake), and industrial practices (e.g. cement production, transport). [Natalie Mahowald]	Accepted
2-1414	A	34:4	34:4	…in the range +0.4 to -0.6 W" -> "…in the range of +0.4 to -0.6 W [Xiaobin Xu]	Accepted
2-1415	A	34:6	34:6	... competing effects of the shortwave ... [MARCOS S. P. GOMES]	Taken into account, rewritten.
2-1416	A	34:9	34:9	... contribution of mineral dust ... [MARCOS S. P. GOMES]	Rejected
2-1417	A	34:9	34:22	comment: too vague [Hartmut Grassl]	Taken into account. Paragraph is partly rewritten.
2-1418	A	34:9	34:22	The change in dust anthropogenic component from TAR is significant and large. Although the different studies are discussed and ranges given, it is not fully justified why a value of 10% is chosen. [Eleanor Highwood]	Taken into account, paragraph is partly rewritten.
2-1419	A	34:9	34:22	This paragraph made no sense to me	Taken into account, paragraph is partly

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Robert Levy]	rewritten.
2-1420	A	34:9	34:24	"Tegen and Fung (1995) estimated the anthropogenic contribution to mineral dust to be 30–50% of the total dust burden in the atmosphere. Tegen et al. (2004) provided a more detailed estimate by comparing observations of visibility as a proxy for dust events from over 2000 surface stations with model results and suggest that only 5–7% of mineral dust comes from anthropogenic agricultural sources. However, Mahowald (2004) uses the same observational data and contests these finding suggesting that up to 0–50% of dust may be of anthropogenic origin. However, Tegen et al. (2005) perform some further sensitivity studies by reducing the model threshold friction velocity for dust production and suggest that the model produces too many mineral dust storms if the anthropogenic fraction exceeds 15% and suggest therefore that this is an upper limit. Long-term aerosol size distributions retrieved by AERONET (Dubovik et al., 2002) suggest that there is a distinct coarse mode observable at a limited number of industrial sites, but the contribution to the aerosol optical depth from this mode is negligible compared to that from the more optically active sulphate, black carbon, organic carbon and nitrate that make up the accumulation mode. Thus there remains considerable uncertainty with respect to the anthropogenic component of mineral dust but we revise the estimate to 10%." This paragraph is inconsistent with the literature and should be rewritten. It misses the important point that desert dust could be decreasing due to humans (largely due to carbon dioxide fertilization), and overstates the conclusions of one paper relative to the many other papers published on this topic. More specifics: 1. "However, Tegen et al. (2005) perform some further sensitivity studies by reducing the model threshold friction velocity for dust production and suggest that the model produces too many mineral dust storms if the anthropogenic fraction exceeds 15% and suggest therefore that this is an upper limit." This is not a 'further sensitivity study', it is just doing a statistical significance test of the model/data comparison from the 2004 study, and thus is a model dependent result, based on the results of Mahowald et al., 2004. This statement should be removed from the text. Already in Mahowald et al., 2002 and Luo et al., 2003, it is shown that you cannot tell the difference between concentrations and optical depths in cases with 50% land use and with 0% land use downwind from the source, and even over the source areas. The results from Tegen et al., 2004 and Tegen et al., 2005 are model dependent results—very different results are obtained by other authors in the peer reviewed literature. The most accurate reflection of the literature, as reflected in a recently accepted review article (coauthored by the relevant parties) is that the uncertainty is between 0 and 50% (Mahowald et al., in press) (http://www.cgd.ucar.edu/tss/staff/mahowald/papers/Mahowaldetalironreviewaccepted.pdf). 2. An additional sentence should be added, including the impacts of anthropogenic climate change on dust. Desert dust is known to be very sensitive to changes in climate—	Taken into account The uncertainty range is now increased. However not to the full range given by Mahowald et al. We disagree that the statistical analysis in the comment of Mahowald et al to Tegens work is a good argument to assume that dust can be up to 50% anthropogenic. This report is providing an assessment and can not resolve the ongoing scientific discussion on the anthropogenic fraction of mineral dust. The paragraph is partly rewritten to better reflect these uncertainties. It should be noted that a change in vegetation cover due to carbon dioxide fertilization is not considered as radiative forcing. Note also that the proposed large impact of climate change on dust fluxes (proposed to be larger than that of land use changes – see discussion in chapter 7) would be inconsistent with a large anthropogenic perturbation of todays dust fluxes. Such feedback discussion is to be found in chapter 7 of this report.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				as the ice core variations illustrate (e.g. Petit et al., 1990). The only study which looks at changes between pre-industrial to current climate dust is Mahowald and Luo, 2003, which shows that relative to pre-industrial climate, the current dust loading is 24% lower than preindustrial to 63% higher than preindustrial, depending on the relative importance of land use, precipitation shifts, surface wind changes and carbon dioxide fertilization. Thus, in actuality, the uncertainty in anthopogenic forcing of desert dust has expanded from previous estimates and can be an increase or a decrease in desert dust. Even if you cut the land use uncertainty to an arbitrary 10%, you would still have a change between 24% lower than preindustrial and ~26% higher than preindustrial in the current climate (estimating from the scenarios in Mahowald and Luo, 2003). 3. The AERONET studies do not conclusively show anything about the anthropogenic dust emission amounts, and so this sentence should be deleted from the paragraph. 4. I would propose the following paragraph more accurately reflects the literature. Of course, the resulting uncertainties in mineral aerosols (especially the fact that it could actually be lower than in pre-industrial) need to be propogated into other sections. "Tegen and Fung (1995) estimated the anthropogenic contribution to mineral dust to be 30–50% of the total dust burden in the atmosphere. Tegen et al. (2004) provided an alternative estimate by comparing observations of visibility as a proxy for dust events from over 2000 surface stations with model results and suggest that only 5–7% of mineral dust comes from anthropogenic agricultural sources. However, Mahowald et al. (2004) uses the same observational data and contests these finding suggesting that up to 0–50% of dust may be of anthropogenic origin. Additionally, mineral aerosols likely respond to anthropogenic climate change and changes in carbon dioxide. The only study which addresses this issue (Mahowald and Luo, 2003) suggests that anthropogenic influences cause mineral aerosols to increase by up to 60% or decrease by 24% relative to the preindustrial climate, depending on the relative importance of land use, climate change and carbon dioxide fertilization, all processes which are poorly understood. Thus there remains considerable uncertainty with respect to the anthropogenic component of mineral dust but we revise the estimate to –24% to +60%." Citations: Luo, C., N. Mahowald, and J. d. Corral (2003), Sensitivity study of meteorological paramters on mineral aerosol mobilization, transport and distribution, Journal of Geophysical Research, 108, 10.1029/2003JD0003483. Mahowald, N., C. Zender, C. Luo, J. d. Corral, D. Savoie, and O. Torres (2002), Understanding the 30-year Barbados desert dust record, Journal of Geophysical Research, 10.129/2002JD002097; Mahowald, N., A. Baker, G. Bergametti, N. Brooks, R. Duce, T. Jickells, N. Kubilay, J. Prospero, I. Tegen, The atmospheric global dust cycle and iron inputs to the ocean, accepted in GBC. (Available at: www.cgd.ucar.edu/tss/staff/mahowald);Mahowald, N. M., and C. Luo (2003), A less dusty future? Geophysical Research Letters, 30, 1903, doi: 1910.1029/2003GRL017880.;	

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Petit, J. R., L. Mounier, J. Jouzel, Y. S. Korotkevich, V. I. Kotlyakov, and C. Lorius (1990), Palaeoclimatological and chronological implications of the Vostok core dust record, Nature, 343, 56-58. [Natalie Mahowald]	
2-1421	A	34:9	34:22	This paragraph has little to do with radiative forcing but rather with the aerosol life cycle and should be move to the previous section 2.4.4. [Christiane Textor]	Rejected. Radiative forcing by definition requires an identification of the anthropogenic contribution to the mineral dust loads. This is discussed here.
2-1422	A	34:9	34:22	The FOD correctly revises downward the contribution of anthropogenic dust attributable due to land use change. This is based largely on studies by Tegen et al. who use observed dust storm frequency (DSF) to constrain model anthropogenic dust emissions. The FOD mentions, but does not seem to accept, Mahowald et al.'s result that using Tegen's DSF technique in another model cannot rule out (with statistical significance) anthropogenic fractions up to 50%. Moreover, modeling by Mahowald also shows that CO2 fertilization of vegetation in semi-arid regions is consistent with an anthropogenic-induced decrease in dust emissions of up to -24%. Rather than siding with one study over the other, I recommend that AR4 recognize that uncertainty in anthropogenic dust fraction has, if anything, increased. More comprehensive studies increased uncertainty in the range of anthropogenic dust. The jury is still out! I think the following text by N. Mahowald more fairly summarizes the current state of understanding of anthropogenic mineral dust: "2.4.5.6 Mineral dust Tegen and Fung (1995) estimated the anthropogenic contribution to mineral dust to be 30-50% of the total dust burden in the atmosphere. Tegen et al. (2004) provided a more detailed estimate by comparing observations of visibility as a proxy for dust events from over 2000 surface stations with model results and suggest that only 5-7% of mineral dust comes from anthropogenic agricultural sources. However, Mahowald et al. (2004) uses the same observational data and contests these finding suggesting that up to 0-50% of dust may be of anthropogenic origin. Additionally, mineral aerosols likely respond to anthropogenic climate change and changes in carbon dioxide. The only study which addresses this issue (Mahowald and Luo, 2003) suggests that anthropogenic influences cause mineral aerosols to increase by up to 60% or decrease by 20% relative to the preindustrial climate, depending on the relative importance of land use, climate change and carbon dioxide fertilization, all processes which are poorly understood. Long-term aerosol size distributions retrieved by AERONET (Dubovik et al., 2002) suggest that there is a distinct coarse mode observable at a limited number of industrial sites, but the contribution to the aerosol optical depth from this mode is negligible compared to that from the more optically active sulphate, black carbon, organic carbon and nitrate that	Taken into account, see response to 2-1420

No.	Batch	Page:line From	Page:line To	Comment	Notes
				make up the accumulation mode. Thus there remains considerable uncertainty with respect to the anthropogenic component of mineral dust but we revise the estimate to -24% to +60%." [Charles Zender]	
2-1423	A	34:9		This paragraph unjustifiably concludes that the present-day anthropogenic fraction of mineral dust is 10%. However, the disagreement between Tegen et al (2004,2005), who argue for this value, and Mahowald et al 2004, who defend the original higher value of 50% (ironically proposed originally by Tegen!) remains unresolved. See also Yoshioka et al (2005), who show that TOMS retrievals are unable to pin down the anthropogenic fraction over Africa. The paragraph tries to finesse this controversy by claiming that the coarse mode (dust) radiative forcing is small compared to other anthropogenic aerosols. This is based upon the Dubovik citation, but while true at industrial locations, neglects a majority of the regions where dust-producing soils are disturbed by human agricultural activity. Since the TAR, the major development in this debate is the introduction of the smaller 10% value by Tegen et al, although the dust community has not been able to rule out the original higher value. Part of the disagreement results from different data sets and techniques used to identify anthropogenic sources. I would modify the concluding sentence to: 'Thus, there remains considerable uncertainty with respect to the present-day anthropogenic percentage of mineral dust. While a value as small as 10% has been proposed since TAR, the original value of 50% cannot be ruled out.' Yoshioka, M., N. Mahowald, J.-L. Dufresne, and C. Luo (2005), Simulation of absorbing aerosol indices for African dust, J. Geophys. Res., 110, D18S17, doi:10.1029/2004JD005276. [Ron Miller]	Taken into account, see response to 2-1420
2-1424	A	34:9		This paragraph discusses the *present-day* fraction of anthropogenic sources of dust. However, there will be additional anthropogenic forcing by dust due to changes in the dust source area resulting from climate change. For example, changing temperature, precipitation, sunlight, and carbon dioxide levels will change the extent and leaf-area of various vegetation classes, exposing new dust sources. Mahowald and Luo 2003 predict an expansion of vegetated regions, and a reduction in dust emission, while Woodward et al 2005 predict that drought will constrict vegetation in certain regions while increasing the dust load. Tegen et al 2004 predicts either an increase or decrease in 21C dust, depending upon which AGCM (ECHAM4 or HadCM3) is used to provide temperature and precipitation to the vegetation and dust transport models. (see also p.2-46 l.3 Section 2.5.7) Mahowald N. M., C. Luo, A less dusty future?, Geophys. Res. Lett., 30 (17), 1903, doi:10.1029/2003GL017880, 2003.	Taken into account, see response to 2-1420

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Ron Miller]	
2-1425	A	34:10	34:12	The phrase 'a more detailed estimate' might imply a refinement in method and reduction in uncertainty between the Tegen et al 2004 study and the Tegen and Fung (1995) study cited in the previous sentence. In fact, Tegen et al 2004 use a completely different data set to constrain a different model in a different way. I would characterize the latter as 'an alternative estimate.' [Ron Miller]	Accepted
2-1426	A	34:12	34:14	The two sentences all start with "however". [Mian Chin]	Accepted
2-1427	A	34:17	34:17	…retrieved by AERONET…" -> "…retrieved from AERONET data… [Xiaobin Xu]	Accepted
2-1428	A	34:20	34:22	Where does the 10% come from? [Istvan Laszlo]	
2-1429	A	34:22	34:22	This is another misleading – 10% anthropogenic dust burden ? 10% anthropogenic dust forcing. [Mian Chin]	Accepted!!!
2-1430	A	34:24	34:31	There are somewhat contradictory results on the desert dust single scattering albedo, and the sentence seems too deterministic. In the Mediterranean low, (between 0.7 and 0.84: Di Iorio et al., 2003, and Meloni et al., 2003; average values of 0.81 at 415.6 nm and 0.94 at 868.7 nm; Meloni et al., 2005), intermediate (0.88-0.89, Meloni et al., 2004; 0.89, Formenti et al., 2001), and relatively high (0.91, Di Iorio et al., 2003; 0.94, Formenti et al., 2001; 0.95, Kubilay et al., 2003; 0.96-0.97, Meloni et al., 2004) values of the single scattering albed have been found. Collaud Coen et al. (2004) measure at Jungfraujoch, on the Alps, values of single scattering albedo between 0.7 and 0.93 at 520 nm during a Saharan dust event in 2001. Kim et al. (2005) find values of the the Asian dust single scattering albedo as low as 0.8. Weaver et al. (2002) for example, found that the Patterson dust optical properties (relatively high absorption) produce the best results in the comparison between modeled and measured TOA SW fluxes. At least in the Mediterranean, the situations seems to be more complex, with different types of dust particles and/or mixing state. An interesting case with markedly different results is described by Meloni et al., 2004. References for the preceding row: Di Iorio, T., A. di Sarra, W. Junkermann, M. Cacciani, G. Fiocco, and D. Fuà, Tropospheric aerosols in the Mediterranean: I. Microphysical and optical properties, J. Geophy. Res., 108 (D10), 4316, doi: 10.1029/2002JD002815, 2003. Formenti, P., M. O. Andreae, T. W. Andreae, C. Ichoku, G. Schebeske, J. Kettle, W. Maenhaut, J. Cafmeyr, J. Ptasinsky, and A. Karnieli, Physical and chemical	Taken into account, discussion of single scattering albedo has been made more precise. However not to the extent the reviewer proposed. Space sonstraints.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				characteristics of aerosols over the Negev (Israel) Desert during summer 1996, J. Geophys. Res., 106, 4871– 4890, 2001. Kim, D.-H., B. J. Sohn, T. Nakajima, and T. Takamura, Aerosol radiative forcing over east Asia determined from ground-based solar radiation measurements, J. Geophys. Res., 110, D10S22, doi:10.1029/2004JD004678, 2005. Kubilay, N., T. Cokacar, and T. Oguz, Optical properties of mineral dust outbreaks over the northeastern Mediterranean, J. Geophys. Res., 108(D21), 4666, doi:10.1029/2003JD003798, 2003. Meloni, D., A. di Sarra, J. DeLuisi, T. Di Iorio, G. Fiocco, W. Junkermann, and G. Pace, Tropospheric aerosols in the Mediterranean: II. Radiative effects through model simulations and measurements, J. Geophys. Res., 108 (D10), 4317, doi: 10.1029/2002JD002807, 2003. Meloni, D., A. di Sarra, T. Di Iorio, and G. Fiocco, Direct radiative forcing of Saharan dust in the Mediterranean from measurements at Lampedusa island and MISR space-borne observations, J. Geophys. Res., 109 (D8), D08206, doi: 10.1029/2003JD003960, 2004. Meloni, D., A. di Sarra, G. Pace, and F. Monteleone, Optical properties of aerosols over the central Mediterranean. 2. Determination of single scattering albedo at two wavelengths for different aerosol types, Atmos. Chem. Phys. Discuss., 5, 4971-5005, 2005. Weaver, C. J., P. Ginoux, N. C. Hsu, M.-D. Chou, and J. Joiner, Radiative Forcing of Saharan Dust: GOCART Model Simulations Compared with ERBE Data, J. Atmos. Sci, 59, 736-747, 2002. Collaud Coen, M., E. Weingartner, D. Schaub, C. Hueglin, C. Corrigan, S. Henning, M. Schwikowski, and U. Baltensperger, Saharan dust events at the Jungfraujoch: detection by wavelength dependence of the single scattering albedo and first climatology analysis, Atmos. Chem. Phys., 4, 2465–2480, 2004. [Alcide di Sarra]	
2-1431	A	34:24	34:44	The following text and references are from Mikami et al. 2005: Aeolian Dust Experiment on Climate Impact: An Overview of Japan-China Joint Project ADEC, Global Planetary Change, (accepted). During ADEC field campaigns, three sky-radiometers were operated in the dust source region (Aksu, Qira, and Shapotou in China), two in a downwind area in China (Beijing and Qindao), and four in Japan (Naha, Fukuoka, Nagoya, and Tsukuba) (Uchiyama et al., 2005b). Based on the ADEC data, sensitivity experiments of direct RF caused by MD for the optical and physical properties of MD were conducted using one dimensional radiative transfer model, the Streamer-based Radiative Transfer Model for ADEC Sciences (SARTMAS) (Aoki et al., 2005) and using a k-distribution model for solar and	Taken into account, discussion of single scattering albedo has been made more precise. However not to the extent the reviewer proposed. Space sonstraints.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				thermal radiation transfer (Shi et al., 2005). Aoki et al. (2005) simulated the atmospheric and dust profiles with a chemical transport model MASINGAR (Tanaka et al., 2005) at four locations: the Sea of Japan, the desert in Tarim Basin, the Sahara Desert, and snow in Siberia. The experiment results confirmed that the sensitivity of instantaneous RF in the shortwave (SW) region at the top of the atmosphere (TOA) to the refractive index strongly depends on surface albedo. Namely, the effect of the difference in the MD model on instantaneous RF is significant over high albedo surfaces and is relatively small over the sea because the multiple reflections between the atmosphere (dust) and surface enhance light absorption by dust particles over high albedo surfaces. Over desert surfaces, the instantaneous RF in SW at TOA produced both positive and negative values within the possible refractive index range of MD. The diurnally averaged RF in SW at TOA also produced both positive and negative values in the possible range of desert albedo. It was found that for small dust particles with an effective radius of less than 0.6 mm, RFs by MD changed depending on the difference in surface type even if the broadband albedo was the same. The vertical positional relationship of cloud cover to dust layer was also very important for RF at TOA in all spectral regions over desert and sea surfaces. However, the effect of cloud cover was generally small over snow surface because cloud albedo was close to the underlying snow albedo. Shi et al. (2005) performed numerical sensitivity experiments to evaluate the impact of optical characteristics on the RF. The experiments involved the effects of refractive indices, SSA, asymmetry factor and optical depth of MD. They used an updated data set of refractive indices of ADEC-2 model in Aoki et al. (2005), which represents East Asian dust, and the data set by Woodward (2001)*. The main differences between the two optical models are: (1) the real part of refractive index of the ADEC-2 model is slightly larger than that of the Woodward model (Woodward, 2001) at most wavelengths from solar to infrared bands; and (2) the imaginary part of refractive index of the ADEC-2 model is generally smaller than that of the Woodward model over solar wavelengths. Shi et al. used a k-distribution model (Shi, 1998) to calculate RF. Numerical simulation was conducted using the large dust event on 4 to 15 April 2001. The daily dust concentration was provided by the NARCM model by Gong et al. (2003). Their results indicate that the ADEC-2 model has stronger scattering and weaker absorption, which leads to higher negative forcing at the top of the atmosphere (TOA) as compared with the Woodward model. SSA is a primary factor in determining whether RF due to MD is positive or negative in the atmosphere (Aoki et al., 2005b). Recently, SSA for Saharan dust has been found to have a higher value (Kaufman et al., 2001, Haywood et al., 2001, 2003) than that previously reported (Shettle and Fenn, 1979, Hess et al., 1998, Sokolik and Golitsyn,	

No.	Batch	Page:line From	To	Comment	Notes
				1993). However, data on SSA features in dust source regions in East Asia are lacking to date. Uchiyama et al. (2005a) retrieved SSA from sky-radiometer data in the ADEC network sites from the Taklimakan Desert to Japan. The averaged SSAs during ADEC IOP1, April 2002, at Aksu, Qira, Shapatou, Qingdao, Naha, Fukuoka, Nagoya, and Tsukuba were 0.955, 0.933, 0.914, 0.942, 0.944, 0.953, 0.933, and 0.973, respectively. In addition to the retrieved SSA analysis, in situ measurements of SSA by Particle Soot Absorption Photometer (PSAP; Radiance Research) and Nephelometer (M903; Radiance Research) were carried out at Qira, Beijing, and Tsukuba (Uchiyama et al., 2005a). SSA measured by PSAP and M903 were between 0.91 and 0.93 at Qira, between 0.80 to 0.88 at Beijing, and between 0.8 and 0.9 at Tsukuba. The SSA measured by PSAP and M903 at Qira were consistent with the SSA inferred from the sky-radiometer. This means that unpolluted aeolian dust has lower absorption than originally believed. The SSA derived from PSAP and M903 at Beijing and Tsukuba is lower than the SSA inferred from the sky-radiometer. This is partly because the SSA derived from PSAP represents information of dust particles near the surface and, hence, represents the dust particles mixed with absorbing aerosols during long range transport within the boundary layer. References Aoki, T., Tanaka, T. Y., Uchiyama, A., Chiba, M., Mikami, M., Yabuki, S., and Key, J., 2005. Sensitivity Experiments of Direct Radiative Forcing by Mineral Dust using Spectrally Detailed Radiative Transfer Model. J. Met. Soc. Japan. 83A, 315-331. Gong, S.L., Zhang, X. Y., Zhao, T. I., Mckendry, I. G., Jaffe, D. A., and Lu, N. M., 2003. Characterization of soil dust aerosol in China and its transport and distribution during 2001 ACE-Asia: 2. Model simulation and validation. J. Geophys. Res., 109, D9, doi:10.1029/2002JD002633. Haywood, J., Francis, P., Osborne, S., Glew, M., Loeb N., Highwood, E., Tanré, D., Myhre, G., Formenti, P., and Hirst, E., 2003. Radiative properties and direct radiative effect of Saharan dust measured by the C-130 aircraft during SHADE: 1. Solar spectrum. J. Geophys. Res., 108, 8577, doi:10.1029/2002JD002687. Hess, H., Koepke, P., and Schult, I., 1998. Optical Properties of Aerosols and Clouds: The Software Package OPAC, Bull. Am. Met. Soc., 79, 831-844. Kaufman, Y. J., Tanré, D., Dubovik, O., Karnieli, A., and Remer, L. A., 2001. Absorption of sunlight by dust as inferred from satellite and ground-based remote sensing. Geophys. Res. Lett., 28, 1479-1482. Shettle, E. P., and Fenn, R. W., 1979. Models for the aerosols of the lower atmosphere and the effects of humidity variations on their optical properties, AFGL-TR-79-0214, Air Force Geophysics Laboratory, 94pp. Shi, G.-Y., 1998. On the k-Distribution and Correlated k-distribution Models in the Atmospheric Radiation Calculations. Scientia Atmospherica Sinica (Special Issue	

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Dedicated to the 70th Anniversary of the Founding of the Institute of Atmospheric Physics, Chinese Academy of Sciences). 22, 555-576. Shi, G-Y., Wang, H., Wang, B., Li, W., Gong, S., Zhao, T. and Aoki, T., 2005. Sensitivity experiments on the effects of optical properties of dust aerosols on their radiative forcing under clear sky condition. J. Met. Soc. Japan. 83A, 333-346. Sokolik, I., and Golitsyn, G., 1993. Investigation of optical and radiative properties of atmospheric dust aerosols. Atmos. Environ., 27A, 2509-2517. Tanaka, T. Y., Kurosaki, Y., Chiba, M., Matsumura, T., Nagai, T., Yamazaki, A., Uchiyama, A., Tsunematsu, N., and Kai, K., 2005. Possible transcontinental dust transport from north Africa and the middle east to east Asia. Atmos. Environment. 39, 3901-3909. Uchiyama, A., Yamazaki, A.,Togawa, H., Asano, J., 2005a. Absorption Property of Aeolian Dust as inferred from Sky-radiometer and Ground-Based Measurement. Proceedings of the Fourth ADEC Workshop. Uchiyama, A., Yamazaki, A., Togawa, H., and Asano, J., 2005b. Characteristics of Aeolian dust observed by sky-radiometer in the ADEC Intensive Observation Period 1(IOP1). J. Met. Soc. Japan. 83A, 291-305. Woodward, S., 2001. Modeling the atmospheric life cycle and radiative impact of mineral dust in the Hadley Center climate model. J. Geophys. Res., 106, 18155–18166. [Guang-yu Shi]	
2-1432	A	34:24	34:31	In this paragraph, optical property of mineral dust is described. During April 2001 to March 2005, a project related to Asian mineral dust was performed under the cooperation of Japan and China. In this project, the optical property of Asian mineral dust was investigated. The results of this project are not referred. Using composition of mineral dust and the ground-based measurement of spectral irradiance, Aoki et al. (2005) show that Asian mineral dust is less absorbing than suggested by previous models. In addition, Uchiyama et al. (2005) show that analysis of SSA from sky-radiometer data reveals less absorbing than suggested by previous models, too. Aoki, Te., T. Tanaka, A. Uchiyama, M. Chiba, M. Mikami, and J. Key, 2005: Sensitivity tests of direct radiative forcing by mineral dust using spectrally detailed radiative transfer model, J. Meteorol. Soc. Japan, 83A,315-331 Uchiyama A., A.Yamazaki, H. Togawa, J. Asano, and Guangyu Shi, 2005: Single scattering albedo of Aeolian Dust as inferred from Sky-radiometer and in-situ Ground-Based Measurement. SOLA, in press. [Akihiro Uchiyama]	Taken into account, discussion of single scattering albedo has been made more precise. However not to the extent the reviewer proposed. Space sonstraints.
2-1433	A	34:24	34:24	…Tanre et al., 2003); transported" -> "…Tanre et al., 2003), transported	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Xiaobin Xu]	
2-1434	A	34:24		This paragraph discusses radiative properties of dust particles, but neglects the uncertainty in forcing due to variations in mineralogy. These variations are not accounted for in any global dust model of which I am aware, due to the absence of a global atlas of source mineralogy. Moreover, the aerosol mineralogy will depend upon the dynamics of emission in addition to the mineralogy of the underlying soil. I suggest adding a sentence like: 'While uncertainties in solar absorption by Saharan dust have been reduced since TAR, absorption by particles from other source regions (with different mineralogical distributions) remains uncertain, and is generally not represented by global models. [Ron Miller]	Taken into account, discussion of single scattering albedo has been made more precise. However not to the extent the reviewer proposed. Space sonstraints.
2-1435	A	34:25	34:31	While recent measurements indeed suggest that mineral dust particles are less absorbing at solar wavelengths than previously assumed, this should not be taken as cartainty, considering that most of the measurements are based on remote sensing of transported dust. [Ina Tegen]	Taken into account, discussion of single scattering albedo has been made more precise. However not to the extent the reviewer proposed. Space sonstraints.
2-1436	A	34:33	34:44	Based on the refractive indices measurements over China desert area, Wang et al (2004) estimated that a peak value of radiative forcing due to mineral dust for the spring mean of 2001 is up to 10 Wm-2 over China desert region and - 4Wm-2 over West Pacific Ocean at the top of atmosphere. Shi et al. (2005) performed further numerical sensitivity experiments to evaluate the impact of optical characteristics of mineral dust on its radiative forcing and indicated that the peak value of RF for a strong dust storm occurred during April 4 -15 in 2001 may reach up to 12 Wm-2 over China desert area and -12 Wm-2 over West Pacific Ocean. More important thing is that Shi et al. (2005) found a huge uncertainty in estimating the radiative forcing due to mineral dust comes from the refractive indices, such as the WMO and ADEC data sets, of dust, which are used in the model (see their Figs.4 and 5). Related references WANG Hong, SHI Guangyu, Aoki Teruo, WANG Biao?ZHAO Tianliang?Radiative forcing due to dust aerosol over east Asia-north Pacific region during spring, 2001, Chinese Science Bulletin 2004 Vol. 49, No. 20 2212—2219. SHI Guangyu, WANG Hong, WANG Biao?LI Wei, GONG Sunling,, ZHAO Tianliang, AOKI Teruo, Sensitivity experiments on the effects of optical properties of dust aerosols on their radiative forcing under clear sky condition, Journal of the Meteorological Society of Japan, 2005,Vol.83, pp.333-346. [Guang-yu Shi]	Taken into account, discussion of single scattering albedo has been made more precise. However not to the extent the reviewer proposed. Space sonstraints.
2-1437	A	34:35	34:39	Please, indicate if the listed instantaneous radiative effects are at the surface or at the top	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				of the atmosphere. The value of -130 W/m2 is apparently reported by Haywood, J. M., S. R. Osborne, P. N. Francis, A. Keil, P. Formenti, M. O. Andreae, and P. H. Kaye, The mean physical and optical properties of regional haze dominated by biomass burning aerosol measured from the C-130 aircraft during SAFARI 2000, J. Geophys. Res., 108(D13), 8473, doi:10.1029/2002JD002226, 2003, and not by Haywood et al (2003b, see the reference on page 77, lines 21-23). [Alcide di Sarra]	
2-1438	A	34:38	34:38	measured the spectral…" -> "determined the spectral… [Xiaobin Xu]	Rejected, style
2-1439	A	34:46	34:51	Although it is recognized that dust particles during their residence time in the atmosphere can accrete low-vapor-pressure material formed by gas-to-particle conversion, the addition of this material to the dust mode has little effect on dust optical properties [Anderson et al., 2003; Clarke et al., 2004]. [Tim Bates]	Taken into account. The effect of condensation of condensable material on dust is indeed probably small, though difficult to assess.
2-1440	A	34:46	34:51	The major effect of the dust to DCF is in reducing the burden of anthropogenic species in the accumulation mode and reducing the aerosol lifetime. [Tim Bates]	Accepted
2-1441	A	34:46	34:51	Bauer and Koch (JGR 2005) should be cited as an example of the change in sulfate radiative forcing due to sulphate uptake on dust particles. Bauer S. E., D. Koch (2005), Impact of heterogeneous sulfate formation at mineral dust surfaces on aerosol loads and radiative forcing in the Goddard Institute for Space Studies general circulation model, J. Geophys. Res., 110, D17202, doi:10.1029/2005JD005870. [Ron Miller]	Accepted
2-1442	A	34:50	34:51	Is the effect dependent on the size of the dust or of other species? [Tami Bond]	Accepted – of dust
2-1443	A	34:53	34:57	Again, for the sake of proper perspective and clarity, and in order to minimize the possibilities for being misunderstood, the radiative forcing by dust should be expressed in terms of its total column radiative forcing (RF), along with the anthropogenic ARF component separately identified as being due to anthropogenic activities. It should be made clear that the uncertainties associated with attributing some fraction of atmospheric dust to human impacts is incomparably greater than measuring the total dust fraction of atmospheric aerosol. [Andrew Lacis]	Accepted
2-1444	A	34:53	34:56	I was surprised by the large values that TAR estimated for the RF due to mineral dust. It seems to be quite counter-intuitive. The new value given in this assessment document of-0.2 to +0.1W/m2 seems to be much more plausible, even though I find it hard to support a	Accepted. Some recent work still suggests a SW+LW positive radiative forcing. However – we now have

No.	Batch	Page:line From	Page:line To	Comment	Notes
				+0.1 W/M2 value, or any net globally positive value, for that matter. [Jerry Mahlman]	revised the estimate to -0.2 +-0.2
2-1445	A	34:53	34:56	"Given the reduction in the anthropogenic component of the mineral dust since TAR and that the majority of the models in the AEROCOM project produce estimates of the direct RF due to anthropogenic mineral dust that are close to zero, the range of the direct RF due to mineral dust from Ramaswamy et al. (2001) of –0.6 to +0.4 W m–2 is reduced considerably to –0.2 to +0.1 W m–2." Model simulations can't tell the difference in concentration or optical depth (even over the source areas!) between a 0-50% land use source (Luo et al., 2003; Yoshioka et al., 2005), so that most groups avoid the problems with trying to guess the anthropogenic part. Therefore the statement that the majority of the AEROCOM models do not include anthropogenic emissions does not address what the anthropogenic emissions are and should be left out of the manuscript. The radiative forcing uncertainty bars should be increased to reflect the real uncertainty in mineral aerosols (see comment 2). [Natalie Mahowald]	Accepted, the AeroCom citation in this respect was wrong.
2-1446	A	34:53	34:57	Given the controversy over the anthropogenic fraction of dust (c.f. comment on p.34 l.9), it is premature to reduce the TAR range for dust radiative forcing, although one might exclude the positive forcing values, given recent work on solar absorption by Saharan dust. However, this estimate does not include forcing resulting from changes to dust source areas due to the effect of climate change upon vegetation. (This comment also applies to the dust radiative forcing listed in the executive summary: p.4 l.57, p.5 l.4-5) [Ron Miller]	Accepted
2-1447	A	34:53	34:55	Do the AEROCOM models actually consider an anthropogenic dust component? Otherwise they would clearly predict zero forcing. [Ina Tegen]	Accepted, AeroCom citation wrong
2-1448	A	34:55	34:55	Is this "direct RF due to mineral dust" from the "anthropogenic dust"? [Mian Chin]	Accepted, clarified
2-1449	A	34:56		I would recommend giving a best estimate value for forcing by anthropogenic dust in addition to the uncertainty range; e.g., -0.05 ± 0.15, unless the authors mean 0 with asymmetric error bars, in which case this should be explicitly stated. The consequence of not stating a value is that this forcing is implicitly taken as zero in adding forcings. [Stephen E Schwartz]	Accepted,
2-1450	A	34:57	34:57	For completeness, there should be a separate section to describe the role and radiative effects of sea salt aerosol, since sea salt is one of the principal aerosol species. Sea salt aerosol climatologies have been produced by e.g., Tegen et al. (1997), Chin et al. (2002). Observational aspects are discussed by e.g., Quinn et al (1998), Quinn and Coffman (1999), Haywood et al. (1999). The role of sea salt aerosol in aqueous chemistry is	Rejected. Though the radiative effect of sea salt is undoubtedly existing there is no indication of anthropogenic sea salt which ultimately is needed to fall into the forcing definition. Sea-salt mixtures

No.	Batch	Page:line From	Page:line To	Comment	Notes
				described by Chamedies (1992). Sea salt particles are efficient CCN, and they interact with other atmospheric aerosol species. Even if the anthropogenic component of sea salt RF is near zero, the climatic effects of other atmospheric aerosols would certainly be different than their current contributions if there really were no sea salt aerosols present in the atmosphere. [Andrew Lacis]	with organic carbon discussed elsewhere.
2-1451	A	35:1	35:10	What is the logic here that we don't understand how the aerosol components contribute to the total aerosol RF but we understand the total aerosol RF? I disagree that we should not use the propagation of errors to estimate the uncertainty. I think we should. [Mian Chin]	Taken into account. An error propagation analysis is now added.
2-1452	A	35:1	35:10	A very nice concept. [Jerry Mahlman]	Accepted
2-1453	A	35:1	36:56	Changes in the surface radiative forcing is mentioned in chapter 2.8.1 but in spite of an increasing number of publications neither the magnitude nor the consequences on the hydrological cycle are discussed. Please, add a discussion about this topic to chapter 2.4.5.7 referring to work by Stanhill&Moreshet, 1994, Liepert and Tegen, 2002 and Wild et al., 2005 as well as model studies by Roeckner et al., 1999, Ramanathan et al., 2001, Liepert et al., 2004, AeroCom results. Changes of the surface energy budget due to aerosols may be as important as changes of the cloud microphysical properties. [Johann Feichter]	Accepted – reference is now made and especially a link to chapter 7 where this is discussed in more detail.
2-1454	A	35:1	36:56	This whole section is difficult to understand because the way it is written. Sentences are too long and are not clear. Examples are p35, lines 36-42, p35line54-57, p36,line10-22. [Santiago Gassó]	Taken into account
2-1455	A	35:1	36:56	I think this part contains too much text. This should be written more concisely,particularly the paragraphs 2,3,and 4 of page 35. [Xiaobin Xu]	Taken into account
2-1456	A	35:2	35:10	I would maintain that the anthropogenic components are also better constrained by in-situ observations and that the large uncertainty in the total aerosol amounts is a result of dust and seasalt whose emssions are much more uncertain (Bates et al., 2005 ACPD). This is consistent with your argument on page 36 line 10-22. [Tim Bates]	Noted
2-1457	A	35:2	35:10	Rephrase for better readability. [Christiane Textor]	Noted
2-1458	A	35:2	:5	The associated uncertainties for each individual component suggested that by combining them the overall uncertainty of aerosol forcing would be very large through uncertainty propagation Large uncertainty is hardly a justification for not summing the forcings and estimating the	Taken into account. An error propagation analysis is now added.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				uncertainty in the sum. [Stephen E Schwartz]	
2-1459	A	35:3	35:3	Replace "robust THEN …" with "robust THAN …" [Charles Ichoku]	Accepted
2-1460	A	35:3	35:3	Delete 'However, 'and start the sentence with 'The..' [Christiane Textor]	Accepted
2-1461	A	35:5	35:5	"the overall uncertainty of aerosol forcing would be very large…" What is wrong with that conclusion? If you get a large overall uncertainty from the uncertainty propagation, this is because the uncertainty is indeed VERY LARGE. We should acknowledge it, not hide it. [Mian Chin]	Taken into account. An error propagation analysis is now added.
2-1462	A	35:7	35:7	"better constrained" ? "better understood". All the uncertainties in the individual component are retained in the combined total. [Mian Chin]	Taken into account. An error propagation analysis is now added.
2-1463	A	35:15	35:15	Replace "Kaufmann" with "Kaufman". [Charles Ichoku]	Accepted
2-1464	A	35:16	35:18	Are there references to substantiate the claim beginning 'Internally mixed black carbon…'? I don't know of any. [Tami Bond]	Taken into account, statement revised
2-1465	A	35:16	35:18	This statement needs a reference. I would have thought internally mixed BC absorbes more than externally mixed BC. Also, it needs to be squared with the statement on lines 33, 34 [Joyce Penner]	Taken into account, statement revised
2-1466	A	35:22	35:22	add Liao et al., JGR, 109 (D16): Art. No. D16207 AUG 21 2004 [Johann Feichter]	Accepted
2-1467	A	35:30	35:33	The discussion of Liao is difficult to understand. The physical mechanism discussed is due to sea salt and dust, but the change in RF is attributed to a doubling of SO2 emissions. [Tami Bond]	Accepted, clarified
2-1468	A	35:36	35:36	should read "is responsible for the fact that the RF from the.." [Graham Feingold]	Accepted, sentence changed see below
2-1469	A	35:36	35:36	should read ".. Is responsible for the fact that…" [Eleanor Highwood]	Accepted, sentence changed see below
2-1470	A	35:36	35:37	"The model specific treatment of transport and removal processes is responsible for that the RF major aerosol components are not independent from each other in a given model." is not clear. Please try to reword it. [Charles Ichoku]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1471	A	35:36	35:37	The sentence reading "The model specific treatment of transport and removal processes is responsible for that the RF from the major aerosol components are not independent from each other in a given model." is unclear. I recommend changing to "Since the processes affecting major aerosol components are not independent of each other, the model specific treatment of transport and removal processes is partly responsible for uncertainties in aerosol RF estimates.", if this captures what was intended. [Brian Magi]	Accepted, Thanks
2-1472	A	35:36	35:42	This is an interesting, but kind of obscure. [Jerry Mahlman]	Accepted, rephrased
2-1473	A	35:36	35:37	This phrase is not clear. Probably the part "…is responsible for that the RF..." should read "… is responsible for the fact that the RF". [Felicita Russo]	Accepted, sentence changed see above
2-1474	A	35:36	35:37	Rephrase for better readability. [Christiane Textor]	Accepted, sentence changed see above
2-1475	A	35:36	35:37	model specific to model-specific? Everywhere [Christiane Textor]	Accepted, rephrased
2-1476	A	35:36	35:53	The issue of error propagation should be discussed in one paragraph, i.e., together with the first paragraph on this page 35. Move the whole paragraph (line 36-53) up to line 11. [Christiane Textor]	Accepted
2-1477	A	35:36		... processes are responsible for the fact that the RF from ... [Jerry Mahlman]	Accepted, sentence changed see above
2-1478	A	35:37	35:38	Dispersivity does not only depend on transport alone, but also on the internal aerosol parameterizations. This should be clear in the text. [Christiane Textor]	Accepted
2-1479	A	35:40	35:40	Using the combined forcing should definitely decrease the scatter as contrasting effects cancel each other out. However, this decreased scatter does not mean that the process is better understood, nor that the uncertainty is smaller. As a consequence, I think the individual forcings are much more important than the total aerosol forcing! [Christiane Textor]	Noted, An error propagation analysis is added.
2-1480	A	35:44	35:44	What is an 'important scatter of possibilities'? [Tami Bond]	Noted, rephrased
2-1481	A	35:44	35:36	This sentence makes absolutely no sense whatsoever I'm afraid. [Eleanor Highwood]	Noted, rephrased
2-1482	A	35:44	35:44	avoid the word 'scatter' here as it can be confused with scatter of radiation. [Christiane Textor]	Noted, rephrased
2-1483	A	35:47	35:52	I assume that the RF at the TOA is meant here?	Noted, rephrased

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Christiane Textor]	
2-1484	A	35:48	35:48	Again, this term 'convenient' really lies in the eye of the beholder. Since sulfates and organic carbon have vastly different sources, such a grouping is not convenient for some purposes. [Tami Bond]	Noted, rephrased
2-1485	A	35:50	35:53	I don't find this bullet point either comprehensible or useful. Since black carbon aerosol usually comes with other aerosols, how can we isolate it and suggest that it is more uncertain and variable than the rest? Also, what is the utility of grouping the 'nice', negative-RF aerosols as opposed to the aerosols that are usually found together, such as black and organic carbon? [Tami Bond]	Noted, however, absorption deserves to be mentioned to be the major cause of uncertainty in the overall TOA forcing
2-1486	A	35:52		....variable of these two aggregated components. [Jerry Mahlman]	Accepted
2-1487	A	35:57	35:57	do you mean "overall reproduction of measurements of the total aerosol optical depth"?? [Joyce Penner]	Accepted
2-1488	A	36:0		Is there an IR forcing by aerosols that should be discussed? (like maybe from high altitude dust?) [Robert Levy]	Accepted
2-1489	A	36:2	36:3	This is a very important observation, and I am glad to see it stated here. [Istvan Laszlo]	Accepted
2-1490	A	36:2	:3	The combined RF taken together from several models is more robust then an analysis per component or by just one model. This statement would seem difficult to support. Model consensus is essentially majority rule. It is somewhat surprising that the modeled aerosol optical depths in Kinne figure 2b agree so closely with observational products, given the spreads in many of the key components of the models. The coefficient of variation (standard deviation over mean) of the global and annual mean AOT's is 21%, whereas the contributions to the total AOT from sulfate, black carbon, particulate organic matter, dust, and seasalt are much greater, respectively, 31%, 51%, 39%, 44%, and 46%. Pursuing this further, the modeled AOT of models UL and GM agree within about 20% of each other and the observations. Yet the sulfate loadings differ by as much as a factor of 3, compensated by differences in the mass scattering efficiencies. The ranges of mass extinction efficiencies for the sulfate, black carbon, particulate organic matter, dust, and seasalt range in the several models examined in that study respectively by factors of 6.7, 3.5, 2.8, 15, and 7.7. Yet the AOT's range only over a factor of 2.3, indicative of substantial compensation in the models that again raises questions of the robustness of the conclusions.	Taken into account. The uncertainty has been revised to reflect also recent work on observational based estimates of radiative forcing. We disagree that the AOD provide no constraint on the total aerosol direct forcing.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				It might be argued that as sea salt and mineral dust are natural aerosols, differences in their contributions to mass loading and aerosol optical depth are of no consequence. However, these contributions still contribute to aerosol optical depth and to any comparisons with observations. Although the fraction of column mass due to dust and sea salt in the several models ranges narrowly from 74% to 93%, the fraction due to the (mainly) anthropogenic species sulfate, black carbon, and particulate organic matter ranges several fold, from 7% to 26%. The contribution of sea salt and dust to AOT ranges from 20 to 79% (or equivalently the contribution of sulfate, black carbon and particulate organic matter ranges from 21% to 80%). These wide ranges again call into question the robustness of the RF of the several models. Note that Yu et al (2005, cited in present report) suggest that the anthropogenic contribution to global annual aerosol optical depth is about 21%, at the extreme end of the model range above. The above considerations suggest that a more careful reading of Kinne might lead to some caution in the conclusion of the robustness of the consensus product. This concern is evidenced also in Table 2.4.3 for sulfate, page 2-99. I have extended that table by calculating NDRFM for entries N-T from the ratios of load and forcing, as follows: (TSU moved to supplemental material) [Stephen E Schwartz]	
2-1491	A	36:2	:3	I would first suggest for completeness that these entries be added to that table. The coefficient of variation for all quantities, for the entire set of entries A-L and N-T range from 0.38 to 0.42. Depending on the choice of interpretation of the uncertainty expressed by ±, this suggests an uncertainty of ± 40% (1 sd) or ± 80% (2 sd). These uncertainties imply a multiplicative uncertainty (1+delta)/(1-delta) of a factor of 2.3 to 9. It would thus seem that a fair reading of this result would require that such an estimate of uncertainty be reflected in the present document. Technical corrections: The unit for NDRFM, forcing per sulfate column burden, should be W/g [= W m-2 per g m-2], not W m-2 g. Sign of all should be negative. [Stephen E Schwartz]	Taken into account. The uncertainty has been revised to reflect also recent work on observational based estimates of radiative forcing. We disagree that the AOD provide no constraint on the total aerosol direct forcing.
2-2717	B	36:7	36:8	There is some confusion between Kaufman et al, PNAS, 2005 (cited incorrectly as ACPD) and Kaufman et al, GRL, 2005. I assume you refer to the GRL paper here. [Olivier Boucher]	Accepted, Reference to the GRL paper is made (Kaufman et al., 2005a). Reference for Kaufman et al., PNAS, 2005b is corrected.
2-1492	A	36:10	36:22	This is a confusing and unconvincing paragraph. It contains too much detail regarding a single, extremely speculative and exploratory approach (from the Kaufman et al., 2005 paper), yet fails to adequately explain the host of assumptions required by that approach nor convey any sense of its overall uncertainty. I suggest that the first two sentences be retained and the remainder of the paragraph be deleted. If the Kaufman et al. method	Taken into account.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				(based on the fine-mode fraction product and numerous assumptions) for deducing anthropogenic aerosol is discussed, it should be tempered by reference to the several papers indicating that the fine-mode fraction product from MODIS seems to contain systematic errors (e.g. Levy et al., 2003; Chu et al., 2005, Anderson et al., 2005b, cited in previous comment.) [Theodore Anderson]	
2-2718	B	36:10	36:22	Kaufmann => Kaufman. Say what the 21% refer to. Generally speaking I think the observationally-based estimates of the direct RF by aerosols should be assessed further. Bellouin et al (2005) is now accepted in Nature and suggest a larger direct RF than models. This is also consistent with latest results by J. Quaas using CERES. [Olivier Boucher]	Accepted
2-1493	A	36:10	36:11	What is the "total radiative perturbation?" (need definition) [Robert Levy]	Ok, sentence changed and the DRE that is included is defined in SOD clearly in 2.4.1.1.2
2-1494	A	36:10	36:14	Does underestimation by 50% contrast with the inference on page 26, line 36-40, that observations and models are similar? [Robert Levy]	Noted, text on page 26 changed
2-1495	A	36:10	36:11	The fact that models underestimate the negative aerosol forcing by 20-50% is in contrast with a medium level of scientific understanding. [Felicita Russo]	Noted, LOSU now changed to low.
2-1496	A	36:10	36:22	This paragraph contains references to two publications that are submitted and thus unavailable to check. This should be avoided if similar information is available elsewhere. [Ina Tegen]	Noted, The Yu paper is submitted to ACP and thus an ACPD version was available during the review of FOD. Unfortunately, the reference for the Kaufman et al paper was wrong and had actually appeared in GRL.
2-1497	A	36:10	:11	When comparing the total radiative perturbation in oceanic clear-sky conditions models appear to underestimate the negative aerosol forcing by 20–50% (Yu et al., 2005). I was unable to find this conclusion in Yu et al; perhaps I missed it. I did find the statement that" DCF is estimated to be about -0.5 W/m 2 with an uncertainty of about 67%. " This would suggest that models are coming up with estimates of DCF of -0.25 to -0.4 to W m-2, which if anything is somewhat higher than the averages given in Table 2.4.5. I suggest this be clarified, and that if the conclusion of 20-50% underestimate is drawn by the present authors (rather than by Yu) that be made clear and the numbers presented. [Stephen E Schwartz]	Accepted
2-1498	A	36:12	36:12	Should be 'allows US [?] to…'	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Tami Bond]	
2-1499	A	36:15	36:15	"Kaufman", not "Kaufmann". [Mian Chin]	Accepted
2-1500	A	36:15	36:15	Kaufman spelled wrong [Robert Levy]	Accepted
2-1501	A	36:15		"Kaufman" not "Kaufmann" the reference is the same as in comment to 2-26,12. [Yoram Kaufman]	Accepted
2-1502	A	36:18		"of the aerosol optical depth" should be "of the total aerosol depth is anthropogenic". [Yoram Kaufman]	Accepted
2-1503	A	36:20	36:21	Ambiguous. Modelled anthropogenic aerosol optical depth is not overestimated, but its fraction is overestimated. [Hongbin Yu]	Accepted
2-1504	A	36:21		optical depth is likely due to deficiencies.... [Jerry Mahlman]	Acccepted. Sentencen clarified
2-1505	A	36:25		Surface forcing.Needs to be defined: Decrease in downwelling or net shortwave irradiance at the surface? Clear sky or all sky? [Stephen E Schwartz]	Accepted. Meant is net shortwave irradiance at surface. All-sky.
2-1506	A	36:27	36:27	…in earlier subchapters…" -> "…in earlies sections… [Xiaobin Xu]	Accepted
2-1507	A	36:32	36:32	Does this mean that the error in t-aer can be up to 0.1 optical depth? This seems quite large. [Tami Bond]	Taken into account. Very locally it can attain 0.2, Text changed to emphasize that in the major part of the globe the bias ranges between 0.05 and -0.1. And that this corresponds to total aerosol, not anthropogenic.
2-1508	A	36:33	36:33	It should be rather 0.2 than 0.1 [Eugene Rozanov]	Taken into account. Very locally it can attain 0.2, Text changed to emphasize that in the major part of the globe the bias ranges between 0.05 and -0.1. And that this corresponds to total aerosol, not anthropogenic.
2-1509	A	36:33		correct: "Figures" to "Figure" [Hartmut Grassl]	Accepted
2-1510	A	36:35	36:38	I welcome the inclusion of some estimate of the atmospheric heating via this mechanism but dislike the use of heating measured in Wm-2. Could this be changed either by changing the unit or referring to radiative perturbation rather than heating.	Accepted, mechanismn name changed to "atmospheric forcing'

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Eleanor Highwood]	
2-1511	A	36:35		uncertainties of +or- 3/w/m2 seems surprising, given the large recent field campaigns in the India and China regions. [Jerry Mahlman]	Rejected. Locally, when large radiative effects are occuring, the difference between atmospheric forcing and surface forcing (=tropopause forcing) can be highly uncertain. This is reflected by the different answers from the models.
2-1512	A	36:37	36:37	For clarity, please insert "anthropogenic" before "aerosols". [Leon Rotstayn]	Accepted
2-1513	A	36:44	36:44	Define what is a 'major' feature. It seems that this criterion of capturing the major features can be used to say that the models are performing decently, and so it should be quantified. [Tami Bond]	Accepted. Clarification has been done in text.
2-1514	A	36:45	36:45	It is the AEROCOM-A results in Kinne et al 2005, not AEROCOM-B listed in the table. [Mian Chin]	Accepted. Clarification has been done in text.
2-1515	A	36:46	36:46	Cite AeroCom Website as on page 28, line 9. [Christiane Textor]	Rejected, citation once in chapter is sufficient.
2-2719	B	36:50	36:50	non-AEROCOM [Olivier Boucher]	Accepted
2-1516	A	36:53	36:55	A note is highlighted! [NADIA GAMBOA]	Taken into account
2-1517	A	36:53	36:53	Mismatch of summing aerosol species does not imply a "medium" level of scientific understanding [Robert Levy]	Accepted, the LOSU changed to low
2-1518	A	36:55	36:55	I would like to call your attention to an alternative approach to estimating CF and DRE incorporating understanding gained from field observations of aerosol distributions and properties into calculations of perturbations in radiatev fluxes due to these aerosols. This study (Bates et al., 2005, ACPD), which focused on three regions downwind of major urban/population centers --North Indian Ocean (NIO) during INDOEX (February-March 1999), the Northwest Pacific Ocean (NWP) during ACE-Asia (March-April 2001), and the Northwest Atlantic Ocean (NWA) during ICARTT (July-August 2004)-- evaluated the current state of observations and of two chemical transport models. Measurements of aerosol burdens, AOD, and DRE were used as measurement-model check points to assess uncertainties. In-situ measured and remotely sensed aerosol properties for each region (mixing state, mass scattering efficiency, single scattering albedo, and angular scattering properties and their dependences on relative humidity) were used as input parameters to	Noted. IPCC considers articles published until December 2005. The Bates paper appeared on ACPD in January 2006. The work is however brought to the attention of the lead authors before and contains useful information.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				two radiative transfer models to constrain estimates of aerosol radiative effects, with uncertainties in each step propagated through the analysis. Constraining the radiative transfer calculations by observational inputs increased the AOD (34±8%), top of atmosphere DRE (32±12%), and TOA direct climate forcing of aerosols (37±7%) relative to values obtained with "a priori" parameterizations of aerosol loadings and properties. The resulting constrained 24 hour average clear sky TOA DCF was -3.3±0.47, -14±2.6, -6.4±2.1 W m -2 for the NIO, NWP, and NWA, respectively. Constraining the radiative transfer calculations by observational inputs was found to reduce the uncertainty range in the DCF in these regions relative to global IPCC [2001] estimates by a factor of approximately 2. [Tim Bates]	
2-1519	A	36:56	36:56	Perhaps it would be useful to summarize and evaluate the principal sources of uncertainty in computing aerosol RF - global and regional optical depth distributions, aerosol composition, internal vs external mixing, aerosol size distribution, vertical distribution, refractive indices, etc. An important question to examine is aerosol forcing under clear-sky and cloudy-sky conditions. Essentially all aerosol measurements refer to clear-sky conditions. It important to know whether GCM aerosol diagnostics refer to clear-sky or total-sky conditions. Since relative humidity is systematically higher under cloudy-sky conditions, hygroscopic aerosol optical depths will also be systematically larger under cloudy-sky conditions. While the presence of cloud greatly reduces the aerosol radiative forcing under cloudy-sky conditions, the aerosol cloud-sky contribution is hardly negligible. As a minimum, it would be important to know how GCMs treat aerosols under cloudy-sky conditions, and whether reported GCM aerosol radiative forcings are really clrear-sky or total-sky values. [Andrew Lacis]	Taken into account. The discussion on the discrepancy between measurements based direct forcing estimate and model derived forcing is readjusted. Note that models report all sky forcing.
2-1520	A	37:0		A fairly general comment on the division between this and Chapter 7. The statement in Chapter 7, p. 63 that chapter 2 deals with direct effects as well as the radiative consequences of the semidirec effect and Chapter 7 with indirect effects seems clear. Yet there is an entire section in Chapter 2 (2.4.6) on aerosol-cloud interactions. There seems to me reason to keep at least part of the radiative forcing of the indirect effect in Chapter 2, but some sections (e.g. 2.4.6.1.3, basic physics of CCN and IN) might better be merged into Chapter 7. [Daniel Murphy]	Noted.
2-1521	A	37:1	41:9	Chapter 2.4.6 should be improved; often it just collocates publications. For instance 2.4.6.1.2 contrasts a paper by Kaufman with one by Koren and one by Krueger&Grassl. First, there are also indications that aerosols increase cloud cover as e.g. cirrus in flight corridors (Boucher, 1999) or low level clouds in Central Europe in summer season	Accepted, the section has been re-written in entirety.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				(Grassl&Krueger, 2002); second, mechanisms causing this effect (if one) are different over the North Atlantic, Amazonia and China. In TAR the believe was that aerosols increase cloud cover and now the impression is given that the overall believe is that aerosols decrease cloud cover. Both hypothesis are not well based. [Johann Feichter]	
2-1522	A	37:1	42:8	Comment on section 2.4.6: "During ADEC experiments (Mikami et al., 2005), Sakai et al. (2004) conducted a raman lidar measurement over Tsukuba (36.1 °N, 140.1°E), Japan. They found ice clouds associated with the Asian dust layer at an altitude of approximately 6 to 9 km. The relative humidity in the cloud layer was close to the ice saturation values and the temperature at the top of the cloud layer was approximately -35°C, suggesting that the Asian dust acted as ice nuclei at higher temperatures. This ice-saturated region was formed near the top of the dust layer. These processes will generally occur in East Asia in a "background Kosa" environment (Murayama, 2001). Even at great transport distances from Asian dust source region, Sassen (2005) found a connection between transported Asian aerosols and icy dust clouds observed during spring 2004 over the interior of Alaska. These findings suggest that an indirect effect of desert dust on cloud formation and composition may be considerable. Quantification of a RF from this mechanism is not achieved at present due to lack of observational facts and modeling effort. I hope that you will briefly refer to this in a relevant section in 2.4.6." [Masao Mikami]	Taken into account
2-1523	A	37:1		Section 2.4.6 Cloud-Aerosol Interactions does a good job of summarising the current state of understanding, but has a couple of weak points. Firstly, it doesn't link observations of the indirect effect with model estimates. Ultimately the error bars on the forcing chart come from models, but it seems to me that the observations alone would result in an even larger uncertainty range. In fact, it seems that aerosol sometimes leads to cloud clearing and sometimes to increased cloud cover (although I appreciate that you don't treat these as forcings). Somehow the models and the observations need to be brought together and you should avoid giving the misleading impression that it is only model uncertainty that counts. Secondly, this section seems to miss out almost entirely the response of deep convective clouds to changes in aerosol. It is at present very hard to give a consensus view on the effects, but deep cloud responses should warrant a subsection of their own. The paragraph starting line 10 p38 mixes information on deep clouds (Andreae et al.) with information on shallow clouds. As written, it is confusing and could be better separated. The response of mixed phase clouds in general could also be more clearly separated. My overall concern is that this chapter tries to shoehorn all effects of constituent changes into the concept of a forcing or a response that leads to radiative changes. Deep clouds are probably unimportant radiatively but energetically their response to aerosol may be important.	Accepted, the section has been re-written. Larger discussion now on constraints to the model results from observations. Re: deep clouds, comment noted, the non-radiative aspects delt with in CH. 7

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Kenneth Carslaw]	
2-1524	A	37:7	37:7	I don't think mis-leading terms should be used "for convenience". [Joyce Penner]	Taken into account
2-1525	A	37:10	37:10	Add reference to Junge, 1975, who first proposed separating aerosol effects into direct and indirect mechanisms. Junge, C. E. (1975). The possible influence of aerosols on the general circulation and climate and possible approaches for modelling. The Physical Basis of Climate and Climate Modelling, Global Atmospheric Research Program (GARP) Publication no. 16, World Meteorological Organization, International Council of Scientific Unions Joint Organizing Committee, pp.244-251. [Theodore Anderson]	Taken into account
2-1526	A	37:13	37:17	This is where sea salt aerosols make a significant contribution. [Andrew Lacis]	Noted
2-1527	A	37:13	37:19	In other words, a very difficult scientific challenge. [Jerry Mahlman]	Noted
2-1528	A	37:16	37:17	The references (Tang, 1997; McInnes et al.,1998; Ming and Russell, 2002).are not appropriate : change with ……..(Mc Figganns et al., 2005 and references herein) [MARIA CRISTINA FACCHINI]	Taken into account. The references are relevant.
2-1529	A	37:17	37:17	Add before "Cloud optical properties...": Furthermore, the chemical composition of the aerosol particles affects ice formation in clouds (Yin et al., 2002, Diehl and Wurzler, 2004,). References: Yin, Y., S. Wurzler, Z. Levin and T.G. Reisin, 2002: Cloud processing of mineral dust particles and its effect on subsequent development of cloud, precipitation and optical properties. J. Geophys. Res. 107, D23, doi:10.129/2001JD001544; Diehl, K., and S. Wurzler, 2004: A freezing module for heterogeneous drop freezing in immersion mode. J. Atmos. Sci., 61, No 15, 2063-2073 [Sabine Wurzler]	Taken into account
2-1530	A	37:23	37:23	I suggest to replace "surface properties" by "surface energy budgets and snow albedo" [Johann Feichter]	Taken into account
2-1531	A	37:26		I think that the following evidence found by Asano et al. (2002) is important But, this paper is not referred. Asano et al. (2002) analyzed the radiation budget of stratocumulus over the East China Sea; the measurement was made by using two synchronized aircrafts below and above cloud layer. In the case of cloud polluted by continental aerosol, the cloud layer absorbed substantial amount of solar radiation in the visible region. This absorption is not so-called anomalous solar absorption. Asano et al (2002) found that the observed radiative property could be reasonably simulated with aerosol-cloud model which are made of water	Taken into account

No.	Batch	Page:line From	Page:line To	Comment	Notes
				droplets, internally-mixed soil-dust and solutions of water-soluble aerosol. This result shows that the cloud layer polluted by aerosol enhances solar heating in boundary layer cloud. Asano, S., A. Uchiyama, A. Yamazaki, J.-F. Gayet, and M. Tanizono, 2002: Two case studies of winter continental-type water and mixed-phased stratocumuli over the sea. II: Absorption of solar radiation, J. Geophys. Res., 107(D21), 4570, doi: 10.1029/?2001JD001108. [Akihiro Uchiyama]	
2-1532	A	37:32	37:39	I would replace lines 32-39 based on my general comments above to: "The estimates in Feingold et al. 2003 confirm that the relationship between aerosol and cloud droplet number concentrations is non-linear, e.g., Nd ~ (Na)b where Nd is the cloud drop number density and Na is the aerosol number concentration. The parameter b in this relationship can vary widely, highlighting sensitivity to aerosol characteristics (primarily size distribution), updraught velocity, and the usage of aerosol extinction as a proxy for CCN (Feingold, 2003). Absolute values of b are therefore only approximate and belie the underlying complexity of the system. Disparity in estimates of b (or equivalent) based on satellite studies (Nakajima et al. 2001; Breon et al. 2002) also suggests that a quantitative estimate of the albedo effect from remote sensors is problematic (Rosenfeld and Feingold 2003), particularly since measurements are not considered for similar liquid water paths. [Graham Feingold]	Taken into account
2-1533	A	37:41	37:54	This would be a good place to mention how meteorological conditions can influence cloud properties, meaning that a careful analysis of observational data is needed in order to isolate the influence of aerosols. For instance, if cloud updrafts become stronger due to increased buoyancy, this will lead to a larger cloud droplet number and a smaller cloud droplet size, even if aerosol number and aerosol properties are held constant. [Jón Egill Kristjánsson]	Taken into account
2-1534	A	37:43	37:	should read "The modelling study of Jiang et al. (2002) shows that.." [Graham Feingold]	Accepted and corrected
2-1535	A	37:43	37:44	Remove parentheses around "Jiang et al." [Patrick Hamill]	Accepted and corrected
2-1536	A	37:43		delete the bracket before "Jiang" [Hartmut Grassl]	Accepted and corrected
2-1537	A	37:43		This and following citations incorrectly formatted [Steven Sherwood]	Accepted and corrected
2-1538	A	37:44		add a bracket before "2002" [Hartmut Grassl]	Accepted and corrected

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1539	A	37:45	37:48	The wording of this implies to me that the albedo effect, as posed by Twomey, has been misunderstood. I don't believe that Twomey ever suggested that LWP is constant, but only that if one were to consider a fixed LWP, then addition of aerosol would increase albedo. Changes in LWP with changes in column-averaged drop number fall into the realm of "cloud lifetime effects". Thus I would modify lines 45-48 to read "Han et al. (2002) analyse the cloud climatology derived from AVHRR, for warm clouds during daytime to examine the relationship between enhanced drop concentrations and liquid water path. The liquid cloud sensitivity can be defined as…" [Graham Feingold]	Taken into account: Replaced "premise" by "relationship" in text
2-1540	A	37:45	37:45	Remove parentheses around "Brenguier..." [Patrick Hamill]	Accepted and corrected
2-1541	A	37:45	37:48	Sentence unclear. [Patrick Hamill]	Noted, sentence slighted re-worded
2-1542	A	37:52	37:52	Before "The results highlight…" I would add "The absence of a clear change in LWP response to changes in aerosols has also been derived from large eddy simulations of stratocumulus clouds (Jiang et al. 2002; Ackerman et al. 2004; Lu and Seinfeld, 2005) and of cumulus clouds (Xue and Feingold 2005; Jiang and Feingold, 2005)." [Graham Feingold]	Accepted
2-1543	A	37:52	37:53	Change to: "The results highlight the difficulty of devising observational studies that can isolate the albedo effect (for clouds of similar LWP) and effects related to changing meteorology and cloud dynamics that affect LWP and therefore cloud RF." [Graham Feingold]	Accepted
2-1544	A	38:1		add: "Krüger and Grassl, 2002 [Hartmut Grassl]	Accepted
2-2720	B	38:7	38:7	can you refer to the original papers instead? [Olivier Boucher]	Accepted
2-1545	A	38:10	38:13	Earlier references to the effect of aerosol on cloud droplet size is the laboratory work of Gunn and Philips 1957. Gunn R, Phillips B.B., An EXPERIMENTAL INVESTIGATION OF THE EFFECT OF AIR POLLUTION ON THE INITIATION OF RAIN, J. METEOR. 14 (3): 272-280 1957 [Yoram Kaufman]	Accepted
2-1546	A	38:10	38:19	This needs more careful explaining to merit its conclusions in this Assessment Report. [Jerry Mahlman]	Taken into account
2-1547	A	38:14	38:14	Is "water vapour content" correct? According to line 18 there is an increase in "relative humidity". This is confusing.	Taken into account, check reference

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Jón Egill Kristjánsson]	
2-1548	A	38:15	38:16	"strong negative correlation" between what quantities? [Jón Egill Kristjánsson]	Noted, as stated on line 14
2-1549	A	38:15	38:19	I suggest that the work of Sherwood (2002) be cited as a hypothesis, and not as established fact. The biomass fire - ice crystal coupling, and an "increase" in relative humidity, is not well established (i.e. there are studies that state that humidity in the lower stratosphere did not increase during the last several decades). [Steven Massie]	Taken into account, stated as hypothesis
2-1550	A	38:16	38:19	You attribute some of the increased relative humidity in the stratosphere to the evaporation of ice crystals in convective clouds above biomass burning. How much evidence is there that these cloud penetrate the tropopause? [Eleanor Highwood]	Taken into account and uncertainty on frequency of tropopause pnetration stated.
2-1551	A	38:16	38:19	Likely causing smaller ice crystals...?? This is a fairly positive endorsement of the Sherwood 2002 idea, especially in light of the fact that there is a recent downturn in stratosopheric H2O. [Joyce Penner]	Taken into account, stated as hypothesis
2-2721	B	38:21	38:25	Isn't this true for models as well? But are we interested in the very fine scale when it comes to estimate the RF? [Olivier Boucher]	Noted, only if the fine scale is shown to affect the result
2-1552	A	38:21	38:25	The much larger effective radii over remote oceans than over highly polluted continental regions have been known for a long time. Perhaps these 5 lines could be moved to the beginning of this subsection, since they make such basic statements. [Jón Egill Kristjánsson]	Taken into account and moved forward
2-1553	A	38:21		Which satellite data suggest larger effective radii in remote regions? Of course this makes physical sense! Do some satelite data not suggest larger effective radii? [Robert Levy]	Noted
2-1554	A	38:22	38:24	Syntax problem [Patrick Hamill]	Noted
2-1555	A	38:24	38:25	This sentence needs clarification. What does "it" refer to? [Joyce Penner]	Noted
2-1556	A	38:27	38:35	Need to make clear that the Koren effect has been observed only over land and it is not clear the extent of this effect beyond the source regions. [Santiago Gassó]	Taken into account
2-2722	B	38:27		This is an important conclusion if true. It needs to be assessed further.	Noted. Satellite retrievals do have

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Olivier Boucher]	uncertainties.
2-1557	A	38:28	38:28	Do you mean the PNAS article by Kaufman et al.? It did not use MISR [Joyce Penner]	Accepted, not MISR but MODIS
2-1558	A	38:28	38:29	This is a very important point, yet it only gets two lines, and really does merit more discussion. If the relation between aerosol and cloud cover is really as large as suggested by Kaufman et al., then the negative forcing due to the cloud-lifetime effect is very large, and we may need to allow for the possibility of a net negative anthropogenic forcing, which is inconvenient to say the least. It also makes it hard to justify leaving the cloud-lifetime effect out of the main results in this chapter, since it is effectively saying that the cloud-lifetime effect is much larger than the cloud-albedo effect. However, there are many uncertainties related to the satellite retrievals, and the extent to which they do reveal a strong cloud-lifetime effect (e.g., J. L. Zhang et al., GRL, 2005). Note that some of the effects discussed by Zhang et al. are artifacts of the satellite retrieval (such as multiple reflections from clouds) whereas there are also real physical effects that may be detected, but which do not show that aerosols increase cloud cover (higher RH implies more cloud, and also larger aerosol optical depth). Perhaps some of the satellite experts (including Kaufman) can help you with this difficult topic. [Leon Rotstayn]	Noted, yes uncertainties are not small in satellite retrieals; passed comment to Ch. 7
2-1559	A	38:28	:29	Kaufman et al used only MODIS not MISR data in that study. [Yoram Kaufman]	Accepted
2-1560	A	38:29	38:29	The paper in PNAS by Kaufman et al. (2005) would be a natural reference here. [Jón Egill Kristjánsson]	Accepted , ref. added
2-1561	A	38:29		You may want to add: " noticable increase in the cloud height was also measured for convective clouds (Koren et al2005)." 2. Koren, I. Y. J. Kaufman, L. A. Remer, D. Rosenfeld and Y. Rudich Aerosol impact on the development and coverage of Convective clouds, Geoph. Res. Lett. 32 (14): Art. No. L14828 JUL 30 2005] [Yoram Kaufman]	Accepted
2-1562	A	38:31	38:35	This paragraph sounds like it contradicts the previous paragraph [Patrick Hamill]	Taken into account and reworded slightly.
2-1563	A	38:31	38:35	I thought it might be better to include this in the semi-direct section on the following page, which lacks any hint that there may be an observational basis for its existence [Keith Shine]	Noted, semi-direct now removed from section and discussed further in CH. 7
2-1564	A	38:41	38:43	The references are not appropriate and in the correct place, change with : "Nevertheless, earlier observations of fog water (Facchini et al., 1999; Facchini et al., 2000), suggest that the presence of organic aerosols may reduce surface tension and lead to and increase in the cloud droplet number concentration (Nenes et al. 2002; Ming et al., 2005a; Mc Figgans et al., 2005)" .	Accepted. Will check the two Facchini references whether the later one is more appropriate?

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[MARIA CRISTINA FACCHINI]	
2-1565	A	38:44	38:44	Last word: instead of "drop-size" it should be "particle size" [Sabine Wurzler]	Accepted
2-1566	A	38:48	38:48	Insert before "The review study..": "Ervens et al. (2005) address numerous composition effects in unison to show that the effect of composition on drop number concentration is much less than suggested by studies that address individual composition effects such as surface tension." [Graham Feingold]	Accepted
2-1567	A	38:49	38:49	"point outs" should be 'points out' [Jón Egill Kristjánsson]	Accepted
2-1568	A	38:52	39:3	The new perspective on state of mixture and morphology has also been derived from a number of studies using Transition Electron Microscopicity (TEM) equally important at to the results derived by the aerosol mass spectrometers. Relevant papers: Bigg, E.K., and C. Leck, 2001, Properties of the aerosol over the central Arctic Ocean, J. Geophys. Res., 106 (D23), 32,101-32,109. Leck, C., M. Norman, E.K. Bigg, and R. Hillamo, 2002, Chemical composition and sources of the high Arctic aerosol relevant for fog and cloud formation, J. Geophys. Res., 10, dio:10.1029/2001JD001463. Leck, C., and E.K. Bigg, 2005a, Biogenic particles in the surface microlayer and overlaying atmosphere in the central Arctic Ocean during summer, Tellus 57B, 305-316. Leck, C., and E.K. Bigg, 2005b, Evolution of the marine aerosol – A new perspective, Geophys. Res. Lett., 32, L19803, doi:10.1029/2005GL023651. Lohmann, U., and C. Leck, 2005, Importance of submicrone surface active organic aerosols for pristine Arctic Clouds, Tellus 57B, 261-268. Pósfai, M., Li, J., Anderson, J.R. and Buseck, P.R. 2003. Aerosol bacteria over the Southern Ocean during ACE-1. Atmos., Res. 66, 231-240. [Caroline Leck]	Noted, how are biogenic particles in pristine conditions relevant to the anthropogenic contribution?
2-1569	A	38:53		a comma is missing after "mixtures" [Hartmut Grassl]	Accepted
2-1570	A	38:56	38:56	Suggest to insert the following sentence after "formation." Sentence: "Parametrizations of mineral dust as heterogeneous ice nuclei have been provided by recent laboratory experiments (e.g., Zuberi et al. Hung et al. 2003)." (Zuberi, B.; Bertram, A. K.; Cassa, C. A.; Molina, L. T.; Molina, M. J. Geophys. Res. Lett. 2002, 29, 142-1-142-4 (10.1029/2001GL014289).) (Hung, H. M., Malinowski, A., and Martin, S. T., "Kinetics of Heterogeneous Ice Nucleation on the Surfaces of Mineral Dust Cores Inserted into Aqueous Ammonium Sulfate Particles," Journal of Physical Chemistry A, 2003, 107,	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				1296-1306.) (Archuleta CM, DeMott PJ, Kreidenweis SM, Ice nucleation by surrogates for atmospheric mineral dust and mineral dust/sulfate particles at cirrus temperatures, ATMOSPHERIC CHEMISTRY AND PHYSICS 5: 2617-2634 OCT 4 2005) [Scot Martin]	
2-1571	A	39:0	40:	In the entire chapter, this section seemed the 'odd man out'. Basically the magnitude of the true cloud albedo indirect effect is unknown. Presumably it is negative. The value quoted in this section is simply an estimate from models that cannot explicitly treat cloud microphysics and ignore real world cloud heterogeneity issues. The uncertainty is likely to be larger than simply the spread of model values. Rather than giving this what amounts to equal billing with the other, better known sources of RF, I would recommend stating the overall result without subtracting its value of 1.2 (giving 2.7 as the main result) then qualifying this with a statement along the lines of 'estimates of the aerosol albedo effect are highly uncertain, but may reduce this value by up to 2 w m-2'. [Roger Davies]	Accepted and the section re-worded to stress the constraints on the model results. Also, the revised Fig.2.9.1 will show the pdf of only the more well known forcings.
2-1572	A	39:0		Section 2.6.2 This section summarizes the literature but does not critique the results. Current GCM's do not have the microphysics or subgrid scale processes to accurately predict cirrus clouds or contrails explicity in the models. The parameterizations to address contrails introduce significant uncertainties. This should be discussed in this section as well. [Steven Baughcum]	Taken into account and more information is presented about parameterizations in Table 2.4.6
2-1573	A	39:0		Cloud lifetime effect and Semi-direct effect I would raise several concerns associated with classifying these effects as "first-response feedback effects" rather than forcings in the context of climate forcing and response: 1. It should be remembered why the cloud lifetime effect and the semi-direct effect were classified as forcings in the first place. The occurrence of each of these phenomena exerts an immediate change in net absorbed flux at the TOA, a change in the absorbed solar energy that drives the planetary climate system. As these changes are exactly the nature and kind of changes associated with other forcings, it was (and arguably still is) reasonable to classify these phenomena as forcings, with the expectation that the climate system response to these phenomena, on climatically meaningful time scales of years to decades, will be the same, per change in net TOA radiative flux, as for other forcings. 2. As these responses are quite rapid (time scales of minutes to hours) it is likely that these phenomena are subgrid relative to the time steps and spatial grid of GCMs, and thus likely to be missed in GCM calculations. So not only would these phenomena not show up as a forcing, they would be missing altogether in modeled climate response to anthropogenic aerosols. It thus remains to be demonstrated, by climate model calculations, whether in fact the models can capture these phenomena. I doubt if such	Noted and passed to CH. 7 as Chap. 2 is going to restrict itself to RF as defined in this chapter. (1) The context and implication of the word "immediate" is a subjective one. There can well be an "immediate" change in water vapor and cloud amount (actually so in some models) such that the flux changes would then not be classifiable as RF per the definition. (2) The timescale of this "immediate" effect is not clear, at least according to models. And, in any case, it seems difficult to use observations to quantify the "immediate-ness". Reject that they do not show up in model response as in 20th century climate integrations – all

No.	Batch	Page:line From	Page:line To	Comment	Notes
				calculations have been done, and thus it is likely that these phenomena have slipped through the cracks by this change in classification. I would note further that the time and space scales of these phenomena are much shorter than the stratospheric adjustment to increased GHG concentrations, which is nonetheless considered on the forcing side of the equation in considerations of climate sensitivity. 3. As the changes in atmospheric fluxes associated with the cloud lifetime effect and the semi-direct effect are no longer deemed a forcing, hence the "forcings" associated with the increases in aerosol responsible for these phenomena are artificially diminished. Assuming that these phenomena are in fact captured by GCMs, the result would be that the response of the climate system and attributed to anthropogenic aerosol would be the same as if these phenomena were considered forcings, and therefore much greater than that associated with the direct aerosol effect and with the cloud albedo indirect effect (or per unit forcing, much greater than the climate system response per forcing associated with other forcings). This would have the effect of artificially changing the climate system response per forcing (climate sensitivity) as a consequence of putting these changes in radiative flux on the response side of the equation. It would certainly seem that these issues should be aired in the present report. [Stephen E Schwartz]	of the 3 effects are implicit when a model allows for aerosol-cloud interactions. So, the phenomena does not slip through the proverbial crack. Instead, the problem that the literature has not comprehensively resolved is at what point does "feedback" begin, and up to what extent the process can be said to be within the purview of RF, and whether this is robust across models; nevertheless, the full model response captures the totality of the effect. Agreed that caution has to be exercised when speaking of climate sensitivity in the context of the aerosol-cloud interactions.
2-1574	A	39:1	39:27	I think that these explanations need to be simplified for the non-expert IPCC report reader. Without this, most casual, but scientifically literate, readers would simply skip this. Simplify, if possible. One way to achieve this would to invite some of the Chapter scientists to take a hand in dealing with this "opaque information" problem. [Jerry Mahlman]	Noted
2-1575	A	39:1	39:1	Add after Maria et al., 2004: In order to make the picture even more complex, apparently there coexist several different types of internally mixed particles, differing in their contents of soluble and insoluble substances and thereby significantly affecting cloud droplet sizes (e.g., Eichel et al. 1996). Reference: Eichel, C., M. Krämer, L. Schütz and S. Wurzler, 1996: The water-soluble fraction of atmospheric aerosol particles and its influence on cloud microphysics. J. of Geophys. Res. 101, 12,499-12,510 [Sabine Wurzler]	Taken into account
2-1576	A	39:2	39:2	Please add Aitken in between nucleation and accumulation. [Caroline Leck]	Rejected: nucleation is Aitken mode
2-1577	A	39:5	39:6	Rephrase: The chemical composition of the institial particles differs from the nuclei in the ice crystals. Homogeneous freezing is likely one ice forming mechanism at relatively low temperatures. [Sabine Wurzler]	Taken into account, split into 2 sentences
2-1578	A	39:10	39:10	Add: The chemical composition not only of the soluble but especially of the insoluble	Taken into account, include refs.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				fraction (e.g., dust, soot) of the aerosol particles affects significantly heterogeneous ice formation in mixed phase clouds and thereby their optical properties, as has been shown by Diehl and Wurzler (2005) and Lohmann and Diehl (2005) by a combination of laboratory and model studies. References: Diehl, K., and S. Wurzler, 2004: A freezing module for heterogeneous drop freezing in immersion mode. J. Atmos. Sci., 61, No 15, 2063-2073; Lohmann, U., and K. Diehl, 2005: Sensitivity studies of the importance of dust ice nuclei for the indirect aerosol effect on stratiform mixed phase clouds. J. Atmos. Sci. in press [Sabine Wurzler]	
2-2723	B	39:19		Can you assess recent literature on this: Koren et al (2005) / Kaufman et al. (2005) ? What are the evidences, what are the caveats? [Olivier Boucher]	Taken into account
2-1579	A	39:20	39:27	It would seem natural to link this discussion to subsection 2.4.6.1.2 (or vice versa). We still don't fully understand the changes in cloud cover, but it is conceivable that it is partly linked to the lifetime effect. [Jón Egill Kristjánsson]	Noted, 2.4.6.1.2 no longer in section
2-1580	A	39:29	39:29	Include something on observations which at the least hint at the semi-direct effect [Keith Shine]	Noted, discussion on semi-direct has been moved to CH. 7
2-1581	A	39:36	39:36	Here it would be natural to add 'Kristjánsson et al. (2005)' after or before "Hansen et al., 2005". The full reference is: Kristjánsson, J. E., T. Iversen, A. Kirkevåg, Ø. Seland, and J. Debernard, 2005: Response of the climate system to aerosol direct and indirect forcing - the role of cloud feedbacks. J.Geophys.Res., in press. [Jón Egill Kristjánsson]	Accepted
2-1582	A	39:36	39:36	"fractions" - note that within our LES studies, the semi-direct effect in stratocu acts more via a thinning of the cloud, rather than a reduction of cloud fraction. I suspect GCMs and reality differ in this respect [Keith Shine]	Noted, discussion on semi-direct has been moved to CH. 7
2-2724	B	39:37	39:37	this is a bit simplified, isn't it? [Olivier Boucher]	Noted, discussion on semi-direct has been moved to CH. 7
2-1583	A	39:37	39:39	In Cook and Highwood (2004) it is clear that part of the cloud changes in aerosol runs are due to changes in surface temperature after the addition of aerosol. Do you mean surface temperature effects here or are you referring to something totally different? [Eleanor Highwood]	Noted, discussion on semi-direct has been moved to CH. 7
2-1584	A	39:44	39:44	Before "Johnson (2005), insert " … as well as the importance of modification to land surface fluxes in reducing cloudiness (Feingold et al. 2005)" [Graham Feingold]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2725	B	39:50		Some of the recent work by Quaas could be referenced here. Quaas J., O. Boucher (2005), Constraining the first aerosol indirect radiative forcing in the LMDZ GCM using POLDER and MODIS satellite data, Geophys. Res. Lett., 32, L17814, doi:10.1029/2005GL023850. [Olivier Boucher]	Taken into account; section now includes more discussion of the observational constraints of the cloud albedo effect
2-2726	B	39:53	39:53	in a more rigorous way: can you say why? Or detail the advances in modelling in an earlier section. [Olivier Boucher]	Taken into account
2-1585	A	39:55	39:56	change to: "The main problems in the comparison between model results still resides in the sub-grid representation of clouds, and in the formulation of the relationships between…" [Graham Feingold]	Taken into acocunt, inlcude phrase the : sub-grid representation of clouds
2-1586	A	39:55	40:2	Note that Chen and Penner (A.C.Phys. Disc, 2005 -- I think it is accepted now) do not find large differences in forcing associated with the particle to droplet parameterization. There are larger differences associated with the basic cloud fields in the models, I suspect. [Joyce Penner]	Taken into account
2-1587	A	40:4	40:6	The experimental set-up should be described in more detail. What values for CO2 were applied? [Eugene Rozanov]	Taken into account
2-2727	B	40:6	40:6	gas precursors (no dash) [Olivier Boucher]	Accepted
2-1588	A	40:8	40:10	With only a little stretching, one can claim that the indirect RF should virtually cancel out the RF due to CO2! It seems that more attention to quantify these forcings is needed. [Jerry Mahlman]	Taken into account; also revised Figure 2.9.1 has pdf with only the well known forcings
2-1589	A	40:8	40:10	Is this statement based on Mc Figgins et al.? [Joyce Penner]	Maybe line numbers wrong?
2-1590	A	40:8	40:10	Clarify how the best estimate and range for indirect sulfate are arrived at. Why is a lower indirect aerosol forcing not indicated by the "best" models apparently showing smaller forcing (4 left hand models in 2.4.4 plus average of common model in rhs of 2.4.4b) ? [Peter Stott]	Taken into account, average and standard deviation of all model results, not assigning particular weights.
2-1591	A	40:15	40:15	change "disposition" to "deposition". [Graham Feingold]	Accepted
2-2728	B	40:16	40:16	disposition??? [Olivier Boucher]	Accepted
2-1592	A	40:16		disposition [Junying Sun]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1593	A	40:17	40:19	I know of at least one other study which satisfies these criteria and that is 'Storelvmo et al. (2005)', which is already in the reference list. In that study an even smaller estimate of the aerosol indirect is found, as shown in Figure 7.5.3 in Chapter 7. So, the results of that paper confirm the statement made here.G6 [Jón Egill Kristjánsson]	Taken into account
2-1594	A	40:17	40:19	I find this statement curious since Lohmann et al. used the algorithm of Chuang et al. 2002. [Joyce Penner]	Taken into account
2-1595	A	40:17	40:19	I query whether the smallest cloud-albedo effect came from Lohmann et al (2000) because of the assumed internal mixing, or because of the assumption that sulfate formed by in-cloud oxidation was assumed to condense entirely on existing particles. It might be best to check with Ulrike Lohmann. [Leon Rotstayn]	Taken into account; checked with UL
2-1596	A	40:20		delete included [Junying Sun]	Accepted
2-1597	A	40:27	40:28	add: "W m-2" after "0.078" and "0.5345" [Hartmut Grassl]	Accepted
2-1598	A	40:27	40:28	a lot of significant figures! [Keith Shine]	Accepted and modified
2-1599	A	40:27		0.078 Wm-2 [Junying Sun]	Accepted
2-1600	A	40:28	40:29	Authors say that other models will be added. This section *must* be expanded to include other models. Fewer decimal places would also be appropriate. I hope that a central value can be chosen for each indirect effect in this report, even if the uncertainties are large. [Tami Bond]	Accepted
2-1601	A	40:28	40:28	Why quote numbers to such a high precision ? [Peter Stott]	Accepted
2-1602	A	40:28		0.5345 W m-2 [Junying Sun]	Accepted
2-1603	A	40:29	40:30	A whole sentence is highlighted! [NADIA GAMBOA]	Accepted
2-1604	A	40:30		The report could have been much better if a comprehensive comparison between the models and measurements would be done also for the indirect effect at least on the regional sclae where measurements are available. [Yoram Kaufman]	Taken into account; more discussion is included now
2-2729	B	40:38	40:41	This is a bit expeditous, isn't it? If it is as large as -1.4 Wm-2 it would certainly deserve	Noted; discussion has been removed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				more attention. Moreover the text points to section 2.8 but there is very little discussion there of the efficacy of the first indirect effect. If the lifetime effect is included in the efficacy of the cloud albedo effect, then how come the upper bound of its efficacy is 1.1 !?! [Olivier Boucher]	and is in CH. 7
2-1605	A	40:39	40:41	How these numbers were calculated? [Eugene Rozanov]	Noted; discussion has been removed and is in CH. 7
2-1606	A	40:39	41:9	The text should be modified to include the recent combined satellite and model results limiting the aerosol indirect contribution to -0.3 to -0.5 W/m^2 (J. Quass et al., Constraining the total aerosol indirect effect in the LMDZ and ECHAM4 GCMs using MODIS satellite data, Atmos. Chem. Phys. Discuss., 5, 9669-9690, 2005). [Petr Chylek]	Taken into account and discussion included.
2-1607	A	40:40	40:40	Since it is argued by the authors that the cloud-lifetime effect is not a "forcing", it would be more consistent to use a different term here, such as "radiative perturbation". [Leon Rotstayn]	Noted; but paragraph removed and discussion is now in CH. 7
2-1608	A	40:41	40:41	It is stated that the cloud lifetime effect is included in the efficacy term (Section 2.8) but I see nothing about it in Section 2.8. It would be better to cross-reference to Chapter 7, where the authors do make a serious effort to deal with the cloud lifetime effect. [Leon Rotstayn]	Noted; discussion is now in CH. 7
2-2730	B	40:44	40:44	Do you mean there is a larger probability for the semi-direct effect to be negative than positive? Where do these estimates come from? Can you provide references? [Olivier Boucher]	Noted; discussion is now in CH. 7
2-1609	A	40:44	40:44	What references are you using for these estimates? [Keith Shine]	Noted; discussion is now in CH. 7
2-1610	A	40:47	40:49	It is unclear to which values are for TOA and which are for SRF in Table 2.4.6 [Istvan Laszlo]	Taken into account, reference to surface forcing removed from text (not included in table)
2-1611	A	40:47	40:47	The term "these indirect effects" is inaccurate, because Table 2.4.6 only gives results for the cloud-albedo and total indirect effects. [Leon Rotstayn]	Taken into account, written out explicitly
2-2731	B	40:50	40:51	I do not understand the meaning of this sentence (although I think I see what the authors mean). Could you reword as: "…it is possible to obtain near zero TOA radiative forcing but with a significant redistribution of the relative solar heating between the tropsphere and the surface (ie more heating of the atmosphere and less heating of the surface)." [Olivier Boucher]	Taken into account, reference to surface forcing removed from text
2-1612	A	40:50	40:51	"radiative fluxes … tropospheric fluxes" (?)	Taken into account, trpospheric

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Hartmut Grassl]	replaced by TOA
2-1613	A	41:0		The soluble gas effect on cloud formation was discussed in the 2nd and 3rd assessment reports, and since its influence on the radiative balance remains unknown, it constitutes an uncertainty which should be discussed under subsection 2.4.6.4. I suggest adding the following paragraph and references: An issue which has not been included in climate models so far is the effect of soluble gases such as HNO3 (Kulmala et al, 1993; Laaksonen et al., 1998; Xue and Feingold, 2004) and NH3 (Hegg, 2000; Romakkaniemi et al., 2005a), which can cause an increase in cloud drop activation and thereby influence cloud albedo. Chen and Penner (2005) have shown that the uncertainty in radiative forcing caused by different parametrizations relating the aerosol field and cloud drop number concentrations is as high as 0.5 Wm-2. It is probable that the maximum soluble gas effect to the radiative balance is on the same order, since the gases also cause uncertainty in the activated fraction of aerosol particles. Climate model simulations of the effect of HNO3 are now possible using the parametrization of Romakkaniemi et al. (2005b). Chen, Y., and J.E. Penner, 2005: Uncertainty analysis for estimates of the first indirect aerosol effect. Atmos. Chem. Phys. Discuss., 5, 4507-4543. Hegg, D.A., 2000: Impact of gas-phase HNO3 and NH3 on microphysical processes in atmospheric clouds. Geophys. Res. Lett., 27, 2201-2204. Kulmala, M., A. Laaksonen, P. Korhonen, T. Vesala, T. Ahonen, and J.C. Barrett, 1993: The effect of atmospheric nitric acid vapor on cloud condensation nucleus activation. J. Geophys. Res., 98, 22949-22958. Laaksonen, A., P. Korhonen, M. Kulmala, and R.J. Charlson, 1998: Modification of the Köhler equation to include soluble trace gases and slightly soluble substances. J. Atmos. Sci., 55, 853-862. Romakkaniemi, S., H. Kokkola, and A. Laaksonen, 2005a: Soluble trace gas effect on cloud condensation nuclei activation: Influence of initial equilibration on cloud model results. J. Geophys Res.,110, D15202, doi:10.1029/2004JD005364. Romakkaniemi, S., H. Kokkola, and A. Laaksonen, 2005b: Parameterization of the nitric acid effect on CCN activation. Atmos. Chem. Phys. 5, 879-885. Xue, H., and G. Feingold, 2004: A modeling study of the effect of nitric acid on cloud properties. J. Geophys. Res., 109, D18204, doi: 10.1029/2004JD004750. [Ari Laaksonen]	Accepted
2-2732	B	41:1	41:9	This paragraph is not part of section 2.4.6.2.3 on the semi-direct effect. Quaas et al. (Science submitted, 2005) did estimate the total aerosol effect from CERES measurements. [Olivier Boucher]	Taken into account, 2.4.6.2.3 no longer in section

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1614	A	41:7	41:7	The reference here should be to Anderson et al., 2003b. This reference is not included in the bibliography for this chapter. It is: Anderson, T. L., R. J. Charlson, S. E. Schwartz, R. Knutti, O. Boucher, H. Rodhe and J. Heintzenberg (2003b) Climate forcing by aerosols - A hazy picture, Science, 300, 1103-1104. [Theodore Anderson]	Accepted
2-2733	B	41:7	41:7	Anderson et al. (2003) report a range for the total aerosol forcing rather than the indirect effect. This reference is missing from the reference list. You want to refer to the Science paper rather than the JGR paper. [Olivier Boucher]	Accepted
2-1615	A	41:11	41:11	Section 2.4.6.4 Uncertainties: It should be stated, probably at the beginning of this section, that a major source of uncertainty in all anthropogenic aerosol forcings is poor knowledge of the actual amount and distribution of anthropogenic aerosol. This poor state of knowledge is indicated by the large range of values for component mass and optical depth among existing chemical transport models (AeroCom project, Kinne et al., 2005). [Theodore Anderson]	Accepted
2-1616	A	41:11	42:8	Please, discuss the large uncertainties associated with the simulation of marine Sc - the clouds most susceptible to aerosol effects. E.g. Bony&Dufresene, GRL, 2005: "...marine boundary layer clouds ... constitutes, currently, the main source of uncertainty..." [Johann Feichter]	Taken into account
2-1617	A	41:11	42:8	Issue 1: An important source of atmospheric aerosols is in-situ formation from gaseous precursors. Measurements conducted using different platforms (ground, ships, aircraft) and over different time periods (campaign or continuous-type measurements) indicate that the formation and growth of atmospheric aerosols is taking place practically all over the world (Kulmala et al. 2004a). The development of new instruments for nanoparticle and trace gas measurements have enabled a more detailed investigation of this process and enabled new discoveries. [Markku Kulmala]	Accepted or taken into account?; should this be included before 2.4.6? Consider in the "Advances" section of 2.4
2-1618	A	41:11	42:8	Issue 1, continued: For example, the first and longest continuous data set on atmospheric aerosol formation from the SMEAR II station (Kulmala et al. 2001, Mäkelä et al. 1997) show that the number of aerosol formation events may have been increasing during the recent years (Dal Maso et al. 2005). Atmospheric aerosol formation has been shown to lead to a significant increase in the number cloud condensation nuclei (CCN), even at highly-polluted regions such as Po Valley in Italy (Laaksonen et al. 2005a) and New Delhi, India (Mönkkönen et al. 2005). In forested areas, a direct observational link	Accepted or taken into account?; should this be included before 2.4.6? Consider in "Advances" section

No.	Batch	Page:line From	Page:line To	Comment	Notes
				between aerosol formation and subsequent CCN production and cloud droplet activation has been demonstrated (Lihavainen et al. 2003; Kerminen et al. 2005, Komppula et al. 2005). [Markku Kulmala]	
2-1619	A	41:11	42:8	Issue 1, continued: The radiative effect by atmospheric aerosol production is large enough (Kurten et al., 2003, Kerminen et al. 2005) that it should be taken into account when simulating the indirect radiative forcing by atmospheric aerosols. A further complication associated with this matter arises from the fact that both natural (biogenic VOCs) and anthropogenic (sulphur dioxide and anthropogenic VOCs) precursor gases are involved in atmospheric aerosol formation. [Markku Kulmala]	Accepted or taken into account?; should this be included before 2.4.6? see 1618
2-1620	A	41:11	42:8	Issue 1, References: Dal Maso M., Kulmala M., Riipinen I., Wagner R., Hussein T., Aalto P.P. & Lehtinen K. 2005. Formation and growth of fresh atmospheric aerosols: eight years of aerosol size distribution data from SMEAR II, Hyytiälä, Finland. Boreal Env. Res. (this issue). Komppula M., Lihavainen H., Kerminen V.-M., Kulmala M. & Viisanen Y. 2005. Measurements of cloud droplet activation of aerosol particles at a clean subarctic background site. J. Geophys. Res. 110, D06204, doi:10.1029/2004JD005200. Kerminen V.-M., Lihavainen H., Komppula M., Viisanen Y. and Kulmala M. (2005) Direct observational evidence linking atmospheric aerosol formation and cloud droplet activation. Geophys. Res. Lett. 32, L14803, doi:10.1029/2005GL023130. Kulmala M., Hämeri K., Aalto P.P., Mäkelä J.M., Pirjola L., Nilsson E.D., Buzorius G., Rannik Ü, Dal Maso M., Seidl W., Hoffmann T., Jansson R., Hansson H.-C., Viisanen Y., Laaksonen A. & O'Dowd C.D. 2001. Overview of the international project on Biogenic aerosol formation in the boreal forest (BIOFOR). Tellus 53B: 324--343. Kulmala M., Vehkamäki H., Petäjä T., Dal Maso M., Lauri A., Kerminen V.-M., Birmili W. & McMurry P. H. 2004a. Formation and growth rates of ultrafine atmospheric particles: A review of observations. J. Aerosol Sci. 35: 143--176. Kurtén T., Kulmala M., Dal Maso M., Suni T., Reissell A., Vehkamäki H., Hari P., Laaksonen A., Viisanen Y. & Vesala T. 2003. Estimation of different forest-related contributions to the radiative balance using observations in southern Finland. Boreal Env. Res. 8: 275--285. Laaksonen A., Hamed A., Joutsensaari J., Hiltunen L., Cavalli F., Junkermann W., Asmi A., Fuzzi S. & Facchini M.C. 2005b. Cloud condensation nucleus production from nucleation events at a highly polluted region. Geophys. Res. Lett. 32 (6): L06812 10.1029/2004GL022092 Lihavainen H., Kerminen V.-M., Komppula M., Hatakka J., Aaltonen V., Kulmala M. & Viisanen Y. 2003. Production of "potential" cloud condensation nuclei associated with	Accepted or taken into account?; should this be included before 2.4.6? see 1618

No.	Batch	Page:line From	Page:line To	Comment	Notes
				atmospheric new?particle formation in northern Finland. J. Geophys. Res. 108 (D24): 4782. Mäkelä J.M., Aalto P., Jokinen V., Pohja T., Nissinen A.. Palmroth S., Markkanen T., Seitsonen K., Lihavainen H. & Kulmala M. 1997. Observations of ultrafine aerosol particle formation and growth in boreal forest. Geophys. Res. Lett. 24: 1219--1222. Mönkkönen P., Koponen I.K., Lehtinen K.E.J, Hämeri K., Uma R. & Kulmala M. 2005. Measurements in a highly polluted Asian mega city: Observations of aerosol number size distribution, modal parameters and nucleation events. Atmos. Chem. Phys. 5: 57--66. [Markku Kulmala]	
2-1621	A	41:11		Chapter 2, Page 41, line 11 Section: 2.4.6.4 Uncertainties The following text:is suggested. Even high resolution models have difficulty accurately estimating the amount of cloud liquid water in a grid square. Guan et al. (2002) compared cloud forecasts from weather forecast models with in-situ measurements made by aircraft. For the entire in-situ dataset, three different cloud forecast schemes show a similar skill in detecting the presence of clouds, with a true skill statistic ranging between 0.27 and 0.34. The magnitude of the liquid or ice water content of the clouds was also poorly predicted, sometimes showing no correlation between in-situ measurements and model prediction (Authors note: Almost identical results/errors are being found using ground based microwave radiometers estimates of cloud liquid water path). Similar skills could be expected with climate models with resulting inaccuracies in model effective radii and LWC. For ice clouds, the particle concentrations cannot be easily measured with present equipment because of the difficulty of detecting small particles (Hirst et al., 2001) and the fact that ice particles often shatter when hitting the probes (Korolev and Isaac, 2005). The Meyers et al. (1992) parameterization of ice nuclei is often used to determine the ice particle concentration in GCMs, although it was formed using measurements at the surface over a limited temperature range. Gultepe et al. (2001) showed that this parameterization does not represent ice particle measurements aloft made during several field projects at different locations. The uncertainty in modeling ice nucleation and the subsequent ice particles in GCMs is a major challenge for the future. Ice particles in clouds are often represented in models by simple shapes, or even spheres. However, Korolev et al. (2000) have shown that most ice particles in the atmosphere are irregular in shape. Smaller particles tend to be more spherical but they quickly become shaped as they get larger (Korolev and Isaac, 2003). The radiative properties of ice particles in GCMs often do not effectively simulate the normally found irregular shapes thus introducing some uncertainties. However, progress is being made in parameterizing	Taken into account

No.	Batch	Page:line From	Page:line To	Comment	Notes
				ice particle sizes in GCMs by a number of authors (e.g. Boudala et al., 2002). Many supercooled clouds in the atmosphere contain both ice and liquid particles (Cober et al., 2001 and Korolev et al., 2003). For reasons not fully understood, these clouds tend to be stable and long lasting. However, most GCMs cannot accurately represent such clouds, especially over a large grid square. This also represents an uncertainty in modeling clouds and cloud processes within GCMs. Boudala, F.S., G.A. Isaac, Q. Fu, and S.G. Cober, 2002: Parameterization of effective ice particle sizes for high latitude clouds. Inter. J. Climatol., 22, 1267-1284. Cober, S.G., G.A. Isaac, A.V. Korolev and J.W. Strapp, 2001: Assessing cloud-phase conditions. J. Appl. Meteor., 40, 1967-1983. Hirst E., Kaye P H., Greenaway R S., Field P., and Johnson D W., 2001: Discrimination of Micrometre-sized Ice and Super-cooled Droplets in Mixed-phase Cloud. Atmospheric Environment 35, 1, 33-47. Guan, H., S.G. Cober, G.A. Isaac, A. Tremblay and A. Methot, 2002: Comparison of three cloud forecast schemes with in-situ aircraft measurements. Wea. Forecasting, 17, 1226-1235. Gultepe, I., G. A. Isaac, and S. G. Cober, 2001: Ice crystal number concentration versus temperature. International J. of Climate, 21, 1281-1302. Korolev, A., G.A. Isaac, and J. Hallett, 2000: Ice particle habits in stratiform clouds. Q.J.R.M.S. 126, 2873-2902. Korolev, A.V., G.A. Isaac, S.G. Cober, J.W. Strapp, and J. Hallett, 2003: Observations of the microstructure of mixed phase clouds. Quart. J. Roy. Meteorol. Soc., 129, 39-65. Korolev, A.V. and G.A. Isaac, 2003: Roundness and aspect ratio of particles in ice clouds. J. Atmos. Sci., 60, 1795-1808. Korolev, A.V., and G.A.Isaac, 2005: Shattering during sampling by OAPs and HVPS. Part 1: Snow particles. J. Tech., 22, 528-542. Meyers, M.P., P. J. DeMott, W. R. Cotton, 1992: New Primary Ice-Nucleation Parameterizations in an Explicit Cloud Model. J Appl. Meteor, Vol 31, pp 708-721. [George Isaac]	
2-1622	A	41:11		Chapter 2.4.6.4 Page 41, line 11 Clouds often do not cover a complete grid square and are inhomogeneous in terms of cloud cover. Much effort has gone into addressing transport of solar radiation for inhomogeneous clouds (e.g. Barker et al.1999;Rossow et al. 2002). It has been demonstrated that realistic inhomogeneous clouds often transport solar radiation quite differently from their plane-parallel horizontally homogeneous counterparts. In addition, the effects of horizontal variations in liquid-water path (LWP) and droplet effective radius (re) on solar radiative transfer properties of stratiform clouds (Raisanen et al., 2003) need	Taken into account

No.	Batch	Page:line From	Page:line To	Comment	Notes
				to be addressed. Barker, H.W., Stephens, G. L. and Fu, Q.,1999: The sensitivity of domain-averaged solar fluxes to assumptions about cloud geometry. Q. J. R. Meteorol. Soc., 125, 2127–2152 Räisänen, P., G. A. Isaac, H. W. Barker and I. Gultepe, 2003: Solar radiative transfer for stratiform clouds with horizontal variations in liquid water path and droplet effective radius. Q. J. Roy. Meteor. Soc. 209, 2135-2149. Rossow, W. B., Delo, C. and Cairns, B. 2002: Implications of the observed mesoscale variations of clouds for the earth's radiation budget. J. Climate, 15, 557–585 [George Isaac]	
2-1623	A	41:12	41:17	True, and well said! [Jerry Mahlman]	Thanks.
2-1624	A	41:13	41:13	Please omit "somewhat": They are crude. [Caroline Leck]	Noted.
2-2734	B	41:19	41:20	"Even though …TAR": on which basis do you say that? The spread can only increase in time, not decrease. You may want to say that "the spread in magnitude from recently published studies since the TAR is substantially smaller than it was at the time of the TAR", but even such a statement is dubious. [Olivier Boucher]	Noted, the sentence refers to the reduced spread in the estimate of the albedo RF by different models with sulphate and SS as aerosol categories (Fig. 2.4.4a).
2-1625	A	41:19	41:21	But, ensembles of models, and model runs, can! [Jerry Mahlman]	Noted
2-2735	B	41:21	41:21	underestimates -> underestimated [Olivier Boucher]	Accepted
2-1626	A	41:21	41:21	underestimates -> underestimated [Reto Knutti]	Accepted
2-1627	A	41:22	41:24	Unclear sentence: "Uncertainties may be underestimates in all models… " [Robert Levy]	Accepted
2-1628	A	41:29	41:29	after the sentence beginning "Further, these models…", insert: "A fundamental problem is that GCMs do not resolve the small scales(order 100s of metres) at which aerosol-cloud interactions occur." [Graham Feingold]	Accepted
2-1629	A	41:31	41:31	after "… on a fundamental microphysical level" insert "… although some modeling studies suggest that the albedo effect is more sensitive to the size of aerosols than to composition (Feingold 2003)." [Graham Feingold]	Accepted
2-1630	A	41:35	41:44	What assumptions have been made about paticle's shape for the calculations?	Noted, models assume spherical

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Eugene Rozanov]	particles as CCN
2-1631	A	41:44	41:44	Add after the end of the last sentence: The effects of aerosol particles on heterogeneous ice formation and thereby on cloud life times, precipitation behaviour and optical properties likely are insufficiently understood and present another challenge for both observations and modelling. [Sabine Wurzler]	Taken into account; but comment included at end of subsection
2-2736	B	41:46	41:48	can you give references for these models? [Olivier Boucher]	Noted; the models presented in Fig. 2.4.4 are listed in Table 2.4.6
2-1632	A	41:46	41:54	I think this paragraph is a nice "basic" introduction to modeling, it should be written before discussion of model results [Robert Levy]	Taken into account
2-2737	B	41:49	41:49	zing? [Olivier Boucher]	Accepted typo for parameterising
2-1633	A	41:49	41:49	change to "… typically parameterising…" [Graham Feingold]	Accepted
2-1634	A	41:49	41:49	put an "i" between "parameter" and "zing" to form the word "parameterizing" [Charles Ichoku]	Accepted
2-1635	A	41:49	41:49	parameter zing -> parameterizing [Reto Knutti]	Accepted
2-1636	A	41:49	41:49	...parameterizing… [Jerry Mahlman]	Accepted
2-1637	A	41:49	41:49	Typo: replace "parameter zing" by "parameterizing" [Sabine Wurzler]	Accepted
2-1638	A	41:49	41:49	…typically parameter zing…" -> "…typically parameterizing… [Xiaobin Xu]	Accepted
2-1639	A	41:49		correct to: "parameterizing" [Hartmut Grassl]	Accepted
2-1640	A	41:50	41:51	"no model includes natural biogenic particles". That's not true. For instance, all models participating in AeroCom take into account DMS from marine biosphere or organics from vegetation -in a simple way. In transient climate simulations for instance Roeckner et al., 1999, or Takemura et al., JGR, 2005, take into account DMS emissions. [Johann Feichter]	Taken into account, it was meant natural "primary" biogenic.
2-2738	B	41:51	41:51	Some models do have a rough parametrisation of natural biogenic aerosols (condensing terpene emissions). [Olivier Boucher]	Taken into account, it was meant natural "primary" biogenic.
2-1641	A	41:51	41:51	I'm not sure what is meant by "no climate model includes natural biogenic particles".	Taken into account, it was meant

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Many do include (in a simple way) natural organic aerosol from vegetation, usually following Guenther et al (1995), so perhaps what the authors mean is that variations in these particles (e.g., due to a warmer climate) are not treated, or that primary particles are not treated explicitly. Please clarify. [Leon Rotstayn]	natural "primary" biogenic.
2-1642	A	41:51		correct: "modelled" to "model" [Hartmut Grassl]	Accepted
2-2739	B	41:53	41:53	owing to ?? [Olivier Boucher]	Accepted, typo "is" should be "its"
2-1643	A	41:53	41:53	...owing to its distinctly… [Jerry Mahlman]	Accepted
2-1644	A	41:53	41:53	Typo: Last word in this line should be "it's" not "is" [Sabine Wurzler]	Accepted
2-1645	A	41:54	41:55	Rephrase the sentence "The presence of organic carbon …" [Ramachandran Srikanthan]	Accepted
2-1646	A	42:1	42:8	I have the sense that laboratory physics is controlling some of these discussions, with uncertain extrapolation to the real atmosphere, with field observations lurking between the two, thus sustaining confusion as to what is really going on. [Jerry Mahlman]	Noted
2-2740	B	42:4	42:4	not clear. Is it really a 15% increase in the CDNC? Or a change in the width or average radius of the distribution? [Olivier Boucher]	Accepted; not CDNC but width
2-1647	A	42:5		add: "depending on size distribution shape" after "effect" [Hartmut Grassl]	Accepted
2-1648	A	42:10		Section 2.5. The title of this section is called "surface changes". To my best knowledge, the term "surface change" is new in IPCC report. Readers should be informed of the actual meaning of this concept, in terms of climate change. It would be better to give a clear definition of surface change and list the relevant items of physical properies of the (land) surface. Without the definition, I got confused because some subsections seem to have nothing to do with surface changes that I understand. For example, "anthropogenic heat release" and "effects of CO2 change on plant physiology" are quite different issues from surface change. Anthropogenic heat release inputs energy to the climate system, but it is not directly related to surface change. Effects of CO2 change on plant physiology is a response or a indirect forcing caused by CO2 emission. And the RF of black carbon in snow ice is more a indirect forcing caused by BC aerosol. If all the topics are to be included in this section, another section title should be chosen to better cover the topics.	ACCEPT We will consider how to make the section title clear

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Moreover, the readability of this section (particularly the introduction part) could be improved by beginning with the definition, itemizing the physical properties of the surface, and discussing the mechanisms that modify the properties. [Xiaobin Xu]	
2-1649	A	42:12	42:12	I found this introduction a bit long compared to other sections. [Keith Shine]	ACCEPT. The introduction will be shortened if possible.
2-1650	A	42:14	:18	The first two sentences of this subsection effectively say the same thing. I advise that text is deleted from "In addition..."on line 16 to "...surface albedo", and replaced by "e.g.". This turns following discussion of albedo change due to land use changes into an example of the general statement in the first sentence of (sub)section 2.5.1 Some further editing may be required. [Florens De Wit]	NOTED
2-1651	A	42:18	:19	Agricultural land is contrasted against natural landscape; this is misleading. There are no natural landscapes in many of the countries participating in UNFCCC and IPCC. In fact, most countries will view some agricultural and very much antropogenic landscapes as examples of natural landscapes. Change this sentence into: "The albedo of agricultural land can be very different from that of park land or forrest, which makes the choice of clearing or re-planting areas relevant for climate.". [Florens De Wit]	REJECT. The scope of the chapter is to compare present-day states with former states, and much more of the global land surface was natural in pre-agricultural and pre-industrial times.
2-1652	A	42:18		Changes in water availability can influence the surface energy budget as much as or more than changes in albedo. I discussed this in a 1984 paper in the Journal of Climate and Applied Meteorology. Please see Anthes (1984) for a discussion and demonstration. Reference could go in line 38. As discussed in Chapter 1, it is important to reference scientists for their earlier contributions to a topic. [Richard Anthes]	NOTED. This topic is discussed later in this section and in chapter 7.
2-1653	A	42:18		Anthes, R.A., 1984: Enhancement of Convective Precipitation by Mesoscale Variations in Vegetative Covering in Semiarid Regions. J. Climate and Applied Meteorology, 23, 541-554. [Richard Anthes]	NOTED.
2-1654	A	42:19	42:20	Albedo of forested land is also "greener!" and the spectral differences are important! [Robert Levy]	NOTED. Spectral differences were assessed as less important than broadband albedo changes but consideration will be given to discussion of this if space allows.
2-1655	A	42:22	42:22	Changes in surface albedo induce… (remove "change")43	ACCEPT

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Joyce Penner]	
2-1656	A	42:25	42:27	the process the leads to a decrease in albedo for snow covered forests versus snow covered bare ground is shadowing rather than multiple reflections. Technically, trees increase the interaction cross section with absorbers (vegetation elements) and hence decrease albedo. I am not certain that Harding and Pomeroy or others have been able to demonstrate that the multiple reflection between snow on trees or on the ground and the absorbers in the trees is the cause of the decrease in albedo. Simply put, less photon reach the bright snow due to shading and those that do reach the snow are not "seen" by the sky due to shading. [Richard Fernandes]	REJECT The sentence in question only refers to the albedo remaining low when snow cover is large, not to the process as a whole
2-1658	A	42:38	42:41	I did not see mention to the impact of land cover change on leaf area index and hence on canopy conductance. Perhaps this is what is meant by "the flux of moisture through evaporation". But, conventionally evaporation does not include transpiration. It also makes sense to mention canopy conductance since this sentence already brings up other physical quantities like emissivity [Richard Fernandes]	ACCEPT. "Evaporation" was indeed intended to include transpiration, but this text will be revised to clarify this.
2-1660	A	42:38	42:51	Non-radiative forcing is a bit of a stretch - probably better to be left out. Many of these non-radiative forcings probably qualify as physical processes that go un-modeled in current GCMs, but if they somehow were to be modeled, they would then be a part of some feedback process. Nuclear energy, radio-active decay, and tidal friction produce some extra energy, but they are too miniscule to be of concern. Human modifications of the global surface albedo are included as part of "RF due to anthropogenic surface albedo change", while the effects of buildings on surface roughness specification typically go ignored. Basically, unless the effect is significant enough to get translated into some tangible number of W/m2, the effect is justifiably ignored. [Andrew Lacis]	REJECT. This chapter deals with all drivers of climate change. Feedbacks occur as a respone to a surface temperature change and the processes discussed here do not fall into this category and are considered to be a forcings but are not a radiative forcings.
2-1661	A	42:38	42:51	It may be helpful to reference chapter 7 here, where these effects are discussed in detail. [Susan Solomon]	ACCEPT
2-2741	B	42:54	42:55	Delete "This was …TAR" as it is repeated in the next sentence. On line 56 can you change "effect" into "non-radiative forcing" as you've just said above that ythis report adopts the same term as Jacob et al (2005). [Olivier Boucher]	ACCEPT
2-1662	A	42:54	42:55	delete: "and was not discussed in the TAR," [Hartmut Grassl]	ACCEPT
2-1663	A	42:57	43:1	The section on land-use change does not do justice to the topic, either in terms of stating what is available in the literature, nor explaining the difficulties in researching it, nor in making clear the potential for quantification of the land-use effect to change the perceived	REJECT. Further discussion of land use effects is in Chapter 7. Attribution and adjustment of temperature records

No.	Batch	Page:line From	Page:line To	Comment	Notes
				role of GHG's as presented elsewhere in the report. A fuller statement of the uncertainties is needed to properly convey the emerging importance of this topic. I suggest that after line 57 be added: "A difficulty in quantifying the overall effects of land-cover changes is that, once land-use modification has occurred the unperturbed state is no longer observable, so the temperature increment cannot be directly measured. This substantially complicates the task of identifying climate changes potentially attributable to radiative forcing agents. Estimated adjustments are made to the surface climate data to try and remove the effects of land use modification (e.g. Peterson 2003) and other nonclimatic biases, but evidence has emerged since the TAR that these are likely inadequate (McKitrick and Michaels 2004) and that an overall warming bias may remain in global temperature anomaly records." [Ross McKitrick]	is outside the scope of Chapter 2.
2-1664	A	42:57	43:1	References cited in above cell: Peterson, T.C. (2003). "Assessment of Urban Versus Rural in situ Surface Temperatures in the Contiguous United States: No Difference Found." Journal of Climate 16(18) 2941—2959.; McKitrick, R and P. J. Michaels (2004). "A Test of Corrections for Extraneous Signals in Gridded Surface Temperature Data" Climate Research 26(2) pp. 159-173. "Erratum," Climate Research 27(3) 265—268. [Ross McKitrick]	REJECT see response to 2-1663
2-1665	A	42:57	43:1	Additional Note on McKitrick and Michaels (2004): This paper was the subject of much controversy, mostly unpublished. I wish to enter some comments into the IPCC review record to counter what was largely unfair commentary presented in such a way as to preclude response. The paper's results are robust and its conclusions are highly pertinent to AR4 deliberations. A published comment by Benestad (Climate Research 2004 27:171-173) argued against this on the grounds that the SH data and a subset of explanatory variables failed to predict the NH dependent variables; the reply by McKitrick and Michaels pointed out that this was an ill-posed test, and the cross-validation exercize in the paper itself (in which the North and South American data were withheld and skillfull predicted) is more appropriate; also Benestad acknowlegded attempting a number of respecifications and found they yielded "similar, although not identical, model coefficients, t-values, and R2 scores to those reported by McKitrick & Michaels, indicating that the analysis captures similar relationships." An unpublished commentary on the internet identified a minor coding error in which latitude data was used in degrees while a cosine calculation assumed they were in radians. This error was corrected and new results promptly published (CR Vol 27(3)) showing only minor effects on the coefficients and standard errors and the upholding of the original conclusions of the paper. Additional, unpublished internet commentary suggested that the standard errors were mis-estimated because of clustering effects in the data. This primarily reflected the failure of the commentator to understand the estimator used in the original paper, but	NOTED, although this comment is outside the scope of this chapter. Adjustment of temperature records is in Chapter 3, attribtion is in Chapter 9.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				additional code presenting replication of the results applying an exact clustering adjustment was made available at the paper's SI (http://www.uoguelph.ca/~rmckitri/research/gdptemp.html). [Ross McKitrick]	
2-1666	A	43:2	43:17	How is albedo measured? How important that these are accurately measured or understood for retrievals of other parameters (e.g. aerosol?) [Robert Levy]	NOTED – this may be better in Ch7 so this will be discussed with Ch7 authors
2-1667	A	43:2		No mention of angular dependence of surface "reflectance" [Robert Levy]	NOTED – this may be better in Ch7 so this will be discussed with Ch7 authors
2-1668	A	43:4	43:16	12 million km2 (0.09%), further down 49 million km2 (37% of global land): at least one of those four numbers must be wrong, the factor between the two km2 values and the two percent values must be the same. [Reto Knutti]	ACCEPT. This will be corrected.
2-1669	A	43:4	43:4	The figure 0.096% should read 9.6% [Piers Maclaren]	ACCEPT
2-1670	A	43:9		a full stop is missing after "2.5.1)"; correct: "U.S." to "USA" [Hartmut Grassl]	ACCEPT
2-1671	A	43:11	43:16	MINOR COMMENT: In line 11, it is reported that "croplands areas have decreased in China.." and in line 15/16, the text states "China had a steady expansion of croplands throughout most of the last three centuries". Need for clarification. [Malte Meinshausen]	ACCEPT. This will be clarified.
2-1672	A	43:11		correct: "croplands" to "cropland" [Hartmut Grassl]	ACCEPT
2-2742	B	43:23	43:23	can we be more quantitative here? Is the rate of deforestation (in km2 per year) constant or increasing with time? For instance Brazil is arguing that it has stabilised or even decreased last year. [Olivier Boucher]	ACCEPT
2-1673	A	43:25	43:57	How does one infer the likelihood of a positive RF, given that land-use changes typically result in an increased albedo? [Jerry Mahlman]	ACCEPT Assuming this refers to the +ve forcing (0.24 WM-2) in line 43-33, this was a typo which has been corrected.
2-1674	A	43:25	45:2	The section does a good job of defining the reasons for uncertainty in estimates of RF due to anthropogenic surface albedo change, but then comes up with a surprisingly small estimate of that uncertainty. Others come up with much larger estimates. For example, Charlson, Valero and Seinfeld conclude: "To date, the results from different measurements and modeling approaches are inconsistent among themselves and with each	ACCEPT

No.	Batch	Page:line From	Page:line To	Comment	Notes
				other. The magnitudes of the inconsistencies exhibited by both measurements and models of albedo changes and effects are as large as, or larger than, the entire entire enhanced greenhouse gas effect when compared in terms of the albedo change equivalent of climate forcing. In fact, albedo change that the the equivalent of the enhanced greenhouse effect is barely detectable by the available methods for measuring albedo." (Charleson, R.J., F.P.J. Valero and J.H. Seinfeld (2005): In search of balance. Science, 308, 806-7). The section needs to explain why it comes to such a different conclusion from their assessment. [Lenny Bernstein]	
2-1675	A	43:25	45:2	(Charleson, R.J., F.P.J. Valero and J.H. Seinfeld (2005): In search of balance. Science, 308, 806-7) finds that "To date, the results from different measurements and modeling approaches are inconsistent among themselves and with each other. The magnitudes of the inconsistencies exhibited by both measurements and models of albedo changes and effects are as large as, or larger than, the entire entire enhanced greenhouse gas effect when compared in terms of the albedo change equivalent of climate forcing. In fact, albedo change that the the equivalent of the enhanced greenhouse effect is barely detectable by the available methods for measuring albedo." -- this should be reflected in this section. [Howard Feldman]	ACCEPT
2-1676	A	43:25	45:2	The section presents a significant list of reasons for uncertainty in estimates of radiative forcing from anthropogenic surface albedo change, but then comes up with a relatively small estimate of that uncertainty. This is surprising, given some of the recent literauture on this issue. Charlson, R.J., F.P.J. Valero, and J.H. Seinfeld (2005): In search of balance. Science, 308, 806-7, conclude: "To date, the results from different measurements and modeling approaches are inconsistent among themselves and with each other. The magnitudes of the inconsistencies exhibited by both measurements and models of albedo changes and effects are as large as, or larger than, the entire entire enhanced greenhouse gas effect when compared in terms of the albedo change equivalent of climate forcing. In fact, albedo change that the the equivalent of the enhanced greenhouse effect is barely detectable by the available methods for measuring albedo." In light of literature indicating the significance of, and uncertainty in, albedo estimates, the authors need to present a better justification of their low estimate for uncertainty. [Jeffrey Kueter]	ACCEPT
2-1677	A	43:32	43:32	Some numbers seem to be missing between "…simulated Rfs of" and "and -0.18". [Xiaobin Xu]	ACCEPT
2-1678	A	43:32		delete: "and" before "0.18" at the end of the line [Hartmut Grassl]	ACCEPT
2-1679	A	43:32		delete and	ACCEPT

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Junying Sun]	
2-2743	B	43:33	43:33	Isn't there a minus sign missing here?? [Olivier Boucher]	ACCEPT
2-1680	A	43:33	43:44	PNV used several times before defined. [Robert E. Dickinson]	REJECT – PNV defined in section 2.5.1
2-1681	A	43:33	43:33	Spell out Potential Natural Vegetation [Fortunat Joos]	REJECT – PNV defined in section 2.5.1
2-1682	A	43:33		add: "-" before "W m" [Hartmut Grassl]	ACCEPT
2-1683	A	43:37		One citation for MODIS albedo is Schaaf et al., First operational BRDF, albedo and nadir reflectance products from MODIS, Remote Sens. Environ., 83, 135-148, 2002, or, more recently, Gao et al., Variability of MODIS albedo for major global vegetation types, J. Geophys Res., 2005 (but I admit I am a coauthor on these; the paper now cited is only about MODIS land cover) [Wolfgang Lucht]	NOTED
2-1684	A	43:38		0.75 W m-2 [Junying Sun]	REJECT. The number is the ratio of (RF relative to 1750) to (RF relative to PNV).
2-2744	B	43:48	43:50	I thought Gunnar looked at that for BB regions and concluded the non-linear contribution to the effect was very small. [Olivier Boucher]	ACCEPT. The value of this sentence will be considered.
2-1685	A	43:48	43:48	add "the " at end of line [Joyce Penner]	ACCEPT
2-1686	A	43:48		add: "the" after "took place in" [Hartmut Grassl]	ACCEPT
2-1687	A	43:55		correct: "change" to "changes" [Hartmut Grassl]	ACCEPT Although re-wording of this sentence has removed this anyway
2-1688	A	44:1	44:2	This conclusion seems to be inconsistent with the previous section that argues for a net negative RF due to land use changes? [Jerry Mahlman]	REJECT Assuming this refers to lines 45.1-45.2, the slight possibility of a positive forcing was discussed in the previous section.
2-1689	A	44:4		correct: "bases" to "basis" [Hartmut Grassl]	ACCEPT
2-1690	A	44:5	44:8	To be complete the Global Land Cover 2000 product from the Joint Research Centre of the EU should be cited. [Richard Fernandes]	NOTED

No.	Batch	Page:line From	To	Comment	Notes
2-1692	A	44:14		add: "-" before "0.29 W m" (?) [Hartmut Grassl]	ACCEPT
2-1693	A	44:28	44:44	How does one infer the likelihood of a positive RF, given that land-use changes typically result in an increased albedo? [Jerry Mahlman]	As stated in lines 44.38-44.39, possible positive RFs result from particular choices of albedo for particular land cover types.
2-1694	A	44:29	44:31	Some comment may be appropriate regarding the use of AVHRR and MODIS for albedo estimation. Work by Pinty et al. suggest these instruments may give differences as they are single view angle sensors and that instruments such as MISR are more appropriate. Some reference to this work on intercomparison of different sensors for albedo measurements may be appropriate. Pinty, B., M. Taberner, S. Liang, Y. Govaerts, J. V. Martonchik, A. Lattanzio, C. B. Schaaf, M. M. Verstraete, R. E. Dickinson, N. Gobron and Jean-Luc Widlowski (2005) 'MODIS/Meteosat/MISR Surface Albedo Comparison Results', 9th International Symposium on Physical Measurements and Signature in Remote Sensing (ISPMSRS), Beijing, China, 17-19 October 2005. [Richard Fernandes]	NOTED
2-1696	A	44:29	44:29	insert "to" after "subject" to read: "subject to a number of uncertainties". [Charles Ichoku]	ACCEPT Although re-wording of this sentence has removed this anyway
2-1697	A	44:29	44:29	change "subject a" to "subject to a" [Joyce Penner]	ACCEPT Although re-wording of this sentence has removed this anyway
2-1698	A	44:29	44:29	…radiation processes are subject a number of uncertainties." should read "… radiation processes are subject to a number of uncertainties [Felicita Russo]	ACCEPT Although re-wording of this sentence has removed this anyway
2-1699	A	44:29	44:29	…are subject a number of uncertainties." -> "…are subject to a number of uncertainties. [Xiaobin Xu]	ACCEPT Although re-wording of this sentence has removed this anyway
2-1700	A	44:29		add: "to" after "subject" [Hartmut Grassl]	ACCEPT Although re-wording of this sentence has removed this anyway
2-1701	A	44:43		delete the first bracket before "Matthews" and add a bracket before "2003" [Hartmut Grassl]	ACCEPT
2-1702	A	45:4	45:23	This section is backed up with observations of BC in snow and ice at various locations from which an etsimate of radiative forcing is made. I appreaciate the uncertainy statement related to the extererenal/internal mixture issue but have some doubt on the extrapolation from the observation followed by modell estimated on changes in surface albedo etc to be applid to all ice/snow covered surfaces. Based on my own experience over the ice and snow covered Arctic pack ice in summer our meteorological measurements showed usually a well-mixed bounadry layer from the surface to about	NOTED

No.	Batch	Page:line From	Page:line To	Comment	Notes
				200m, where a temperature inversion, with the highest temperatures well above 0 C around 1km, separated air gaining its properties from the surface (only natural aerosol sources) from air influenced by far more distant conditions outside the pack ice region. This vertical structure facilitates mixing of properties from the surface through the boundary layer, while at the same time it inhibits exchange with the free troposphere. Vertical profiles of concentrations of the long-lived gases acetone and acetonitrile that are primarily of continental man-made origin confirmed this finding. Concentrations were always much higher above the inversion than below it. Unless there are unknown rapid surface sinks for these gases in the Arctic, the implication is that either for gases or particles including BC, there will be little interchange of air from above the inversion to below it , that is, any decrease of surafce albedo of snow and snow melt is not be expected. The value of section 2.5.4 is questioned. [Caroline Leck]	
2-1703	A	45:4		correct: "Snow Ice" to "Snow and Ice" [Hartmut Grassl]	ACCEPT
2-1704	A	45:6	45:23	I am a bit confused here. When I look at "clear-sky" snow fields in a polluted environment, I see snow cover that accumulates black carbon, but the carbon particles tend to heat up, and sink into the snow cover, thus acting to damp the tendency for the particles to heat up over the duration of the snow event. [Jerry Mahlman]	NOTED
2-1705	A	45:10	45:13	This entire sentence "The uncertainty in this estimate …" is not properly worded and needs rewording. It is confusing. [Charles Ichoku]	ACCEPT this will be re-worded
2-2745	B	45:16	45:16	Are you talking surface or TOA albedo? Presumably surface albedo as 0.4% of the global planetary albedo would be a very large effect. Can you clarify? [Olivier Boucher]	ACCEPT this will be clarified
2-1706	A	45:21	45:21	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	ACCEPT
2-1707	A	45:21		correct the formula "CO2" [Hartmut Grassl]	ACCEPT
2-1708	A	45:22	45:22	Not clear why you favour the Hansen estimate over others [Keith Shine]	ACCEPT the reasons for the choice of central estimate will be clarified
2-1709	A	45:27	45:45	It is obvious that land-use change and land-cover change can exert significant direct effects on the local/regional climate. But, the real question is: How big are the regions that are affected, and what, if any, of the obvious, but differing, regional land-surface/land-cover changes integrate globally to produce a quantitatively relevant contribution to RF, the bottom line for this Chapter's analysis? Also, these local/regional	ACCEPT The size of the regions affected will be quantified. The scope of the chapter also includes non-radiative forcings and this section will be clarified to

No.	Batch	Page:line From	Page:line To	Comment	Notes
				numbers seem to be big enough to influence the mega-city scale heating on a year-around basis. [Jerry Mahlman]	distinguish these from RF.
2-1710	A	45:34	45:34	The meaning of "…affecting on regional climates…" is not clear. That sentence needs rewording. [Charles Ichoku]	ACCEPT "on" has been removed
2-1711	A	45:34		delete: "on" after "affecting" [Hartmut Grassl]	ACCEPT
2-1712	A	45:35		a full stop is missing at the end of the line [Hartmut Grassl]	ACCEPT
2-1713	A	45:37		add: "change" after "cover" [Hartmut Grassl]	ACCEPT
2-1714	A	45:41		delete "B" before "Govindasamy" [Hartmut Grassl]	ACCEPT
2-1715	A	45:50	45:50	insert "as the" before "…heating of" to read "…such as the heating of" [Charles Ichoku]	ACCEPT
2-1716	A	45:50		correct: "production" to "use"; and insert "as" after "such" [Hartmut Grassl]	ACCEPT
2-1717	A	45:57		Add "which will be reflected in local surface temperature measurements" [Vincent Gray]	REJECT Unnecessary as this is obvious from line 45.56
2-1718	A	46:0		Section 2.6 I agree that contrail and aircraft induced cloudiness should be included. However, has it been considered to include ship tracks? I would have thought these have a considerable impact, even if they don't induce the same cloudiness since they are formed as lower altitude clouds. They are obvious on satellite images after all. [Eleanor Highwood]	Rejected. Only aviation in scope
2-1719	A	46:1	46:21	The relevance of plant physiology to a review of radiative forcing is not clear - material perhaps better moved to chapter 7 and whatever needed here summarized [Robert E. Dickinson]	REJECT This process is considered to be a forcing (non-radiative), and hence within the scope of this chapter, because it involves an effect of CO2 (it is not a feedback following surface temperature change). But material on the process should be included in chapter 7.
2-1720	A	46:1	46:21	This appears to be an interesting and likely quantitatively significant phenomenon. Since we are still on track to doubled atmospheric CO2 over pre-industrial levels, this could	NOTED

No.	Batch	Page:line From	Page:line To	Comment	Notes
				become a growingly important phenomenon. [Jerry Mahlman]	
2-1721	A	46:11	46:11	…has been made. (Sellers et al., 1996) propose…" -> "…has been made. Sellers et al. (1996) propose… [Xiaobin Xu]	ACCEPT
2-1722	A	46:17	46:18	delete: "so a pertubation … mechanism" [Hartmut Grassl]	REJECT We consider it reasonable to suggest that if a change in the surface moisture budget has been detected, an effect on surface temperature may have occurred.
2-1723	A	46:17	46:18	delete: "so a pertubation … mechanism" [Hartmut Grassl]	REJECT We consider it reasonable to suggest that if a change in the surface moisture budget has been detected, an effect on surface temperature may have occurred.
2-1724	A	46:23	46:28	I think that the opening sentence reveals an incomplete understanding of aircraft contrails, and their effect on RF. First, the statement that their existence depends upon ambient temperature and humidity seems to be conceptually incomplete. Clearly, a contrail in the right temperature and humidity regime does indeed form, but lacking the critical third variable (slow, large-scale (cooling) vertical velocity, the contrail dissipates rather rapidly. Except near mega-airports, the inference of a small (of order 0.1W/M2) contribution to global RF appears to be quite robust. [Jerry Mahlman]	Rejected. The role of large scale cooling is implicit in the formation of ice supersaturated air masses in which persistent contrails form.
2-1725	A	46:23	48:12	It would be more sensible to move this section to Aerosols following the 2.4.6 Cloud-Aerosol Interaction. [Andrew Lacis]	Noted
2-1726	A	46:23	48:11	Section 2.6 Owing to lack of time, I have focussed in on this section as an area of main interest. Overall, I would say that this section is a very good synthesis of the available work and is complete in its review and referencing. A few minor comments follow: [David Lee]	Noted. Thank you.
2-1727	A	46:23		Section 2. 6 The discussion of contrails does not address the critical issue in understanding or modeling contrail effects. Persistent contrails occur in regions of the atmosphere which are supersaturated with respect to ice. The processes which produce and control such supersaturated regions are not well understood nor are they modeled well. In particular, the roles of small scale vertical motion and the identification/role of ice nuclei on production of both natural and anthropogenic cirrus clouds are not well understood. Relative humidity observations and calculations are very sensitive to	Noted. The uncertainties are significant in contrail evaluations as is noted in the text and in cited references.

No.	Batch	Page:line From	To	Comment	Notes
				temperature. Small uncertainties in temperature (e.g., 1 degree) have large effects (14%) on RHi. Global observational data on RHI is limited. Few (if any) models incorporate the detailed microphysics or sub-grid scale processes necessary to address these processes. Most GCM's have a dry bias in the upper troposphere. [Steven Baughcum]	
2-1728	A	46:23		Section 2.6 These uncertainties in RHi then introduce significant uncertainties in our ability to predict contrail coverage and the optical properties of contrails (or contrail/cirrus) since the ice water content and mean sizes will depend strongly on the ambient temperature and humidity.This should be discussed in section 2.6 [Steven Baughcum]	Rejected. Discussing detailed dependencies in contrail formation is beyond the scope of this chapter.
2-1729	A	46:23		Section 2.6 does not offer any definitive conclusions to policy makers. Some researchers have suggested re-designing the air space system to mitigate contrails. Unfortunately, this comes at the price of increased fuel use (hence more CO2). Does the IPCC feel that the uncertainties surrounding the impact of contrails versus that of CO2 are sufficiently resolved to enable action? Or, is further research needed. Though this is clearly a difficult question, it is important to policy makers that the IPCC attempt to answer it. [Lourdes Maurice]	Rejected. Mitigation options and future projections are beyond the scope of this chapter.
2-1730	A	46:23		The Gleneagles Plan of Action calls for working with the IPCC to provide, as part of its forthcoming Fourth Assessment Report, an up-to-date assessment of the latest evidence on aviation's impacts on the climate. Section 2.6 is a very cursory treatment of aviation and only a very limited handful of aviation experts were involved in the assessment. Suggest making some comment as to whether the IPCC believes this section answers the Gleneagles action, or if more detailed work is needed. [Lourdes Maurice]	Rejected. Aviation is not evaluated as a separate sector in this report or chapter and hence the Gleneagles request can not be addressed here.
2-1731	A	46:27	46:27	There is no need to write RF (RF). Page 7, line 4 indicates that hereafter RF is radiative forcing. [Lourdes Maurice]	Accepted.
2-1732	A	46:27		delete: "(RF) [Hartmut Grassl]	Accepted.
2-1733	A	46:34	46:36	Section 2.6.1 There is a modelling study examining the effect of changes in humidty and temperature distributions, which is already references (Marquart et al., 2003 - j. Clim.) [David Lee]	Noted.
2-1734	A	46:35	46:35	write more accurately: ''in the upper troposphere" (instead of free troposphere) [Mikhail Danilin]	Accepted.
2-1735	A	46:36	26:37	The phrase "Aviation aerosol also can potentially alter the properties of clouds that form later in air containing aircraft emissions." contradicts the earlier statement that effects of aviation	Rejected. We discuss here only the role of aviation aerosol on clouds.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				emissions that are not specific to just aviation (such as aerosols) are discussed in other sections. [Lourdes Maurice]	
2-1736	A	46:36	46:36	The term "aviation cloudiness" is not very clear. Suggest using "aviation-induced cloudiness" as is done subsequently in the report. [Lourdes Maurice]	Accepted.
2-1737	A	46:42	46:42	Insert the following sentence after ''…suitable." : Contrails persist only under ice supersaturated conditions and only long-lived contrails affect climate. [Mikhail Danilin]	Accepted. Sentence changed.
2-1738	A	46:43	46:44	Cut the current sentence after (Hartmann et al., 1992) and add the following sentence: Contrail TOA net RF has a strong diurnal dependence and typically is positive (Meerkoetter et al., 1999). [Mikhail Danilin]	Accepted. Reference added.
2-1739	A	46:45	46:45	write ''ECMWF reanalysis" instead of ''atmospheric data" [Mikhail Danilin]	Accepted. Add meteorological descriptor.
2-1740	A	46:46	46:48	"assumed to vary linearly with fuel use" . This assumption is hard to justify and introduces a signficant uncertainty in the calculations. Persistent contrails occur where Rhi > 100% which means that at many (most?) temperatures, most of the ice in the contrail will be from the ambient atmosphere. Aircraft emitted water is probably a small contribution to the total ice content of those contrails which contribute signficantly to radiative forcing. [Steven Baughcum]	Accepted. Clarified that aircraft flight regions stay unaltered.
2-1741	A	46:46	46:46	Rewrite the sentence as follows: The associated contrail RF is derived from choosing an optical depth for contrails and knowing their coverage. However, all existing global contrail RF calculations ignore effects of ice particle shape, which are important (e.g., Zhang et al., 1999) [Mikhail Danilin]	Rejected. Sentence is clear as written. Ice particle shape is but one of several aspects influencing optical depth.
2-1742	A	46:46	46:47	rewrite the sentence as follows: ''The global contral RF values are assumed to vary linearly with aicraft fuel use or distance flown. However, this assumption treats all aircraft equal for contrail production, which is false according to the contrail calculations in a plume (Sussman and Gierens, 2001; Lewellen and Lewellen, 2001)." [Mikhail Danilin]	Rejected. Sentence correctly describes current model results.
2-1743	A	46:46	46:48	Add the following sentence after ''… Ponater et al. (2002)": Global contrail coverage calculations are still quite uncertain because of our current poor knowledge of relative humidity distribution in the UT, simplified representaion of the sub-grid scale processes, and the normalization procedure apllied homogeneously over the globe using regional satellite observations of contrails (Bakan et al., 1994; Meyer et al., 2002).	Rejected. Uncertainty statement not needed here.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Mikhail Danilin]	
2-1744	A	46:46	46:48	It is unclear what level of certainty can be assigned to the statement when one assumes that flight tracks remain unchanged. It is unclear to what degree the variability in flight tracks have an influence over the underlying scientific understanding. [Lourdes Maurice]	Noted.
2-1745	A	46:51	46:51	Is it worth to discuss forcings with a magnitude of 0.006 W/m2. Response of such a low forcings is most likely not distinguishable from noise. [Johann Feichter]	Noted.
2-1746	A	46:51	46:51	The paper by Fichter et al in Met Z (14:563-572) comes up with a much lower value (2 -3 mWm-2). [Keith Shine]	Noted. The Fichter result is for 1992 aircraft operators.
2-1747	A	46:52	46:57	The estimates of RF denoted as best estimates use the lower values of optical depth computed by Ponater et al (theoretical) and Meyer (over Europe only). The theoretical results do not use the supersaturations typically observed in the upper troposphere that lead to persistent contraisl and hence are unlikely to yield a realistic average everywhere. The Meyer results may be good for Europe, but recent, careful analyses determined from data over the US (Minnis et al., 2005; Palikonda et al., 2005) yield average AIC optical depths that more than twice their counterparts over Europe as estimated by Meyer et al. Minnis, P., R. Palikonda, B. J. Walter, J. K. Ayers, and H. Mannstein, 2005: Contrail properties over the eastern North Pacific from AVHRR data. Meteorol. Z., ., 14, 525-536. Palikonda, R., P. Minnis, D. P. Duda, and H. Mannstein, 2005: Contrail coverage derived from 2001 AVHRR data over the continental United States of America and surrounding areas. Meteorol. Z., 14, 537-548. [Patrick Minnis]	Accepted. Comment added.
2-1748	A	46:54	46:54	Write ‘‘overlap with ambient clouds and diurnal variability of air traffic" instead of ‘‘interaction with other clouds", which is too general and misleading, since interaction means contrail indirect effect, which is not discussed here. [Mikhail Danilin]	Rejected. Statement is sufficiently clear and is referenced.
2-1749	A	47:0		Section 2.6.4 The discussion of aviation aerosols needs to first describe what is known about the composition of ice nuclei in the upper troposphere to provide context to the aviation aerosol issue. The composition of ice nuclei and the effect of surface coatings are not well understood, particularly in terms of the roles of soot and sulfate aerosols. This would provide context to how aviation aerosols might impact cirrus and the uncertainties. Archuleta and co-workers (ACP, 5, 2617, 2005) discuss ice nucleation by mineral particles (which appear to be the best IN) and discuss the role of particle size (small particles are most sensitive to coatings). [Steven Baughcum]	Rejected. Adding this detail is not warranted here.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2746	B	47:4		I suggest you look and refer to the good study by Stubenrauch and Schumann (Impact of air traffic on cirrus coverage, GRL, 32, 2005). [Olivier Boucher]	Accepted.
2-2747	B	47:12	47:15	IPCC - 1999 --> IPCC (1999) [Olivier Boucher]	Rejected. This is standard format for IPCC text.
2-1750	A	47:16	47:18	The statement that "two studies have confirmed positive trends" seems to be overstated. The Stordahl paper (cited as one of the two studies) in it's conclusion says "the relationship between cirrus cloud amount and aircraft density is uncertain and we cannot draw firm conclusions or quantify the effect with high certainty. ... Our results may still be influenced by natural variability, climate change, and other anthropogenic impacts". [Steven Baughcum]	Noted. This uncertainty is noted already in the text.
2-1751	A	47:16	47:21	I think that the statement about significant positive trends in cirrus cloudiness in regions of high air traffic (Zerefos et al., 2003; Stordal et al., 2005) is too strong. Stordal et al. (2005) put a question mark in their title and wrote in their conclusions (2nd sentence): ''However, the relationship between cirrus cloud amount and aircraft density is uncertain and we can not draw firm conclusions or quantify the effect with high certainty." Also, both studies show a positive statistically significant trend only in some seasons and some regions of high air traffic. For example, Zerefos et al (2003) saw these trends only in summer over North Atlantic and winter over North America. [Mikhail Danilin]	Noted. This uncertainty is noted already in the text.
2-1752	A	47:16	47:21	I suggest the following revision of these sentences: ''Since IPCC-99, several studies tried to link possible trends in cirrus clouds with air traffic (Zerefos et al., 2003; Stordal et al., 2005; Stubenrauch and Schumann,2005). [Mikhail Danilin]	Rejected. Not needed.
2-1753	A	47:16	47:21	Zerefos et al. (2003) found statistically significant positive trends in cirrus clouds (>2%/decade) only over North Atlantic in summer and North America in winter. Stordal et al. (2005) also found positive trends in cirrus clouds over Europe, North Atlantic, and East coast of USA. However the correlation between air traffic and cirrus trend was high only for some regions with high air traffic (Europe, North Atlantic). Stubenrauch and Schumann (2005) analyzed TOVS Path-B satellite data during the 1987-1995 period and did not found a statistically significant trend in cirrus cloudiness over regions of high air traffic. However, a possible trend in cirrus cloudiness emerged when only contrail-favorable conditions were considered. These studies indicate possible links between air traffic and cirrus trends. However, more studies are necessary with longer and better cirrus data sets and proper accounting for natural variability of cirrus-related parameters (like ice nuclei and RHI near the tropopause). " [Mikhail Danilin]	Noted. Qualifiers added.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1754	A	47:16	47:21	The referenced papers both make a point that cirrus changes may also have contributions due to anthropogenic impacts other than aviation. That caveat should be explicitly included here as well. The correlations are supportive of the aviation-induced cirrus effect playing a role in increases in cirrus, but the correlations include significant uncertainty and other anthropogenic effects (notably lofted particles from ground sources) should be mentioned here. The ground level emission of particles by non-aviation sources is orders of magnitude greater than the aviation source at altitude, so even if a small fraction is lofted to aviation cruise altitudes, they could rival the contribution from aviation. Thus, the relative impact of aviation and other anthropogenic sources on clouds continues to be hard to quantify. Suggest adding to the end of this paragraph a sentence related to comments in the Stordal reference: "These trends still have significant uncertainties and may be influenced by anthropogenic impacts other than aviation." [Lourdes Maurice]	Accepted.
2-1755	A	47:18	47:21	Do the developers of the ISCCP database or the cirrus cloud research community believe that a trend of 1-2%/year is statistically significant when uncertainties in the data and algorithms are considered as well as natural variability? (I'm not an expert on this but this is an important question for the chapter authors to consider) [Steven Baughcum]	Noted. The uncertainties have precluded a best estimate as noted in the text.
2-1756	A	47:20	47:21	Does this sentence mean that cirrus trends from natural vaiability could not be separately quantified? [Joyce Penner]	Noted. Yes
2-1757	A	47:21	47:21	This paragragh left out the important trend findings of Minnis et al. (2004) who showed that only over the USA could the trends in cirrus cover over a 25-year period (the longerst contrail-cirrus trend study period) be attrributed to contrails exclusivley. They used bothe surface and ISCCP cloud data and upper tropospheric humidity to isolate the contrail effect. They found that over the 25-year period 1971-95, cirrus cover oincreased only over the North Atlantic and Pacific and the USA. WHile the trends over the northern oceans are likely to be due in part to aircraft, the UTH was also increasing at the same time. The cirrus cover over other areas with heavy air traffic decreased or was invariant apparently due to decreasesin UTH. The increase in AIC over Europe appears to have staved off a decrease in cirrus that should have occurred because of dramatic drops in UTH. [Patrick Minnis]	Accepted.
2-1758	A	47:23	47:24	Section 2.6.3 As an author of the Stordal et al paper, I would prefer to see the range also given, i.e. 10 to 80 mW/m2 [David Lee]	Accepted.
2-1759	A	47:25	47:27	I do not agree with the statement "good agreement" The Minnis et al estimate was an upper bound of 26 mW/m2, whereas Stordal estimated an upper bound of 80 mW/m2	Accepted. Restated that the mean is in good agreement with the maximum

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[David Lee]	upper bound by Minnis et al.
2-2748	B	47:26	47:26	AIC RF estimate" rather than "AIC estimate [Olivier Boucher]	Accepted.
2-1760	A	47:28	47:28	The term "aviation cloudiness" is not very clear. Suggest using "aviation-induced cloudiness" as is done elsewhere in the report. [Lourdes Maurice]	Accepted.
2-1761	A	47:31	47:36	This section should be rewritten to better reflect the analyses of Hansen, Ponater, and Shine with regard to the Minnis 2004 paper. All three analyses suggest explanations for disparity. The authors should also review and cite the reply by Minnis (J. Climate, 18, 2783 (2005)) [Steven Baughcum]	Noted.
2-1762	A	47:31	47:48	This analysis appears to be wrong, although it needs to be checked out more thoroughly through an independent analysis. [Jerry Mahlman]	Noted.
2-1763	A	47:31	47:31	Suggest using "aviation-induced cloudiness" rather than "aviation cloudiness." [Lourdes Maurice]	Accepted.
2-1764	A	47:31	47:36	Remove lines 31-36. The cited data appears to lack agreement with other data presented in the report. If, as suggested, the entire surface temperature response from 1973 to 1994 can be attributed to aviation-induced cloudiness, then one could deduce that anthropogenic CO2 emissions and other greenhouse gases during that time period had zero impact on climate change. Following the Minnis et al. reasoning, a fundamental conclusion could be that the error bar associated with contrails has been reduced to "fairly certain" or "certain", and the RF's for all other greenhouse gasses have been greatly reduced. This is clearly wrong and not supported by discussion in the rest of the report. While removing a minority opinion is not generally advisable, in this instance it is justified on the basis that data in question is incongruous with the overwhelming consensus appearing in the report. Statements like this also have the propensity to divert research and commercialization efforts from addressing other emissions that may have a greater impact on climate change than contrails. [Lourdes Maurice]	Accepted. Section revised.
2-1765	A	47:31	47:36	If lines 31-36 are not removed as suggested above, need to acknowledge that the Minis et al. interpretation is not the overwhelmingly accepted view of the scientific community. The scientific uncertainty and debate regarding contrails should be acknowledge; in particular the authors should review and include comment from the rebuttal to the Minnis' paper written by K. Shine of the UK's Reading University (Keith P. Shine. 2005: Comments on "Contrails, Cirrus Trends, and Climate". Journal of Climate: Vol. 18, No. 14, pp. 2781–2782.).	Accepted. Section revised.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				. [Lourdes Maurice]	
2-1766	A	47:31	47:35	I recommend you drop the reference to the Minnis work. I think Shine showed pretty convincingly that it was in error. It is also presents a logical inconsistency for the IPCC FAR if it is read without knowledge of the details -- e.g. if aviation was responsible for all the measured surface temperature change then what about everything else? [Ian Waitz]	Rejected. It is important to make a statement about this work.
2-1767	A	47:33	47:34	The statement "This unexpectedly large impact has not been confirmed in two climate model studies" is understated and misleading. Both studies derive results that are orders of magnitude lower than the Minnis 2004 study. [Steven Baughcum]	Accepted. Section revised.
2-1768	A	47:34	47:34	write ''… in three other independent studies" instead of ''…in two climate model studies". [Mikhail Danilin]	Accepted. Section revised.
2-1769	A	47:34	47:34	Two studies are mentioned, but there are three references [Eugene Rozanov]	Accepted. Section revised.
2-1770	A	47:36	47:36	Add the following sentence after ''… Shine (2005)": Minnis (2005) accepted criticism of Shine (2005), but argued that estimates by Minnis et al. (2004) and Shine (2005) represent maximum and minimum regional response. [Mikhail Danilin]	Accepted. Section revised.
2-1771	A	47:36	47:36	Another explanation is that the GCMs do not simulate regional climate responses very accurately (Minnis, 2005). Minnis, P., 2005: Response to comment on "Contrails, Cirrus Trends, and Climate." J. Climate. 18, 2783-2784. [Patrick Minnis]	Accepted. Section revised.
2-1772	A	47:38	47:48	Observed trends of contrails might better fit Chapter 3 [Robert E. Dickinson]	Noted.
2-1773	A	47:46	47:47	Add the following sentence before ''Thus,…": However, Schumann (2004) claimed that statistical significance of the Travis et al. (2002,2004) studies was weak, since a similar magnitude DTR change was also observed in 1982 and obviously was caused by different reasons. [Mikhail Danilin]	Noted. Reference added.
2-1774	A	47:47	47:48	I disagree that the DTR effect is unexpected. Anytime clouds occur during the day, they will reduce the amount of absorbed sunlight and thereforfe the maximum temperature. Contraisl that form in otherwise clear skies reduce the insolation and therefore should reduce the maximum temperature. That is a physical model that is not far fetched. [Patrick Minnis]	Noted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1775	A	47:47	47:47	I think perhaps I know why you use the word "unexpected" but it is rather cryptic for most readers - what is unexpected? [Keith Shine]	Accepted. Section revised.
2-1776	A	47:53	47:54	Write the following more acurate sentence instead of that started with ''As a result…": The aviation tracer simulations showed that subsonic fleet could be a significant source of soot mass and sulfate aerosol concentration (but not sulfate mass density) at cruise altitudes (Danilin et al., 1998)." [Mikhail Danilin]	Rejected. Detail not needed here.
2-1777	A	47:54	47:56	Regarding the sentence "The most important aerosols are those composed of sulphate and black carbon (soot)," there is strong evidence now that condensed hydrocarbons are also present in aviation aerosol emissions. The condensed hydrocarbons may be due to "small inefficiencies in the combustion of the fuel" (or more accurately "incomplete combustion" since, especially at cruise, the engine efficiency is so high and the incomplete combustion is so small compared to the production of CO_2) or due to contamination of the exhaust stream by small amounts of engine lubrication oil. Suggest rewording: "The aerosols are composed of sulphuric acid, condensed organic species, and black carbon (soot). Sulphuric acid aerosols arise from the emissions of fuel sulphur and black carbon aerosol results from small inefficiencies in the combustion of aviation fuel. Organic contributions to these aerosols may arise from aviation fuel combustion inefficiencies or from contamination of the exhaust stream by small amounts of engine lubrication oil." [Lourdes Maurice]	Rejected. All emissions are referenced with IPCC-1999. The most important are thought to be sulfate and soot.
2-1778	A	47:54	47:54	PROOFREADING TYPE COMMENT: Substitute "affects" with "effects" ... [Malte Meinshausen]	Rejected. Sentence changed.
2-1779	A	48:3	48:4	The authors should point out that the Hendricks study cited assumes that all aircraft soot particles are ice nuclei (as a limiting case). Recent observations in the AIDA aerosol chamber (Moehler and co-workers, JGR, 110, doi:10,1029/2004JD005169, 2005) indicates that soot with a sulfuric acid coating has a significantly higher ice nucleation threshold than pure soot. Since aircraft emitted soot are expected to be coated with sulfuric acid in the plume, these observational results would suggest that the indirect effect of aircraft soot on cirrus will be significantly less than the models currently predict. [Steven Baughcum]	Accepted. Reference added.
2-1780	A	48:9	48:12	The last sentence of this paragraph is incorrect, since Mannstein and Schumann (2005) estimated that RF from contrail-induced cirrus could be at leat 10 times larger than that of linear contrails and aviation induced CO_2 increases. Hence, I suggest to modify the last sentence as follows: Some preliminary estimates of RF of aviation-induced cloudiness are reported by Mannstein and Schumann (2005) or could be derived from the relevant	Rejected. The last sentence only refers to the effect of aviation aerosol, which has not been estimated.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				correlation studies (Section 2.6.3). [Mikhail Danilin]	
2-1781	A	48:13		Would be useful to define terms such as sunspot, faculae, network, ephemeral region before using it in section 2.7 [Fortunat Joos]	Accepted. Descriptions included in revised text.
2-1782	A	48:13		Section 2.7 is too detailed. This particularly applies to figures 2.7.1 to 2.7.5, which would suit in papers, but not in AR4. [Peter Siegmund]	Taken into account – Figures 2.7.1 and 2.7.2 have been replaced with more simple figures
2-1783	A	48:14	53:34	The solar section is excellent. I would like to suggest that a summary table bringing together all the reasons for improved confidence in the solar forcing would be extremely helpful. This would include e.g., observations of the solar diameter, improved calibrations from multiple sources, spectral coverage information, improved analysis of data on our sun compared to other stars, etc. [Susan Solomon]	Accepted, in part. Table 2.7.1 has been modified in response to this suggestion.
2-1784	A	48:15	53:34	This section argues for a lower value of RF for solar irradiance than was reported in the TAR, and also that the evidence for the proposed solar variability-cosmic ray-cloud formation feedback is weak. However, several recent studies show significant climate effects for changes in solar irradiance. Wang, Y. et al (2005): The Holocene Asian Monsoon: Links to solar changes and North Atlantic climate, Science, 308, 854-857, shows such an impact on the Asian monsoon. Hu, F.S., et al.(2003): Cyclic variation and solar forcing of Holocene climate in the Alaskan subarctic. Science, 301:1890-1892 concludes: "Our sudies imply that small variations in solar irradiance induce pronounced cyclic changes in northern high latitude environments. They also provide evidence that centenial-scale shifts in the Holocene climate were similar in the subpolar regions of the North Atlantic and North Pacific, possibly because of Sun-ocean-climate linkages." These studies raise questions about the low RF assigned to solar variability and also whether there is not a yet unidentified feedback that is amplifying changes in solar irradiance and causing the climate changes reported in these studies. [Lenny Bernstein]	Rejected. The RF for solar irradiance is based on current observations and understanding of the amplitudes and mechanisms of the irradiance variations. Paleo studies, such as by Wang et al (2005) and Hu et al (2003) report correlations of climate and solar proxies. In particular, the proxies of solar variability are cosmogenic isotopes. Cosmogenic isotopes are not the result of irradiance variations; rather, both cosmogenic isotopes and solar irradiance variations are associated with changes in the magnetic field of the Sun, but not by common mechanisms. Whereas irradiance variations are associated with the total magnetic flux (actually the closed magnetic flux in active regions), the cosmogenic isotopes variations derive from changes in the open magnetic flux that extends into the heliosphere (about 10-20% of the total). Simulations with

No.	Batch	Page:line From	Page:line To	Comment	Notes
					models of magnetic flux transport on the sun (Wang et al., 2005) show that total and open flux do not appear to vary co-linearly. Thus, relationships between cosmogenic isotopes and climate cannot say anything, conclusively, about changes in solar irradiance. In addition to knowledge of the physical association of irradiance and cosmogenic isotopes, deciphering the implications for solar radiate forcing of paleo correlations between climate and cosmogenic isotopes proxies requires new understanding of the climate response to solar forcing, as well as of the climate impact on the cosmogenic isotopes.
2-1785	A	48:15	53:34	This section presents a lower value for radiative forcing due to changes in solar irradiance than did the TAR. It also dismisses the proposed solar variability-cosmic ray-cloud formation feedback, saying that there is insufficient evidence for supporting it. However, several recent paleoclimatic studies add to the literature showing significant climate effects for changes in solar irradiance. Wang, Y. et al (2005): The Holocene Asian Monsoon: Links to solar changes and North Atlantic climate, Science, 308, 854-857, shows such an impact on the Asian monsoon. Hu, F.S., et al.(2003): Cyclic variation and solar forcing of Holocene climate in the Alaskan subarctic. Science, 301:1890-1892 concludes: "Our sudies imply that small variations in solar irradiance induce pronounced cyclic changes in northern high latitude environments. They also provide evidence that centenial-scale shifts in the Holocene climate were similar in the subpolar regions of the North Atlantic and North Pacific, possibly because of Sun-ocean-climate linkages." Chapter 6 argues that the Medieval Warm Period and Little Ice Age were caused by changes in solar irradiance that affected specific regions of the Earth. Clearly these changes did not stop in 1750. The authors need to either more completely explain why they believe that changes in solar irradiance have had such a small impact on the climate system since 1750 or assign a higher value to radiative forcing due to these changes. [Jeffrey Kueter]	Rejected – same as 2-1784
2-1786	A	48:15		Section 2.7.1. A recent paper, in line with the IPCC draft findings, could be cited in the next draft: Scafetta N. and B. J. West (2005), Estimated solar contribution to the global surface warming using the ACRIM TSI satellite composite, GEOPHYSICAL	Taken into account. However, this paper is not in line with the findings about solar forcing in the IPCC draft,

No.	Batch	Page:line From	Page:line To	Comment	Notes
				RESEARCH LETTERS, VOL. 32, L18713. [Philippe Tulkens]	which attributes the secular increase in the ACRIM irradiance composite (which Scafetta and West use) to instrumental rather than solar effects. It is very likely that Scafetta and West (GRL, 20005) overestimate a solar contribution of 10-30% of surface warming in recent decades. Their paper is cited, but with the caveats that their claim depends crucially on the observational solar irradiance time series adopted for that period; nor is it reproduced by a multivariate linear regression analysis of the solar and climate data. Details are in a comment on this paper (Lean, 2005, Submitted to GRL). Revised text better clarifies the issue of contemporary irradiance trends and surface warming (see also 2-1795).
2-1787	A	48:17	48:31	The serious discrepancies in measuring total solar irradiance from space has to be regarded as an embarassment for NASA, and a point of serious discouragement for the solar physics community. Also, this seems to call into question the solar radiative forcing values shown in Fig. 2.9.2., especially so, given the notorious "glitches" in various NASA times series of differing space-based measurements of total solar irradiance. As a point of empathy with the solar physics community, it is important for us "terrestrial" scientists to acknowledge that the earth-oriented NASA satellite measurements that we "earth people" have to deal with contain equally seriously inadequate, and "glitch-filled", time series. This is more than a tirade from me. Indeed, it is a problem for the entire climate-science community. NASA has simply refused to accept its obligation to provide quantitative data that are centrally relevant for climate warming research, and documentation. Unfortunately, we "terrestrials" get stuck with having to counter such silly claims as "The climate community has spent over 25 Billion $ on global warming research over the past decade." Not even close. The satellites that cannot hope to produce climate-relevant measurements cannot rationally be added to the "cost for global warming" bill" when they were never designed to do so. When I served as Chair of the Advisory Committee for NASA's Mission to Planet Earth section of NASA, this was already identified as a major problem that needed serious remedial attention.	Rejected. To avoid confusion, the text about the radiometric discrepancies in the radiometric measurements from space has been removed. These discrepancies are attributable to instrumental differences- currently thought to be the placement of defining aperture. They are not real solar variability. The problem of cross-calibrating individual observational records is a serious one for all climate records, not just solar irradiance. Recognizing the presence of instrumental effects, the indiviudal observations can be adjusted by utilizing their overlap. The overlapping records exist because of NASA's commitment to acquiring this record.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Unfortunately, that remains true today. [Jerry Mahlman]	Absolute radiometry in space, whether of solar irradiance of terrestrial radiances, is a difficult problem – and one which NPOESS is just beginning to confront - this is not NASA's fault! Thus, a composite record is constructed, whose variations are extrapolated in time using current understanding of the solar sources of irradiance variations, and their temporal histories. The measurement discrepancy does not invalidate the irradiance reconstruction in Fig 2.7.3 on the solar RF estimates are based..
2-1788	A	48:17		Think it better to separate out solar variability from volcanic activity as processes, level on understanding etc very different [Alan Rodger]	Noted
2-1789	A	48:17		Could not find any comment to the ~200 year solar cycle. There is increasing evidence that a peak in solar activity occurred ~1957 and solar activity is now diminishing (e.g. Clilverd et al., 2003) [Alan Rodger]	Noted. Added metion of this cycle to text. The 200 (actually 208?) year cycle is present in cosmogenic isotopes, and it is also present in a number of paleo-climate records. As such, this item may be better suited for Chapter 6 but it could also be noted in Chapter 2. Various predictions of future solar activity could also be included in Chapter 2, but they may be better in Chapter 10, which is about future climate. The predictions have large uncertainties (see Lean et al., Solar Phys., 2005 for details). From spectral synthesis of the cosmogenic isotope record Clilverd et al. (2003) indicate that solar activity is presently peaking, and in 2100 will reach levels comparable to those in 1990. Projections of combined 11-, 88- and 208-year solar cycles also suggest that

No.	Batch	Page:line From	Page:line To	Comment	Notes
					solar activity will increase in the near future, but only until 2030, followed by decreasing activity until 2090 (Jirikowic and Damon, 1994). In contrast, a numerical model of solar irradiance variability which combines cycles related to the fundamental 11-year cycle by powers of 2 predicts a 0.05% irradiance decrease during the next two decades (Perry and Hsu, 2000).
2-1790	A	48:25	48:27	The 5 W/m2 discrepancy seems to be a clear case of a calibration problem. Knowledge of the exact absolute value of the solar irradiance is of considerably lesser importance to understanding global climate change than knowing the relative trend. [Andrew Lacis]	Accepted. The 5 Wm^{-2} discrepancy is without doubt a calibration problem, not a real change in solar irradiance. The text about this radiometric discrepancy has been removed to avoid confusions about instrumental versus true solar variations. For IPCC, knowledge of the variability is a higher priority than knowledge of absolute irradiance, but nevertheless, the improved measurements of forcings will rely increasingly on "benchmark" measurements traceable to absolute standards, not just for solar irradiance but for all climate variables. This issue is crucial for NASA and NOAA future measurement strategy.
2-2749	B	48:27	48:27	check parenthesis [Olivier Boucher]	Noted. Relevant text removed as per 2-1790.
2-1791	A	48:27	48:28	See comment #3. [Joanna Haigh]	Noted – but not sure to which this refers - is it comment 2-3?
2-1792	A	48:44		correct: "rotation on" to "rotation around" [Hartmut Grassl]	Rejected – disagree that this change is necessary
2-1793	A	48:51		Haven't told the reader what TIM is. [Joanna Haigh]	Accepted – "Total Solar Irradiance Monitor" added to text.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1794	A	49:1	52:56	The serious discrepancies in measuring total solar irradiance from space has to be regarded as an embarassment for NASA, and a point of serious discouragement for the solar physics community. Also, this seems to call into question the solar radiative forcing values shown in Fig. 2.9.2., especially so, given the notorious "glitches" in various NASA times series of differing space-based measurements of total solar irradiance. As a point of empathy with the solar physics community, it is important for us "terrestrial" scientists to acknowledge that the earth-oriented NASA satellite measurements that we "earth people" have to deal with contain equally seriously inadequate, and "glitch-filled", time series. This is more than a tirade from me. Indeed, it is a problem for the entire climate-science community. NASA has simply refused to accept its obligation to provide quantitative data that are centrally relevant for climate warming research, and documentation. Unfortunately, we "terrestrials" get stuck with having to counter such silly claims as "The climate community has spent over 25 Billion $ on global warming research over the past decade." Not even close. The satellites that cannot hope to produce climate-relevant measurements cannot rationally be added to the "cost for global warming" bill" when they were never designed to do so. When I served as Chair of the Advisory Committee for NASA's Mission to Planet Earth section of NASA, this was already identified as a major problem that needed serious remedial attention. Unfortunately, that remains true today. [Jerry Mahlman]	Noted. See 2-1787
2-1795	A	49:1		"secular trend in excess of 0.04% over the 27-year period of the irradiance database is likely of instrumental rather than solar origin" A absolutely critical phrase but the phrasing implies this is almost a suggestion. How robust is this statement. [Alan Rodger]	Taken into Account. The text has been rewritten to make the case stronger, and a new Figure 2.7.1 made to demonstrate this. There is substantial and increasing evidence that the "secular trend" in the ACRIM composite is instrumental, not solar, for reasons discussed further in Lean (Comment submitted to GRL, 2005). To reinforce this, the new Fig 2.7.1 shows the ACRIM composite in which irradiance levels in the upcoming solar minimum (2006) appear to be lower than in 1996, which would not be the case were an upward secular trend present.
2-2750	B	49:11	49:11	is --> are [Olivier Boucher]	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1796	A	49:21		correct: "indicate" to "indicates" [Hartmut Grassl]	Accepted.
2-1797	A	49:25	49:39	Comment: would it not be useful to look at trend in the tropical stratosphere as another corroborative clue? [Rasmus E. Benestad]	Noted. A number of studies have isolated solar and anthropogenic trends in stratospheric ozone. Others have analyzed various datasets (e.g., NCEP reanalysis temperatures) that extend throughout the troposphere into the stratosphere (e.g., the Haigh, 2003 reference which is cited). Chapter 2 ZOD described these results but review of ZOD suggested that this text was too detailed, and involved responxes, rathering that forcing, so it was not included in Chapter 2 FOD.
2-1798	A	49:25	49:39	The interesting discusssion of "observed" effects of solar variability would be more credible if placed in Chapter 3 [Robert E. Dickinson]	Noted. The empirical evidence for climate response to solar variability, though increasingly convincing and extensive does not have a "home" in any of the IPCC chapters because of concernt that it cannot be readily explained at the present time, and because if taken as genuine and not spurious observations, the evidence raises flags about current understanding of climate change that may undermine credibility of climate models which are typically unable to reproduce the observed correlations. Comment 2-1872 notes this dilemma. Perhaps for this reason, some empirical evidence for climate responses to solar variability has been included in the Radiative Forcing sections of previous IPCCs. It reflects the fact than the observations of responses are considered to imply something about radiative forcing. This reality is also

No.	Batch	Page:line From	Page:line To	Comment	Notes
					evident in a number of other comments: The FOD of Chapter 3 discusses the relationship between cosmic rays and clouds (in Section 3.4) but not the analysss of MSU and NCEP reanalysis temperatures that have also identified solar signals (Douglass and Clader, Haigh, Gray etc – and many of the references that reviewers suggest be included in Chapter 2). The empirical sun-surface temperature results could be placed in Chapter 3, or they could be placed in Chapter 9, about understanding and attribution. They could also remain in Chapter 2.
2-1799	A	49:25	49:39	The ozone and temperature response in the stratosphere should be also discussed. It is also necessary to mention a substantial disagreement between solar signal in the stratospheric temperature obtained from the different data sets as it was done by Egorova et al. (2004, GRL) and Rozanov et al.(2004, JGR). [Eugene Rozanov]	Noted. The effects of solar UV irradiance changes on stratospheric ozone and temperature are discussed briefly in Section 2.7.1.3, the section on indirect effects of solar forcing. However, lack of space precludes detailed discsussions of indiviudal results.
2-1800	A	49:28	49:29	In the list of publications describing effects of the solar cycle in the troposphere, the publication by Gleisner et al. (Gleisner, H., P. Thejll, M. Stendel, E. Kaas and B. Machenhauer, 2005: Solar signals in tropospheric re-analysis data: comparing NCEP/NCAR and ERA40. J. Atm. Solar-Terr. Phys. 67, 785-791) is missing. In this paper, which is an extension of Stendel, M., J.R. Christy and L. Bengtsson, 2000: Assessing levels of uncertainty in recent temperature time series. Clim. Dyn. 16, 587-601, the authors show from identical analyses of NCEP/NCAR and ERA-40 reanalyses that Sun–climate relations are substantially weaker in the ERA40 than in NCEP due to the presence of temporal inhomogeneities on a decadal time scale in ERA40. [Martin Stendel]	Numerous papers that report aspects of empricial assoications between the sun and climate were not included in the FOD because of lack of space. The ZOD did include a much more detailed discussion of climate responses to solar forcing, but this discussion was removed from the FOD in response to review comments.
2-1801	A	49:33	49:37	Suggest changing words to following: The strongest response occurs in two mid-latitude bands that extend vertically from the lower stratosphere to the surface. Increases of up to 0.5K are observed at the surface near 40-50 N and S (Haigh 2003. [Lesley Gray]	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1802	A	49:34		insert a hyphen between "sub" and "tropical" [Hartmut Grassl]	"sub" and "tropical" removed from text in response to 2-1801
2-2751	B	49:35	49:37	do you mean a 0.5K change in surface temperature between solar maximim and minimum? Doesn't this contradict 0.1K temperature change mentioned above? [Olivier Boucher]	The 0.1 K response is for the global mean signal, wheras the 0.5 K respone is regional.
2-1803	A	49:36	49:37	replace "where they expand equatorward (Haigh, 2003)." by "(Haigh, 2003). The associated response in zonal wind reveals that the sub-tropical jets are weaker and further poleward when the sun is more active (Haigh et al, 2005)." Ref: Haigh, J D, M Blackburn and R Day 2005 The response of tropospheric circulation to perturbations in lower stratospheric temperature. J.Climate, 18, 3672–3691 [Joanna Haigh]	Accepted. Text rewritten.
2-1804	A	49:39	49:39	An important addition to knowledge since TAR concerning solar irradiance is its time series properties. I suggest the following insertion: "Flux data have also supported recent estimation of important time series characteristics of total solar irradiance. These have shown, in particular, that it is antipersistent, or dominated by cumulative negative feedbacks, with Hurst exponent <0.5 (Kärner 2004, 2005). Since LLGHG's increase monotonically over time they can be shown to exert a RF that implies a persistent forcing, dominated by cumulative positive feedbacks (Hurst exponent >0.5). This difference provides a potential alternative to model-based methodology for discriminating solar and anthropogenic forcing on the Earth's climate system." [Ross McKitrick]	Rejected. Too much detail for limited space in Chapter 2, and cooment relates more to detection (Chapter 9?) than forcing.
2-1805	A	49:39	49:39	References cited in above cell: Kärner, Olavi (2003) "On nonstationarity and antipersistency in global temperature series" Journal of Geophysical Research (v107 D20, pp ACL 1-1 to 1-11). Kärner, Olavi (2005) Some examples of negative feedback in the Earth climate system. Central European J. of Physics vol 3 No 2, pp 190-208 [Ross McKitrick]	Noted. Reference is more applicable for attribution (Chapter 9) than forcing (Chapter 2).
2-1806	A	49:51	50:12	The implications of the phrase "during solar rotation" may be missed by the inexpert reader who may then deduce from Fig.2.7.2 that there is less energy coming out of the Sun when it is more active. Although the last 2 sentences of the paragraph retrieve the situation perhaps it might help to introduce the para with a sentence or two explaining that sunspots cause darkening, faculae brightening and that by using 27-day rotation we can investigate the spectra and test models of these features. [Joanna Haigh]	Accepted. Figure 2.7.2 has been revised and the spectral energy change added for the solar cycle, as well as for solar rotation. Text has been reorganized to better clarfy the nature of the solar cycle changes.
2-1807	A	50:18	50:24	It is not clear what exactly is being described here. [Andrew Lacis]	Noted. Text rewritten.
2-1808	A	50:18	50:19	Solanki et al. study should be referenced in the opening line and the differnces between it and the other studies highlighted. Present wording is misleading.	Noted. There is a reference to Fligge and Solanki (2000).Added text a few

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Stephen McIntyre]	paragraphs later to point out that the curve that defines the lower level of shading in Figure 2.7.3 is essentially identical to the latest (available) Solanki and Krivova (2005) irradiance reconstruction. This latter reconstruction was published after TAR, so is not relevant in the context of the opeing sentence.
2-1809	A	50:27	50:27	Explain "aa index" [Ralf Koppmann]	Replaced "aa index" with "geomagnetic activity"
2-1810	A	50:48	50:53	comment: sentence too long [Hartmut Grassl]	Accepted. Text rewritten.
2-1811	A	50:50	50:53	The clause here on Be-10 is hard to interpet. What does it mean. The 14C record also suggest some cyclicity during the Maunder Minimum (Muscheler et al., 2005b). Does this suggest that the Wang and Sheeley model has some problems or that the Be-10 data are flawed. Some more explanation and guidance is required here. [Fortunat Joos]	Taken into account Text rewritten to clarify. The inability of solar magnetic flux models to account for the modulation of heliospheric magnetic field needed to produce the observed ^{10}Be variations in the Maunder Minimum s that 1) the ^{10}Be variation during this time are caused by something else – perhaps unforced climate change, 2) mechanisms of heliospheric and/or atmospheric modulation of the cosmic rays that produce ^{10}Be are not adequately known, 3) the extant sunspot record combined with a flux transport model is not adequate for estimating solar magnetic flux in the Maunder Minimum
2-1812	A	50:50	50:53	Perhaps I am not understanding this very well - as I read it, it is saying that because we cant explain the 10Be variation, there is, therefore, no long term change. It just sounds a bit flimsy as written, but a simple rewording might help. [Keith Shine]	Taken into account – similar to comment 2-1811
2-1813	A	50:55	50:55	A lot of attention is paid to Foster (2004), but this paper is not readily available and it is hard to judge what has been done.	Accepted. The Foster (2004) reference and time series in Figure 2.7.3 have

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Eugene Rozanov]	been removed
2-1814	A	50:55		correct to: "are derived" after the brackets [Hartmut Grassl]	Sentence rewritten.
2-1815	A	51:4		delete: "Lean et al.," [Hartmut Grassl]	Accepted.
2-1816	A	51:15	51:15	geomagnetic and cosmogenic isotopes'? 'cosmogenic isotopes' is sufficient [Fortunat Joos]	Accepted.
2-1817	A	51:20	51:20	I think you need to be quite clear that these are TSI not forcing values. [Keith Shine]	Accepted. Revised text (mainly) uses percentages rather than energy units for the irradiance changes.
2-1818	A	51:23	51:23	Specify which solar irradiance composite is used here. [Fortunat Joos]	Sentence deleted – see 2-1813 re Foster (2004) reference.
2-2752	B	51:41	51:41	Add period at the end of the sentence. [Olivier Boucher]	Accepted.
2-1819	A	51:41	51:41	Insert a period after the word "Maximum." [Patrick Hamill]	Accepted.
2-1820	A	51:41		a full stop is missing after "Maximum" [Hartmut Grassl]	Accepted.
2-1821	A	51:44	51:44	Remove word "such" from "An initial such effort..." [Patrick Hamill]	Accepted
2-1822	A	51:51	51:57	It should be explained more clearly how a change in total irradiance of 0.5 W/m2 becomes a RF of 01.12 W/m2. [Andrew Lacis]	Accepted. Text rewritten.
2-1823	A	51:51	51:51	Ensure that all possibilities are referenced, including the highest. [Stephen McIntyre]	Noted. The text includes a high estimate.
2-2753	B	51:53	51:53	I don't see why accounting for the 11 yr cycle increases the secular trend unless there is a trend in the maximum of the 11 yr cycle itself. Can you clarify? [Olivier Boucher]	Noted. The amplitudes of present-day 11 year sunspot cycles (since about 1950) are larger than in the Maunder Minimum when the cycles were small or absent. Text revised to state this.
2-1824	A	52:12	53:34	I agree with the conclusions of this section. However, there is some relevant work that should be considered. It should be noted that observations of new particle formation between the middle troposphere and lower stratosphere are well reproduced by an H_2SO_4/H_2O ion-induced nucleation model that is based on experimental thermodynamics of cluster ion growth (Lee et al., 2003; Lovejoy et al., 2004). However, it should also be noted that uncertainties in the neutral H_2SO_4/H_2O nucleation rates at the	Noted. The limited space precludes the adidtiona of this detail to the text.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				low temperatures of this region are significant, and efficient neutral nucleation could overwhelm ion-induced nucleation. So, there is increasing evidence that ions are important precursors of atmospheric aerosol, but there is still considerable uncertainty in the strength of the coupling between ionization and new particle formation. The coupling between nucleation and cloud properties is also very poorly understood, as stated in the first draft. [Edward R. Lovejoy]	
2-2754	B	52:12		The indirect effect of solar variability on clouds seem to be gaining some momentum (I mean it is not discarded). [Olivier Boucher]	Noted.
2-1825	A	52:12		On "2.7.1.3. Indirect effects of solar variability." The relation between solar wind and AO index should be mentioned because a similar relation between volcanic activity and AO is referred to. There are several reports dealing with this kind of relation: [1] Palamara & Bryant ("Geomagnetic activity forcing of the Northern Annular Mode via the stratosphere," Annales Geophysicae (2004) 22: 725–731): [2] Boberg & H. Lundstedt ("Solar Wind Variations Related to Fluctuations of the North Atlantic Oscillation," Geophys. Res. Lett., VOL. 29, NO. 15, 1718, 10.1029/2002GL014903, 2002). These relations are useful because they are potentially able to solve the discrepancy between the surprisingly small change in the solar luminosity and the large fluctuation in the global historical temperature records (Esper et al, 2002; Moberg et al., 2005). Another recent study on the AO reveals that the AO can be excited by various kinds of external forces (H. L. Tanaka & M. Matsueda, J. Meteorol. Soc. Jpn., 83, 611-619 (2005)). Thus, relations between the solar magnetic activity and the AO became more realistic than before. [Kiminori Itoh]	Noted. Added this possibility and a reference in revised text.
2-1826	A	52:13	52:24	Lines 13-15 are inconsistent with 16&17 and the rest of this paragraph - how about starting line 13 with "It used to be thought that .." [Howard Roscoe]	Accepted. Text rewritten to clarify.
2-1827	A	52:13	52:13	I suggest some modest rewording to separate out the importance of the UV itself to the atmosphere, and the importance of the UV part of the solar cycle variation. It is a bit mixed in. [Keith Shine]	Noted. See prior comment.
2-1828	A	52:16		delete the comma after "energy" [Hartmut Grassl]	Accepted.
2-1829	A	52:17	52:18	The sentence "Solar UV variation is more variable than total solar irradiance ..." needs to be clarified: it is apparently in contrast with the statement at page 49, line 51-52. The sentence on page 49 is more appropriate, sincea distinction between relative and absolute	Accepted. Text reworded.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				variations is made. The statement on page 52 is true for the relative variability only. [Alcide di Sarra]	
2-2755	B	52:19	52:20	This is quite a weird sentence. Radiation does not have to "reach the Earth's surface" to create a direct forcing…. [Olivier Boucher]	Accepted. Reworded the text and changed "surface" to "troposphere".
2-2756	B	52:24	52:24	add a period at the end of the sentence [Olivier Boucher]	Accepted.
2-1830	A	52:24		a full stop is missing [Hartmut Grassl]	Accepted.
2-1831	A	52:26	52:38	All recent publications with the analysis of solar signal and its downward propagation from the upper stratosphere to lower stratosphere and further down to the surface are missing. For example: Kodera and Kuroda (2002, JGR), Tourpali et al., (2003, GRL), Egorova et al. (2004, GRL), Matthes et al.(2004, JGR). The report of Gray et al. (2005) could contain all missing references, but it is not readily available. The published results explain in part the response in the lower stratosphere by the alteration of the B-D circulation (see also Egorova et al., 2005, ACP). [Eugene Rozanov]	Noted but Rejected, partially. The possible effect on the stratosphere is discussed in the following paragraph., which does note circulation changes and expliciatly cites Matthes et al (2004). Brief related text is added in response to 2-1843. Note that more references were included in the ZOD but removed because of lack of space. The Gray et al provides comprehensive references, so is cited instead of multiple additional references.
2-1832	A	52:31		Gray et al reference -would be better to offer refereed references rather than this commissioned,\ unrefereed work [Alan Rodger]	Noted. However, the Gray et al publication is unqiue in its coverage of the topic, especially recent results.
2-1833	A	52:37	52:39	Suggest replacing last sentence by: For example, solar forcing appears to induce a significant lower stratospheric response (Hood, 2003), which may have a dynamical origin caused by changes in temperature affecting planetary wave propagation, but it is not currently reproduced by models. [Lesley Gray]	Accepted.
2-1834	A	52:40	52:43	The citations in this sentece are inappropriate. The authors have chosen to ignore the primary research articles and instead focus on 'opinion' style pieces. Although these pieces appeared in Science, they are not peer-reviewed in the same sense as actual research articles. The citation of the Haigh 2001 commentary in Science instead of the Shindell et al paper on which it is commenting is particularly ill-chosen. The Rind 2002 citation is another Science 'opinion/review" article rather than peer-reviewed research. There are a great number of research articles in thsi area, and nearly all of them have been neglected here so that these opinion pieces can be included. The latter should be dropped	Noted but Rejected, in part. The Shindell et al result is one of a number of recent papers describing an array of solar-climate associations and model simulations. In fact four papers by Shindell et al. were referenced in the ZOD of Chapter 2. These and many other specific references to climate

No.	Batch	Page:line From	Page:line To	Comment	Notes
				in favor of articles lsuch as Shindell et al., Science, 2001; Tourpali et al, GRL, 2005; Langematz et al., JGR, 2004, Matthes et al., JGR, 2004. [Drew Shindell]	response to solar forcing were removed at the request of ZOD reviewers. Brief text has been reinserted – see response to 2-1843. The many references of the ZOD were replaced by general references to the topical, specifically the overview papers of Haigh and Rind. See response to comment 2-1798, also about the ambiguity of where (or if) to include discussions of understanding and attribution of climate responses to solar forcing in a Chapter 2, which is on Radiative forcings, or in Chapters 3 or 9.
2-1835	A	52:40	52:51	Is there something that could be done to make this paragraph more quantitative. Although the Haigh circulation change is convincing, it is nevertheless rather small in magnitude. [Keith Shine]	Noted.
2-1836	A	52:41	52:41	attitudinal -> altitudinal? [Reto Knutti]	Accepted.
2-1837	A	52:41	52:41	replace "attitudinal" by "altitudinal" (spelling error) [Peter Van Velthoven]	Accepted.
2-1838	A	52:41		correct: "attitudinal" to "altitudinal" [Hartmut Grassl]	Accepted.
2-1839	A	52:42	52:42	Haigh (2001) is not the appropriate reference (since it is just an un-peer reviewed commentary piece. The relevant reference is the paper she was commenting on: Shindell, D.T., G.A. Schmidt, M.E. Mann, D. Rind, and A. Waple 2001. Solar forcing of regional climate change during the Maunder Minimum. Science 294, 2149-2152, doi:10.1126/science.1064363. [Gavin Schmidt]	Noted. See response to 2-1834.
2-1840	A	52:45	52:45	Insert period after "2004)" [Patrick Hamill]	Accepted.
2-1841	A	52:45		Add after "Equatorial wind": (QBO). [Joan Feynman]	Accepted.
2-1842	A	52:45		a full stop is missing after "2004)" [Hartmut Grassl]	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1843	A	52:46		Add text: Solar variability influences annular-like patterns of wintertime climate anomalies, called NAM in the Northern hemisphere. These major modes of atmospheric variability extend from sea level to the top of the stratosphere accounting progressively from 23% to 50% of the total variability (Thompson and Wallace, 1998; Baldwin and Dunkerton, 1999). Observational analysis shows that the NAM is statistically significantly affected by changes in solar UV flux in earlier winter for the West QBO and in late winter for the East QBO (Ruzmaikin and Feynman, 2002). This analysis confirms and extends earlier indications that the stratospheric polar temperature at 30 hPa varies from solar maximum to solar minimum (Labitzke, 1987). Solar influence on Annular Modes was also demonstrated in GCM modeling that did not include the QBO (Shindell et al., 1999; Shindell et al., 2003). Add to the reference list: Baldwin, M. P. and T. J. Dunkerton, Propagation of the Arctic Oscillation from the stratosphere to the troposphere, J. Geophys. Res. 104, 30,937-30,946, 1999. Labitzke, K., Sunspots, the QBO, and the stratospheric temperature in the north polar region, Geophys. Res. Lett., 14, 535-537, 1987. Ruzmaikin and Feynman, Solar influence on major mode of atmospheric variability, J. Geophys. Res., 107, doi: 10.1029/2001/JD001239, 2002. Shindell, D., D. Rind, N. K. Balachandran, J. Lean, and P. Lonergan, Solar cycle variability, ozone, and climate, Science, 284, 305-308, 1999. Shindell, D., G. A. Schmidt, R. L. Miller, and M. Mann, Volcanic and solar forcing of climate change during the preindustrial era, J. Climate, 16, 4094-4107, 2003. Thompson, D. W. J. and J. M. Wallace, The arctic oscillation signature in the wintertime geopotential height and temperature fields, J. Geophys. Res. Lett., 25, 1297-1300, 1998. [Joan Feynman]	Accepted, in part. Comments 2-1831 and 2-1843 also request more text on this topic. A brief sentence has been added in Section 2.7.1.3, with some additional references, including the most recent Shindell et al reference..
2-1844	A	52:48	52:48	Add Gray et al., 2001, 2004 to references here (see refs below). Gray, L. J., S.J. Phipps, T.J. Dunkerton, M.P. Baldwin, E.F. Drysdale and M.R. Allen., 2001. A Data Study of the Influence of the Equatorial Upper Stratosphere on Northern Hemisphere Stratospheric Sudden Warmings. Q. J. R. Meteorol. Soc., 127, 576, pg(s). 1985-2004. [doi:10.1256/smsqj.57606]. Gray, L. J., S. Crooks, C. Pascoe, S. Sparrow and M.A. Palmer, 2004. Solar and QBO Influences on the Timing of Stratospheric Sudden Warmings. J. Atmos. Sci., 61, 23, pg(s). 2777-2796. [doi:10.1175/JAS-3297.1] [Lesley Gray]	Accepted – added most recent reference.
2-1845	A	52:48		After Matthes et al, 2004, add a reference: Ruzmaikin et al., 2005). Add to the reference list: Add to the reference list: Ruzmaikin, A., J. Feynman, X. Jiang, and Y. Yung, Extratropical signature of the quasi-biennial oscillation, J. Geophys. Res., 110, doi:10.1029/2004JD005382, 2005.	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Joan Feynman]	
2-1846	A	52:51	52:51	replace "Grey"by "Gray" (spelling errror) [Peter Van Velthoven]	Accepted.
2-1847	A	52:51		replace "Grey" by "Gray" [Joanna Haigh]	Accepted.
2-1848	A	52:53	52:53	Delete this sentence. While it is correct, it is misleading. It implies that cosmic rays cannot affect the climate system because they contain so much less energy than solar radiation. However, if as proposed by the advocates of a cosmic ray mechanism, they trigger cloud formation, very little energy may be needed. There are many instances in nature where a relatively low energy trigger creates a much larger impact. The energy contained in neutrons that trigger a nuclear chain reaction is much less than one-billionth the energy released by the reaction. [Jeffrey Kueter]	Accepted.
2-1849	A	52:53	52:53	Perhaps start a new subsection here - it is a quite different and much more speculative [Keith Shine]	Rejected.
2-1850	A	52:53	53:34	There is discussion of the Svensmark cloud hypothesis in chap 3 page 39 line 50 to page 40 line 24. Chapter 2 is less sceptical of it than chap 3. [Peter Stott]	Noted. See response to 2-1851.
2-1851	A	52:55	54:8	The section on cosmic rays is helpful but I would like to suggest two additions: (I) it would be useful to discuss available laboratory studies that have probed this proposed mechanism and (ii) it would be useful to reference the discussion of clouds and cosmic rays in chapter 3, where it is suggested that the observed variability of cloudiness is difficult to reconcile with a large effect of this type. [Susan Solomon]	Accepted (partially). A cross reference has been added to Chapter 3. But see also response to 2-1798 about where exactly text about climate responses should be discussed – Chapter 2, 3 or 9. .
2-1852	A	53:0		Section 2.7.2. Volcanic activity This section only mentions the long term volcanic reconstructions of Sato et al (1993) and Ammann et al (2003). Whilst these are the main ones used by most recent climate model studies it is important to mention somewhere in this section that other volcanic optical depth reconstructions have been created and been used in models. Just because there has been a convergence in the use of the volcanic forcings, does not mean that there is overwhelming evidence of their accuracy. There has been pressure within the climate modelling community to use just one volcanic dataset, which is distorting the amount of confidence in the accuracy of the dataset itself. Continued on next row.... [Gareth S. Jones]	Taken into account: We referenced only two data sets here that are compiled for GCM studies for the period of interest. Only these two data sets are used in the IPCC models. That there are limitations in these datasets is stated.
2-1853	A	53:0		... Continued from previous row	Taken into account: We

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Section 2.7.2. Volcanic activity Other sources of Volcanic aerosol data sets... this list is not exhaustive. Stothers RB, "Major optical depth perturbations to the stratosphere from volcanic eruptions: Pyrheliometric period, 1881-1960", Journal of Geophysical Research, 1996 & R.B. Stothers, Major optical depth perturbations to the stratosphere from volcanic eruptions: Stellar extinction period, 1961-1978, Journal of Geophysical Research, 2001. Robertson A et al, "Hypothesized climate forcing time series for the last 500 years", Journal of Geophysical Research, 106, D14, 14783-14803, 2001 J. Grieser, C.D. Schonwiese, "Parameterization of spatio temporal patterns of volcanic aerosol induced stratospheric optical depth and its climate radiative forcing", Atmosfera, 12, 111-133, 1999. A Robock, M.P. Free, Ice cores as an index of global volcanism from 1850 to the present, Journal of Geophysical Research, 100, D6, 11549-11567, 1995 [Gareth S. Jones]	referenced only two data sets here that are compiled for GCM studies for the period of interest. Only these two data sets are used in the IPCC models. That there are limitations in these datasets is stated.
2-1854	A	53:0		The serious discrepancies in measuring total solar irradiance from space has to be regarded as an embarassment for NASA, and a point of serious discouragement for the solar physics community. Also, this seems to call into question the solar radiative forcing values shown in Fig. 2.9.2., especially so, given the notorious "glitches" in various NASA times series of differing space-based measurements of total solar irradiance. As a point of empathy with the solar physics community, it is important for us "terrestrial" scientists to acknowledge that the earth-oriented NASA satellite measurements that we "earth people" have to deal with contain equally seriously inadequate, and "glitch-filled", time series. This is more than a tirade from me. Indeed, it is a problem for the entire climate-science community. NASA has simply refused to accept its obligation to provide quantitative data that are centrally relevant for climate warming research, and documentation. Unfortunately, we "terrestrials" get stuck with having to counter such silly claims as "The climate community has spent over 25 Billion $ on global warming research over the past decade." Not even close. The satellites that cannot hope to produce climate-relevant measurements cannot rationally be added to the "cost for global warming" bill" when they were never designed to do so. When I served as Chair of the Advisory Committee for NASA's Mission to Planet Earth section of NASA, this was already identified as a major problem that needed serious remedial attention. Unfortunately, that remains true today. [Jerry Mahlman]	Noted – same comment as 2-1787
2-1855	A	53:4	53:34	There is no long-term trend in the cosmic ray time series (Richardson et al. (2002), JGR, vol 107, doi: 10.1029/2001JA000507; Benestad (2005) GRL, vol 32, doi:10.1029/2005GL023621). Furthermore, a decrease in GCR cannot explain a reduction in DTR (and cloudiness has apparently not decreased according to the accounts in AR4).	Noted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Rasmus E. Benestad]	
2-1856	A	53:4	53:4	"It is supposed" should be written, "Some scientists suppose" or similar. [Gavin Schmidt]	Noted. Text has been rewritten.
2-1857	A	53:4	53:4	"supposed" by who? [Keith Shine]	Noted. Text has been rewritten.
2-1858	A	53:4		There is no doubt that the cosmic rays reach the troposphere but the real physics may relate to the fact that particles are charged and hence their effectiveness to act as CCN is increased by up to a factor of 1000. [Alan Rodger]	Noted. Text has been rewritten.
2-1859	A	53:9	53:13	Yu (2002) does indeed show a theoretical way that high clouds could respond in an opposite way to that of low clouds to changes in GCR. However his claim that their respective changes may account for differences in surface and tropospheric temperatures trends is purely speculative. Yu 2002 only examined the changes over the last two decades to see if the was any evidence to support the claim. A cursory glance at the change in lapse rate (as given in the IPCC TAR) over a longer period, since 1960, shows that it decreased until 1980 then increased subsequently, whilst GCR was consistently decreasing throughout the same period. Besides which, is the original claim still valid with the latest information about surface/tropospheric temperature trends (Chapter 3)? This speculative claim should be removed. [Gareth S. Jones]	Accepted.
2-1860	A	53:9	53:13	Kazil and Lovejoy (2004) have repeated the Yu (2002) exercise using a model that is constrained by laboratory thermodynamics. Kazil and Lovejoy conclude that throughout the troposphere there is a weak positive response of ion-induced nucleation to variations in ionization. This conclusion is significantly different than that of Yu (2002). [Edward R. Lovejoy]	Noted. See response to 2-1859 – the reference to Yu (2002) has been removed.
2-1861	A	53:13		The evidence suggests no statistical long-term trend in the GCR since 1050s (Richardson et al. (2002), J. Geophys. Res., Vol 107, A10; Benestad 2005, GRL, 32 L15714, doi:10.1029/2005GL023621) [Rasmus E. Benestad]	Noted.
2-1862	A	53:15	53:15	I would recommend to add some other new mechanisms, which are proven to be potentially important for solar-climate issue. It was shown by Zubov et al. (2005,) that Joule heating can affect stratospheric temperature during the declining phase of solar activity. The influence of Energetic Electron precipitation events on the temperature in the entire atmosphere has been demonstrated by Rozanov et al. (2005, GRL). [Eugene Rozanov]	Noted but rejected in detail due to lack of space. But see respone to 2-1868 regarding inclusion in revised text of possible MLT effects on stratosphere.
2-2757	B	53:18	53:18	produce --> produced	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Olivier Boucher]	
2-1863	A	53:18	53:18	add "d" after "produce" (spelling error) [Peter Van Velthoven]	Accepted.
2-1864	A	53:18		correct: "produce" to "produced" [Hartmut Grassl]	Accepted.
2-1865	A	53:19	53:21	Is Tegen et al the appropriate reference? [Johann Feichter]	Deleted this reference.
2-2758	B	53:21	53:21	Tegen et al (2004): is that the appropriate reference? [Olivier Boucher]	Deleted this reference.
2-1866	A	53:21		Tegan et al. (2004) is cited as support for the importance of ion-induced nucleation at low [H2SO4]. This reference does not contain any discussion of nucleation. [Edward R. Lovejoy]	Deleted this reference.
2-1867	A	53:24	53:24	"magnitude" - are we even sure of the sign of the forcing? i.e.if high clouds are more affected by CR's the sign of the effect could be different to if low clouds are changed. [Keith Shine]	Accepted. Removed "magnitude".
2-1868	A	53:27		The role of EUV on the mesosphere, lower thermosphere (MLT) region does not appear to be included. Robinson and Arnold (in various papers published in Annales Geophysicae) showed that by changing the upper boundary condition for absorption or reflection of planetary waves in the MLT region then stratospheric temperatures can be increased by a few K and then again feeds back on the propagation of gravity and planetary waves. Subsequent effects on the troposphere of stratospheric changes has been demonstrated by Haigh and co -workers. Given that the MLT region experiences very large changes through the solar cycle, this is another possible mechanism for solar variability affecting the climate and needs to be incorporated. [Alan Rodger]	Accepted. Inserted parenthetical text in Section 2.7.1.3 about the possibility of solar-indiuced changes in the MTL affecting the statopshere – this response also addresses 2-1875.
2-1869	A	53:28	53:29	"As a result …." I found this sentence vague - what is it referring to? [Keith Shine]	Deleted "As a result".
2-1870	A	53:28	53:29	"As a result… evidence." is illegible. [Peter Van Velthoven]	Rejected.
2-1871	A	53:28		correct: "proffered" to "offered" [Hartmut Grassl]	Rejected – proffer means "to present for acceptance"
2-1872	A	53:34	53:34	The paragraph ending at line 34 could be interpreted as applying a double standard. It acknowledges the lack of scientific knowledge about specific mechanisms that might indirectly amplify solar variability. The authors arguing for such lines of evidence have argued that if such information emerges it would conspicuously undermine the existing attribution of climate changes to GHGs, which in IPCC work is done on the basis of an	Noted but Rejected. See response to 2-1798.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				assumed absence of mechanisms for amplification of solar variability. The report elsewhere accepts model-based attribution evidence that is conditional on there not being a significant solar amplification process. So the AR4 appears to consider the absence of an "unequivocal determination of specific mechanisms" an impediment to asserting the possibility of a dominant solar role in climate change, yet does not consider the absence of such knowledge an impediment to asserting a dominant role for GHG's, even though both assertions are equally dependent on nature of the (unknown) "specific mechanisms" at issue. However, while specific mechanisms may not yet be known, the time series evidence does place some new constraints on the likely processes. Hence I suggest the following sentence be inserted after line 34: "While specific physical mechanisms remain the subject of ongoing research (see, e.g. Veizer 2005) it is possible to say that the antipersistency of both surface and tropospheric temperature series (Karner 2003, 2005) closely corresponds to that of total solar irradiance, and is inconsistent with a dominating cumulative positive feedback (persistency) that would be implied if LLGHG increments exerted a governing effect on the Earth's climate system. Hence the possibility cannot be excluded that mechanisms may exist by which solar variability plays a more dominant role in the Earth's climate than is currently assumed." [Ross McKitrick]	
2-1873	A	53:34	53:34	References cited in above cell: Veizer, Jan (2005). "Celestial Climate Driver: Perspectives from Four Billion Years of the Carbon Cycle." GeoScience Canada 32(1) 13-28. For Karner papers see cell G9. [Ross McKitrick]	Noted.
2-1874	A	53:35	57:23	This section should be reduced in length and moved to be part of the Aerosol section, preferably after section 2.4.2 [Andrew Lacis]	Rejected. However ,section shortened
2-1875	A	53:35		In a rather similar fashion energetic particles from the Earth magnetosphere can have a similar effect to cosmic rays i.e cause charging of particles in the stratosphere and above. Furthermore they can change the chemistry (e.g. NO production and O3 loss) and dynamics of the stratosphere and ultimately the troposphere. The importance of this mechanism has yet to be quantified but needs to be included as a possible mechanism. The text needs to point out that the effects of both cosmic rays and energetic particles differ as a function of latitude, time, magnetic field strength, season etc. and hence the response of the atmosphere is not simple to predict. [Alan Rodger]	Noted. See response to 2-1868.
2-1876	A	53:36	53:36	Change header to 'Explosive Volcanic Activity' since it does not discuss quiet, permanent degassing from volcanoes. [Christiane Textor]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1877	A	53:38	53:38	It would be better to start with the observations of the Pinatubo eruption using the material expressed on page 2-54 in lines 6 - 11. Figure 3 from Hansen et al. (2002) shows the time trend of aerosol optical depth and particle size over recent decades. Radiative forcing for the Pinatubo volcanic eruption is described by Hansen et al. (1996). [Andrew Lacis]	Noted. However, entire section has been shortened.
2-1878	A	53:44	53:44	Change 'volcanic eruptions' to explosive volcanic eruptions' [Christiane Textor]	Accepted
2-1879	A	53:48	53:48	"no major volcanic eruptions" - I think this should be "no major climatically important volcanic eruptions"? [Keith Shine]	Accepted
2-1880	A	53:50	53:51	The mentionned e-folding time of 35 days and duration/lifetime of 2-3 months are incompatible.It should probably be more clearly expressed what the 2-3 months refer to. [Peter Van Velthoven]	Accepted: references added
2-1881	A	53:51	53:53	Change to "However, volcanic ash particles (siliceous material) are directly emitted during eruptions." [Alan Robock]	Accepted
2-1882	A	53:51	53:51	Surely this is the place to mention the decay time of volcanic stratospheric aerosol (about 1.5 yr) [Howard Roscoe]	Accepted: references added
2-1883	A	53:51	53:51	give reference for these times [Christiane Textor]	Accepted
2-1884	A	53:53	53:53	replace "and sediment" by "that sediment" (syntax error) [Peter Van Velthoven]	Accepted
2-1885	A	53:56	53:56	Surely this is the place to mention the volcanic tropospheric sulphate aerosol and ash, which have lifetimes of a few weeks and must act to cool the surface (see Lamb's classic article) [Howard Roscoe]	Accepted
2-1886	A	53:56	55:36	This is a very nice analysis that is convincing and carefully written. [Jerry Mahlman]	Accepted: Thanks
2-1887	A	54:8	54:8	Change "at the low" to "at low" [Alan Robock]	Accepted
2-1888	A	54:9	54:9	Change "happens to be" to "is" [Alan Robock]	Accepted
2-2759	B	54:14	54:14	wavelengths [Olivier Boucher]	Accepted
2-1889	A	54:15	54:19	Not only does Ammann el al 2003 have higher OD values than Sato et al (1993) but also	Taken into account: discussion added

No.	Batch	Page:line From	To	Comment	Notes
				quite different spatial distributions. This is due to them using estimates of total aerosol loading for each eruption, from different sources, and allowing the distribution to be varied depending on the season (as stated in the text here), rather than using (where available) optical observational evidence about what the distribution is. They qualitatively evaluate their distributions of aerosol for each eruption and find a good correspondence apart from Agung (1963) and El Chichon (1982), where relatively good optical observations show quite different distributions (Sato et al 1993, R.B. Stothers, Major optical depth perturbations to the stratosphere from volcanic eruptions: Stellar extinction period, 1961-1978, Journal of Geophysical Research, 106, D3, 2993-3003, 2001.) This artifical distribution error is as an important issue as the uncertainty in the magnitude of global optical depth values. This should be considered and mentioned here. [Gareth S. Jones]	
2-1890	A	54:17		add: "a" in between "used" and "fixed" [Hartmut Grassl]	Accepted
2-1891	A	54:22		delete the comma after "Sato et al." [Hartmut Grassl]	Accepted
2-1892	A	54:27	54:27	Change "its" to "their" [Alan Robock]	Accepted
2-1893	A	54:30		delete the hyphen between "shorter" and "wave" [Hartmut Grassl]	Accepted
2-1894	A	54:31		"efforts" (?) [Hartmut Grassl]	Accepted
2-1895	A	54:34		"normalized them combining" (?) [Hartmut Grassl]	Accepted
2-1896	A	54:37		delete: "by way of" [Hartmut Grassl]	Accepted
2-1897	A	54:55		insert a comma before "e.g." [Hartmut Grassl]	Accepted
2-1898	A	54:56	54:56	Change "e.g." to ", e.g.," [Alan Robock]	Accepted
2-1899	A	55:1	55:7	Note that Ch. 9 ha a figure which shows all the AR4 models. [Joyce Penner]	Accepted
2-2760	B	55:5	55:5	Amman [Olivier Boucher]	Accepted: Should be " Ammann" everywhere
2-1900	A	55:11	55:11	Reference for ERBS Pinagtubo anomaly is Minnis, P., E. F. Harrison, L. L. Stowe, G. G. Gibson, F. M. Denn, D. R. Doelling, and W. L. Smith Jr., 1993: Radiative climate forcing	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				by the Mt. Pinatubo eruption. Science, 259, 1411-1415. [Patrick Minnis]	
2-1901	A	55:15	55:25	It is worth noting here, that if a model does not reasonably reproduce northern Polar night jet the observed winter warming cannot be reproduced. It was shown by Rozanov et al. (2002, JGR) by comparison of the volcanic effects in two versions of UIUC GCM, with and without reasonable PNJ. [Eugene Rozanov]	Taken intio account: It was previously discussed
2-1902	A	55:15	55:25	cite the work of Hans-F. Graf on the effect of volcanoes on stratospheric circulation, and its effects on the troposphere (possibly cite also the work of Judith Perlwitz) [Christiane Textor]	Accepted: References added
2-2761	B	55:21	55:22	Delete second occurrence of the references [Olivier Boucher]	Accepted
2-1903	A	55:21	55:22	delete: "(Yang … 2004)" [Hartmut Grassl]	Accepted
2-1904	A	55:21	55:22	Refs Yang/Schlesinger and Stenchikov appear twice [Reto Knutti]	Accepted
2-1905	A	55:21	55:22	"(Yang and Schlesinger, 2001; Stenchikov et al., 2004)." repeated twice. Remove one. [Alan Robock]	Accepted
2-1906	A	55:21	55:25	The sentences from 21 to 25 are repeated jut below. Please omit them. [Ramachandran Srikanthan]	Accepted
2-1907	A	55:22	55:25	Same sentence appears further down [Reto Knutti]	Accepted
2-1908	A	55:25	55:25	Thermal heating of the stratosphere due to the absorption of upwelling thermal radiation is an important aspect of the radiative effects of volcanic aerosols. The absorption of solar radiation is only a minor contributor to stratospheric heating - strtospheric heating by volcanic aerosols is almost entirely due to the absorption of upwelling thermal radiation (see Lacis and Mishchenko, 1995). This is the reason that the size of the volcanic aerosol is a critically important factor. Typically, the effective radius of volcanic aerosols is usually less than 0.5 microns. However, if the efective radius were as large as 2 microns, the radiative forcing of volcanic aerosols would be near zero. For particles larger than 2.2 microns, the volcanic aerosol greenhouse effects starts to dominate the solar albedo effect to produce surface warming instead of cooling. [Andrew Lacis]	Taken into account: Dissscussion added
2-1909	A	55:31		replace "2.3.6.1" by "2.3.7.1" (?) [Joanna Haigh]	Accepted
2-1910	A	55:33	55:35	delete the paragraph	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Hartmut Grassl]	
2-1911	A	55:33	55:35	This is a duplicate of lines 22-25 and should be removed. [Alan Robock]	Accepted
2-1912	A	55:33	55:36	This paragraph is identical to lines 22-25 [Eugene Rozanov]	Accepted
2-1913	A	55:38	55:39	I find this to be a bit off base. To me, a major volcanic eruption produces a forced growing, then decaying, climate perturbation, not a secular trend, as is stated here. [Jerry Mahlman]	Taken into account: Explained
2-1914	A	55:38	55:38	add " from explosive eruptions " [Christiane Textor]	Accepted
2-1915	A	55:41	55:41	Change "as the Mt." to "as Mt." [Alan Robock]	Accepted
2-1916	A	55:44		add a comma after "(2002)" [Hartmut Grassl]	Accepted
2-1917	A	55:48		add a semicolon after "2003" [Hartmut Grassl]	Accepted
2-1918	A	55:50	55:50	"considered" … this is a bit vague, given the certainty elsewhere about forcing/feedback distinctions! [Keith Shine]	Accepted
2-1919	A	55:54		add a comma after "simulation" [Hartmut Grassl]	Accepted
2-1920	A	55:56	55:56	Add ". Stenchikov et al., 2004" inside the parentheses. [Alan Robock]	Accepted
2-1921	A	56:5		add a comma after "2003" [Hartmut Grassl]	Accepted
2-1922	A	56:11	56:23	This is a nice analysis, although model ensembles and more carefully chosen diagnostics may be needed to pin this point down robustly. [Jerry Mahlman]	Noted
2-1923	A	56:11	56:23	cite the work of Hans-F. Graf on the effect of volcanoes on stratospheric circulation, and its effects on the troposphere (possibly cite also the work of Judith Perlwitz) [Christiane Textor]	Accepted
2-1924	A	56:12		correct: "surface troposphere" to "troposphere including the surface" [Hartmut Grassl]	Accepted
2-1925	A	56:18	56:23	"The dynamical response to the radiative perturbations can…., could contribute to the observed long-term positive trend of the AO." Can we insert a figure to show the	Rejected: Do not have room for that, unfortunately

No.	Batch	Page:line From	Page:line To	Comment	Notes
				observed long-term trend of AO? [Xiaobin Xu]	
2-1926	A	56:19	56:20	Winter warming is evident in observations and this is known since quite a while. See papers by Robock and co-workers [Fortunat Joos]	Accepted: Robock's paper is referenced above
2-1927	A	56:36		add "a" after "produce" [Hartmut Grassl]	Accepted
2-1928	A	56:41	56:56	section 2.7.2.2.3 comment: The influence of direct injection of water vapor by explosive volcanoes into the stratosphere and its influence on chemistry has been discussed. Has there been a recent paper? [Christiane Textor]	Noted
2-1929	A	56:41		section 2.7.2.2.3 comment: The influence of direct injection of water vapor by explosive volcanoes into the stratosphere and its influence on chemistry has been discussed. Has there been a recent paper? [Christiane Textor]	Noted: Direct injection of water by the eruption is usually not climatically significant
2-1930	A	56:42	56:44	Effects of volcanoes on stratopheric ozone also are linked to surface reactions involving nitrogen chemistry as well as chlorine, so a small wording change to reflect that would be helpful here. Among the references where you can find information on this is Solomon et al. (JGR, 1996), which doesn't need to be referenced but may be worth a look as you compose your text. [Susan Solomon]	Noted
2-1931	A	56:56	56:56	Textor et al JGR, 108, 19, 4606, doi:10.1029/2002JD002987, 2003 have shown with model simulations, that direct injectitons of halogens from explosive volcanic eruptions are possible. (Of course I find this highly relevant here, since it is my own work...) [Christiane Textor]	Noted
2-1932	A	57:1	57:23	This paragraph needs substantial clarification and extension. The following statements are not supported by any references and probably are wrong: (i) "…chemistry-climate studies generally show that aerosol-induced stratospheric heating affects the dispersion of aerosol cloud…"; (ii) "…all CCMs overestimate meridional transport in the stratosphere…"; (iii) " .. A simplified treatment of the aerosol microphysics …". As far as I am aware the aerosol is prescribed in all CCMs, therefore it is not clear where these conclusions came from. I would ask also add to the line 15: " Transient simulation with CCM SOCOL (Rozanov et al., 2005, ASR) driven by aerosol loading from GISS data set showed reasonable agreement in stratospheric temperature response in comparison with TOVS data and several reanalysis products, however the total ozone response looks overestimated in comparison with TOMS data. [Eugene Rozanov]	Taken into account: References and discussion added

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1933	A	57:8		correct: "use" to "used" [Hartmut Grassl]	Accepted
2-1934	A	57:11	57:11	Change "conducted transient" to "conducted a transient" [Alan Robock]	Accepted
2-1935	A	57:11		correct: "using couple a" to "using a coupled" [Hartmut Grassl]	Accepted
2-1936	A	57:13	57:13	Change "for Pinatubo" to "for the Pinatubo" [Alan Robock]	Accepted
2-1937	A	57:13	57:13	Change "effect" to "effects" [Alan Robock]	Accepted
2-1938	A	57:13		insert: "a" after reported [Hartmut Grassl]	Accepted
2-1939	A	57:15	57:15	Change "of volcanic radiative impact. The interactive" to "of the volcanic radiative impact. Interactive" [Alan Robock]	Accepted
2-1940	A	57:16	57:16	Change "of aerosol" to "of the aerosol" [Alan Robock]	Accepted
2-1941	A	57:17	57:18	I think that much of the "cure" or "the wrong prescription" for this model problem lies in the choice of advection schemes. See the paper by Eluszkiewicz, et al, Journal of the Atmospheric Sciences, 2000, 57(19): 3185-3201. that demonstrates how Semi-Lagrangian and parcel-in-cell advection schemes produce very apparent meridional spurious diffusion in numerical tests. For many applications, this is not a problem, but it is clearly applicable to analysis of this discussion. [Jerry Mahlman]	Taken into account: References and discussion added
2-1942	A	57:25		I thought that statospheric adjusted RF is what we have been comparing? Why do we need to define Fa now? [Robert Levy]	Accepted. Defined earlier
2-1943	A	57:27	57:29	It would perhaps be more accurate to state that adjusted RF is a useful concept for comparing the relative impact of different radiative forcing mechanisms on the global mean temperature. There certainly are impacts other than changes in global mean surface temperature that are of interest. It is of course possible, but scarcely seems likely, that all other climate parameters of interest will change in lock step with the global mean temperature. [Andrew Lacis]	Accepted
2-1944	A	57:30	57:32	Actually, this "quasi-constant" factor is where all of the 1.5 to 4.5 C uncertainty resides, as examplified in the well publicized climate response estimate to doubled CO2. All of	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				the modeling uncertainties related to climatic feedback processes are contained in the parameter. The no-feedback response (the Hansen et al. (1984) Delta-T-zero) to doubled CO2 is probably somewhere in the range between 1.2 to 1.3 C. Its only real sources of error are radiative transfer related approximations, so it is probably accurate to within 10%. The rest of the global mean temperature response is due to the sum total of the different feedback process contributions which few would claim to have an accuracy that is within 25%. [Andrew Lacis]	
2-1945	A	57:30	57:31	I agree that this linearity argument makes sense. [Jerry Mahlman]	Noted
2-1946	A	57:30	57:32	This statement is not true for the evaluation of the aerosol indirect effect by Rotstayn and Penner (J. Clim., 2001). They found an indirect forcing (first only) of -1.35 W/m2, which, when operating in their model together with the 2nd indirect effect produced a temperature change of -2.24 degrees, which gives a climate sensitivity parameter of 1.67 whereas if you count the 2nd indirect forcing as a forcing and not a response, the climate sensitivity parameter remained within +/- 25%. In fact, this was one reason that they considered it would be better to include this as a forcing rather than a response. I suspect other models will also be larger than 25% if the 2nd indirect effect is not considered a forcing. [Joyce Penner]	Accepted – clairfiaction added
2-1947	A	57:32	57:36	The 25% uncertainty is more applicable to the Delta-T-zero estimates for different GHGs, or to total surface temperature responses, but only if model feedbacks are not a factor (e.g., such simulations conducted with only a single GCM). The efficacy parameters discussed in 2.8.5 are designed to make some sense out of the differing efficiencies of different radiative forcings. But there certainly are some pathological radiative forcing examples (e.g., near surface ozone and absorbing aerosols in Hansen et al., 1997) where the model response is readily understood in terms of the physical processes involved, but which clearly are at variance with the simple relationships expected from 1D model experience. [Andrew Lacis]	Noted
2-1948	A	57:32	57:34	I agree that this linearity argument makes sense. [Jerry Mahlman]	Noted
2-1949	A	57:35	57:	I would say RF is perfect for GHG, however for aerosol effects (the manuscript shows how difficult is the definition of say indirect RFs) it could be much easier to run a GCM and compare global mean temperature responses. [Eugene Rozanov]	Noted
2-1950	A	57:39		This Section also discusses several …	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Jerry Mahlman]	
2-1951	A	57:45	57:46	This is quite obscure. [Jerry Mahlman]	Accepted - reworded
2-1952	A	57:50	58:5	A fairly detailed and thorough analysis of the sensitivity of climate response to vertically dependent radiative forcings is given by Hansen et al. (1997). The Hansen et al. study includes the sensitivity of radiative forcing by cloud changes, ozone, and aerosols as a function of height, including the contributions made by height dependent feedback effects. Hansen et al. (1997) also perform a "ghost" forcing analysis whereby specified amounts of radiative forcing (W/m2, but without spectral structure) are introduced at different points in the atmosphere, or at the ground surface, to study climate response and sensitivity in terms of the model's instantaneous, adjusted, Delta-T-zero, fixed cloud, and full feedback responses. [Andrew Lacis]	Noted
2-2762	B	57:52	57:54	My hint is that surface RF might be a more appropriate measure for the regional climate response but not necessarily for the global response, especially for surface temperature (maybe not for evaporation). [Olivier Boucher]	Noted
2-1953	A	57:54		delete: "components due to" [Hartmut Grassl]	Accepted
2-1954	A	58:7	58:17	This is a useful section and I would like to suggest that adding some further discussion on differences in the latitude gradients between e.g., well mixed GHG versus stratospheric ozone, tropospheric ozone, and aerosols would be extremely important. This would help the document since some later chapters consider for example possible links of changes in the NAM and SAM to ozone and volcanic forcing, as well as possible effects of aerosols on NH/SH temperature contrasts. The current figure shows the gradients in aerosol forcing but because these are so large all other effects are washed out. I wonder if a different color scale on the forcing latitude/longitude distribution contour plot would be helpful (maybe a log scale?) [Susan Solomon]	Accepted – text added
2-1955	A	58:9	58:17	Hansen et al. (1997) provide a detailed comparison of the height and spatial patterns of the radiative forcing (both instantaneous and adjusted) and climate response (with fixed clouds and full feedback) for doubled CO2 and 2% solar irradiance change. Hansen et al. (1997) also provide a pertinent example of a combined CFC increase and tropopause-region ozone decrease radiative forcing, such that the combined global mean adjusted forcing adds to zero. The classical expectation for this example is no global temperature change since the adjusted forcing (and Delta-T-zero) is zero. However, the climate model response is a relatively small negative global mean temperature change if model clouds	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				and kept fixed. But there is a strong positive increase in global mean temperature when full model feedbacks are allowed to operate - a clear demonstration of the practical limitations to the assumed linearity of radiative forcings. [Andrew Lacis]	
2-1956	A	58:21	58:34	For small changes about a reference point, nearly any variability can be made to appear linear. Except for possibly the CFCs, the radiative forcing for all of the principal GHGs are strongly non-linear (see for example the analytic formulas fitted to represent the radiative forcing as a function of changing amounts of CO2, CH4, N2O given by Hansen et al., 1988). It is well understood that everything in radiative transfer, except in the extreme optically thin limit, is non-linear - including the Planck function. The fact that some GCMs find no evidence of non-linearity is probably a clear indication of over-simplified physics. It has to be recognized that all feedback processes interact non-linearly. Obviously, it makes the analysis of comparing radiative forcings and climate responses much more convenient if they can be treated as being linear. More often than not, the climate changes of interest are small enough for the linear approximation to be reasonable, but that is not always the case. Note for example the +/- 2% solar flux and CFC + O3 experiments described by Hansen et al. (1997). [Andrew Lacis]	Noted. Text already says this
2-1957	A	58:21	58:23	But this misses the important point for policy makers and engineers. Policy makers and engineers can only act on future effects. Therefore they need the marginal impacts quantified. It is less important what the current cumulative CO2 RF is (although I recognize this is VERY important for attributing currently measured climate change to past human actions and for understanding the relationship between human actions and climate). Policy makers and engineers need to know what happens if they reduce CO2 (e.g. what is a unit of CO2 reduction worth compared to a unit of NOx/ozone reduction?). That is what is required for a cost-benefit analysis of options for mitigating climate change. [Ian Waitz]	Accepted
2-1958	A	58:24	58:25	See statement above. If you consider the 2nd indirect effect as a response the system is highly non-linear. [Joyce Penner]	Accepted - reworded
2-1959	A	58:25	58:25	One of the most detailled studies with combinations of different forcings is Meehl et al. 2004 J. Climate [Reto Knutti]	Accepted – reference added
2-1960	A	58:25	58:25	Sexton et al use fixed SST's so I am not sure it is entirely a legitimate reference in this context. [Keith Shine]	Accepted – reference dropped

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1961	A	58:27	58:30	why "with respect of emissions"? According to Feichter et al the nonlinearity is due to interactions between the aerosol and the water cycle so that changes in precipitation due to the 2nd indirect effect and due to changes in surface energy budget change distribution and residence time of aerosols in the atmosphere. See in the Conlusions " A strong dependency of the response to the type of forcing has also been found for the total cloud water content and, consequently, for the change in net cloud radiative forcing, which is substantially larger in the combined forcing experiment (21.5 W m2) than in either of the other experiments (20.1 W m2) in GHG and 20.3 W m2 in AP). Therefore, the global warming in GHG&AP is significantly smaller (0.57 K) than that obtained by adding the individual changes (0.85 K)." [Johann Feichter]	Accepted – text clarified
2-1962	A	58:30	58:32	I agree with this conclusion. [Jerry Mahlman]	noted
2-1963	A	58:30	58:34	Actually there is a much longer history here … the nuclear winter studies during the 80's (e.g. a paper by Cess, and (a great!) one by Ramaswamy and Kiehl show that even in radiative convective models you get this non-linearity - i.e. a highly absorbing aerosol (hence with a strong positive radiative forcing) causing a cooling. The reason why is that the tropopause was essentially brought down to the surface by the soot. [Keith Shine]	Noted
2-1964	A	58:31		insert: "to" after "due" [Hartmut Grassl]	accepted
2-1965	A	58:32	58:34	So I could misinterpet this then to say that if the RF from contrails=the RF from CO2 for aviation then the benefit of reducing contrails= the benefit of reducing CO2. But this neglects the fact that the RF from CO2 is due to cumulative emissions (and further that it lasts a long time), etc.... [Ian Waitz]	Accepted – text reworded for clairty
2-1966	A	58:33	58:34	In light of my comment #36, I don't understand your high confidence. [Joyce Penner]	Rejected, better context provided
2-1967	A	58:33	58:33	Correct the sentence "RFs discussed …" [Ramachandran Srikanthan]	accpeted
2-1968	A	58:33	58:33	RFs discussed it this chapter…" -> "RFs discussed in this chapter… [Xiaobin Xu]	accepted
2-1969	A	58:33		It is not clear that the question of linearity or otherwise can be answered by a GCM, since these necessarily cannot capture slow response nonlinear interactions (feedbacks) that may determine the answer. Linearity is just a reasonable hypothesis that is extremely difficult to prove. Suggest replacing 'high confidence of a linear' with 'no evidence so far	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				of a nonlinear'. [Roger Davies]	
2-1970	A	58:33		correct: "it" to "in" after "discussed" [Hartmut Grassl]	accepted
2-1971	A	58:36	59:14	This chapter is very difficult to understand except for the few people that were involved in the debate. I would suggest at least to move Figure 2.2.2 here so that the reader remembers what Fg,Fa, etc means. [Cathy Clerbaux]	Rejected, but clarified
2-1972	A	58:36		Section 2.8.4. I fear the reader is going to be rather confused. Although Fg, Fs etc were portrayed back in Figure 2.2.2 they were not described and it has not been explained how they are calculated. Furthermore, if, as is stated, Fg is a better bet than Fa then why not give values of Fg (as well as or) instead of Fa? As you need to run a GCM to calculate Fg doesn't it rather damage the whole concept of radiative forcing as a simple method to indicate climate response? Some immediate questions: what is the value of Fg for CO2? how different is the climate sensitivity parameter for CO2 if Fg is used instead of Fa? how different are the efficacies if they were based on Fg instead of Fa? I suggest (politely!) that, as it is not used anywhere in the report, you consider dropping all reference to Fg until a much clearer picture can be presented. [Joanna Haigh]	Accepted – text clarified
2-1973	A	58:38	58:43	The objective for computing radiative forcings is to obtain an estimate of the climate system response to some specified disturbance to the existing climate equilibrium. "Instantaneous forcing" for a standard clear-sky atmospshere is an important form of radiative forcing that is simple to obtain and allows direct comparison between different radiation models. It has the advantage of having a reliable standard of reference in line-by-line calculations. Its limitation in predicting a surface temperature response stems from differences that arise from a rapid response of the stratosphere - hence, "adjusted forcing", which allows the stratospheric temperature to equilibrate, so that CFC forcing (with little stratospheric cooling) and CO2 forcing (strong stratospheric cooling) can be placed on the same footing. With adjusted forcing, there is still the problem that 1 W/m2 of forcing in the tropics is not equivalent to 1/W/m2 in polar regions, simply because the Planck radiation is different at the two locations. [Andrew Lacis]	Noted
2-1974	A	58:44	58:47	The Planck temperature dependence problem is resolved by computing Delta-T-zero forcings which allow both the atmosphere and ground to come to thermal equilibrium, but with no feedback processes operating. This eliminates differing sensitivities to temperature either due to latitude, atmospheric height, or spectral region of the forcing. All of these "radiative forcing" calculations are relatively easy to perform off-line with a	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				1-D radiative convective model with a high degree of precision. The inherent limitation of all these off-line radiative forcing calculations is inability to account for realistic variation in cloudiness, convection, topography, water vapor and temperature profiles. Thus, rather than using off-line proxies, it is clearly preferable to compute the radiative forcing quantities directly with a GCM. But computing radiative forcings as a direct GCM diagnostic is not straight forward and brings on a new set of problems. [Andrew Lacis]	
2-1975	A	58:44	58:44	Suggest adding (Fa) after "standard RF definition" to help readers who may be reading this part of the chapter without having read the earlier parts. [Leon Rotstayn]	Formula dropped
2-1976	A	58:46	58:46	see also figure 2.2.2. Fg, fixed global temperature forcing, is not explained sufficiently in the text. What does it mean - are land surface temperatures completely fixed? [Peter Van Velthoven]	Formula dropped
2-1977	A	58:47	58:47	Questions to be addressed include: Where in the GCM is the tropopause? For instantaneous forcing, what day of the year is to be used, or is it more appropriate to perform an annual average? How are the effects of natural (unforced) variability accounted for? With these questions in mind, computing the adjusted radiative frocing, the Delta-T-zero forcing, or any of the other radiative forcing formulations that have been devised, a significant amount of effort is required in order to extract them from the GCM since many different physical processes have to be individually constrained or shut down. (Radiative forcing are not a natural GCM diagnostic output.) The different radiative forcing formulations are useful in that they help to identify key differences between different forcings and their response to the model's fast feedback process interactions. To zeroth order, radiative forcings may be treated as linear and interchangeable, but as improved understanding of climate feedback processes contributions are included, the interactions and responses to different forcings become more complex. [Andrew Lacis]	Noted
2-1978	A	58:48	59:5	It might be worth elaborating that the method from Shine et al (2003) was implemented in an intermediate GCM, but it is not clear how to implement it in a full GCM that has a diurnal cycle of radiation (e.g., how do you hold the land-surface temperature "fixed" without doing horrible things to the land-surface scheme?) In contrast, Hansen's method of computing Fg is very simple, provided that one has access to an estimate of the equilibrium climate sensitivity of the GCM (which will usually be available, since all that is required is a first-order estimate). In my opinion, Hansen's method should become the standard method for calculating estimates of the indirect aerosol effects in GCMs, since it corrects for the errors induced by using the difference of two runs with prescribed SSTs (such as the change in land-surface temperature). It is therefore beneficial to emphasize	Briefly alluded to in revison

No.	Batch	Page:line From	Page:line To	Comment	Notes
				the simplicity of Hansen's method. [Leon Rotstayn]	
2-1979	A	58:51		Efficacies not defined until the next section. [Joanna Haigh]	Accepted -
2-1980	A	58:53	58:57	The logic in these sentences seems off. [Joyce Penner]	Text clarified
2-1981	A	58:54		correct: "has" to "have" [Hartmut Grassl]	accepted
2-2763	B	58:57	58:57	in a different way [Olivier Boucher]	accepted
2-1982	A	58:57	59:1	The description as given is unclear. [Andrew Lacis]	accepted
2-1983	A	59:0	62:	To give a more balanced view, discuss the downside of efficacy. I think that it is essential to say the efficacy is model derived. Also, does this concept work for regional and local forcing? How can one compare CO2 with a highly variable forcing, like ozone or aerosols? Lastly, this works only for the greenhouse effect component, not the interaction with the incoming solar. These factors are not clearly presented [A. R. Ravishankara]	accepted
2-1984	A	59:0		Effective RF = Fe = FE is really confusing. At least call it Fsubscript-e. [Robert Levy]	Formula dropped
2-2764	B	59:1	59:1	there -> their [Olivier Boucher]	Accepted - reworded
2-1985	A	59:1	59:1	Change "there" to "their" [Patrick Hamill]	Accepted - reworded
2-1986	A	59:1	59:1	Change "into there surface and atmospheric only components" to "into surface components and atmospheric components". [Brian Magi]	Accepted - reworded
2-1987	A	59:1	59:1	PROOFREADING TYPE COMMENT: Substitute "...feedbacks into there surface..." with "...feedbacks into the surface..." [Malte Meinshausen]	Accepted - reworded
2-1988	A	59:1	59:1	Correct the sentence "feedbacks into there …" [Ramachandran Srikanthan]	Accepted - reworded
2-1989	A	59:1	59:1	feedbacks into there surface and atmospheric only components…." -> "feedbacks into surface and atmospheric only components…. [Xiaobin Xu]	Accepted - reworded
2-1990	A	59:1		delete: "there"	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Hartmut Grassl]	
2-1991	A	59:1		correct: "atmospheric" to "atmosphere" [Hartmut Grassl]	Accepted
2-1992	A	59:1		replace "there" by "their" [Joanna Haigh]	Accepted
2-1993	A	59:5	59:5	The efficacy of climate forcings also depends on the time scale over which they are allowed to operate (Hansen et al., 2005). [Andrew Lacis]	Accepted – text added
2-1994	A	59:7	59:7	"more representative" -> "better predictor" … and with the important caveat that this is the case in models! We don't really know about reality. [Keith Shine]	Accepted - reworded
2-2765	B	59:11	59:12	…a GCM response … that it is readily … [Olivier Boucher]	Accepted – reworded
2-1995	A	59:12		insert: "it" after "that" [Hartmut Grassl]	Accepted – reworded
2-1996	A	59:15		The concept of efficacy is new and this is clear in the summary of chapter 2 - it should also be clear here, close to the definition [MARCOS S. P. GOMES]	Accepted - reworded
2-1997	A	59:15		Section 2.8.5. The main conclusion that can be drawn from the dicussion of Efficacies is that the accuracy that can be achieved by use of a climate sensitivy/radiative forcing model is limited. Efficacies can not be used to assess the potential effect of any new agent in a given GCM but just (perhaps) the relative effect of an existing agent in a different GCM. This all seems like a lot of effort to force the physics into a model which was (is) useful as a zeroth order estimator but has reached the limits of its applicability. I suggest that, given rapidly increasing availability of CPU time, efforts might be better spent on more GCM runs and on work (with simpler models) aimed at understanding the underlying physical mechanisms. This comment is not intended as destructive - I am trying to see it from the point of view of a reader who is interested in the processes of climate change rather than esoteric discussions about empirical models. [Joanna Haigh]	Noted
2-1998	A	59:15		2.8.5 The role of charged particles in forming aerosols has yet to be determined and hence needs to be considered in 2.9.2 also. P67, line 37 applies [Alan Rodger]	Rejected – not meaningful here
2-1999	A	59:15		Section 2.8.5. No reference is given to the concept of global warming potential (GWP) in this section of the draft text . There are clear differences between the concepts of climate efficacy and GWP but those are not emphasized in the text. The concept of GWP is	GWPs not relevent here

No.	Batch	Page:line From	Page:line To	Comment	Notes
				relatively well known and the concept of climate efficacy is less known. I would suggest the addition of an explicit paragraph describing the differences between these two concepts and their respective purposes. [Philippe Tulkens]	
2-2000	A	59:17	59:18	A fairly detailed description and analysis of the efficacy of different climate forcings is presented by Hansen et al. (2005). [Andrew Lacis]	Noted
2-2001	A	59:17	59:56	The "efficacy" approach will likely prove to be a valuable diagnostic, but it almost guaranteed to be rather opaque to the non hard-core scientist reader or user. I suggest that its use here in this assessment be offered cautiously. [Jerry Mahlman]	Accepted - reworded
2-2002	A	59:17	59:18	The concept of climate efficacy is referred to and two articles are cited for the definition. The definition could be given in the IPCC report for the sake of clarity. [Philippe Tulkens]	Accepted – defined here
2-2003	A	59:18	59:18	…Nazarenko, 2004). Preliminary … [Rolf Philipona]	accepted
2-2004	A	59:18		a full stop is missing after "2004)" [Hartmut Grassl]	accepted
2-2005	A	59:25		delete: "you" in "give you the effective" [Hartmut Grassl]	accepted
2-2006	A	59:27		insert: "the" before "mechanism" [Hartmut Grassl]	Accepted - reworded
2-2007	A	59:41	59:43	The spectral structure of the radiative forcing may also affect the interaction of the radiative forcing with different feedback mechanisms. [Andrew Lacis]	Noted
2-2008	A	59:54	59:54	Correct the sentence "have smaller efficacies that …" [Ramachandran Srikanthan]	Accepted - reworded
2-2009	A	59:54	59:54	...to have smaller efficacies that those that affect" -> "…to have smaller efficacies than those that affect [Xiaobin Xu]	Accepted - reworded
2-2010	A	59:54		correct: "that" to "than" [Hartmut Grassl]	Accepted - reworded
2-2766	B	60:1	60:9	Govindasamy et al. 2001 : 2001a or 2001b (can you update reference list as well) [Olivier Boucher]	Accepted - reworded
2-2011	A	60:1		correct: "it" to "It" [Hartmut Grassl]	Accepted - reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2012	A	60:6	60:15	Diferent assumed vertical profiles for CFCs, CH4, CO2 may also be the source of some differences in radiative forcings and efficacies that are attributed to these GHGs. [Andrew Lacis]	Noted
2-2013	A	60:7	60:10	Which NCAR model? The later version of the NCAR has successfully addressed its earlier surprisingly low climate sensitivity problem. I assume here that these calculations have used this later more physically plausible NCAR model. [Jerry Mahlman]	Model added
2-2014	A	60:8	60:8	Reason why solar "efficacy" should be less than IR "efficacy" is completely unclear. [Stephen McIntyre]	Text clarified
2-2015	A	60:8		delete: "B" at the end of the line [Hartmut Grassl]	Text clarified
2-2016	A	60:17		Section 2.8.5.3. Will Stott et al (2003, Do models underestimate the solar contribution to recent climate change?, J.Clim., 16, 4079-4093) be mentioned in the Detection & Attribution chapter? I think their conclusion is consistent with what is presented here if the (model-derived) efficacy is too low compared with the true atmospheric response. [Joanna Haigh]	Citation added
2-2017	A	60:18	60:26	The solar efficacy range of 0.75 - 1.0 seems to be rather large for what might be considered to be the simplest of all forcings. Are albedo feedbacks and ozone interactions to blame? [Andrew Lacis]	No
2-2767	B	60:22	60:24	Isn't it better in the end to use a fully coupled model? [Olivier Boucher]	Granted
2-2018	A	60:24		by "direct solar RF" I presume you mean "direct forcing by TSI" as stratospheric (UV) effects should be incuded in Fa, not counted as "indirect". [Joanna Haigh]	Accepted
2-2019	A	60:29	60:29	replace "pertubations" by "increases" otherwise the next sentence (higher tropical tropopause temperatures) doesn't make sense [Keith Shine]	Text clarified
2-2768	B	60:37	60:37	an --> and [Olivier Boucher]	accepted
2-2020	A	60:37	60:37	Correct the sentence "performed experiments …" [Ramachandran Srikanthan]	accepted
2-2021	A	60:37	60:37	…ozone changes throughout the atmosphere an in the troposphere separately," -> "…ozone changes throughout the atmosphere and in the troposphere separately, [Xiaobin Xu]	accepted

No.	Batch	Page:line From	To	Comment	Notes
2-2022	A	60:37		correct: "an" to "and" [Hartmut Grassl]	accepted
2-2023	A	60:39	60:39	.As this is only one model,generic conclusions are…" -> ".As this is result from only one model, generic conclusions are… [Xiaobin Xu]	accepted
2-2769	B	61:2	61:4	This is inconsistent with previous statement that the cloud lifetime effect was included in the efficacy of the cloud albedo effect. The cloud lifetime effect could double the efficacy of the cloud albedo effect. [Olivier Boucher]	Setence reworded -see Penner comments
2-2024	A	61:7	61:12	A negative efficacy? Is there a simple, but plausible physical interpretation of such a thing? [Jerry Mahlman]	Yes. Text clarified as to physical meaning
2-2025	A	61:7	61:27	The ratio of temperature change to forcing for fossil fuel carbon aerosols, biomass aerosols, and sulfate aerosols in the model described by Penner et al., 2005 is .0029, 3.714, and 0.866 (with the latter similar to the sensitivity parameter for this model to doubling of CO2). This efficicacy is very different from 0.7!! (I sent this paper to the co-ordinating lead authors last May): Penner, J.E., M. Wang, A. Kumar, L. Rotstayn, and B. Santer, 2005: Effect of Black Carbon on Mid-Troposphere and Surface Temperature Trends, in Human-Induced Climate Change: An Interdisciplinary Assessment, ed. by M. Schlesinger, M. Schlesinger, H. Kheshgi, J. Smith, F. de la Chesnaye, J. Reilly, C. Kolstad, and T. Wilson, Cambridge University Press, in press. [Joyce Penner]	Accepted – reference added
2-2770	B	61:9	61:10	Do you have a supporting reference for this statement? I understand that a positive RF can induce a global mean cooling (because heating occurs in the atmosphere at the expense of the surface. But I can't see how the opposite (negative RF and a global-mean warming) can be achieved by adding aerosols (you can by removing absorbing aerosols for sure). [Olivier Boucher]	No – text reworded to better explain meaning
2-2026	A	61:9	61:11	comment: not substantiated ? [Hartmut Grassl]	accepeted
2-2027	A	61:29	61:41	My "efficacy confusion" grows. Can one go to efficacy school? [Jerry Mahlman]	Yes – but you need to pay me
2-2028	A	61:34		delete: one "that" [Hartmut Grassl]	accepeted
2-2771	B	61:36	61:36	a efficacy --> an efficacy [Olivier Boucher]	accepeted
2-2772	B	61:36	61:36	ponater et al 2005 --> (2005)	accepeted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Olivier Boucher]	
2-2029	A	61:36		put "2005" in brackets [Hartmut Grassl]	accepeted
2-2773	B	61:37	61:39	I am not sure you can really say that from our study. The experiments were for fixed SSTs so energy was not conserved. Locally the evaporative cooling at the surface dominated the GHG effect but we cannot really say globally. [Olivier Boucher]	Accepted – reference dropped
2-2030	A	61:41	61:41	"and Shine" ;-) [Keith Shine]	accepted
2-2031	A	61:43		The discussions on RF and improvements made since the Third Assessment Report are very good. However, section 2.8.6 lacks useful advice suitable for policy makers. Suggest including some guidance on what information can and cannot be derived from the use of RF, and the potential implications to policy makers, in this section. [Lourdes Maurice]	Accepted
2-2032	A	61:45	61:49	RFs may be within a factor of two. However, is this also the case for efficacies? For instance the climate sensitivity against CO2 as calculated by different models ranges between 0.9 - 2.3. This is more than a factor of two. (2xCO2 forcing = 2.2 W/m2, temperature response ranges between 2 - 5 K). [Johann Feichter]	Accepted – text clarified
2-2033	A	61:46	61:48	Does this imply that the global mean temperature prediction to within 25% leads to the conclusion that the climate sensitivity to doubled CO2 is now known to be 2.7 +/- 0.7 C? What exactly does the factor of 2 for any realistic RF refer to? [Andrew Lacis]	Accepted – text clarified
2-2034	A	61:49	61:49	replace "than" by "from" [Peter Van Velthoven]	accepeted
2-2035	A	61:51	61:56	I like the concept of 'first-response' mechanisms, but it is not well defined. This concept could be very useful in the future, so I encourage the authors to provide a clear definition, perhaps with examples. [Tami Bond]	Defintion tightend in first section
2-2036	A	61:54	62:3	Instantaneous forcing Fi might be termed as the "no-response" forcing, while Fa, the "first response" forcing includes the response of the fastest responding reaction (stratospheric temperature) of the climate system to a radiative perturbation. The aerosol semi-direct effect should rightfully be a model feedback - to the extent that the model's cloud prediction scheme responds to local temperature changes. The cloud life time and Twomey effects would likewise be model feedbacks to the extent that the model includes a response to changes in cloud microphysics. In the absence of such physics, these	Noted -

No.	Batch	Page:line From	Page:line To	Comment	Notes
				processes may be formulated in terms of some prameterized "forcing" that is applied to the cloud field in response to prescribed aerosol changes. [Andrew Lacis]	
2-2037	A	62:11	62:12	I do not believe it is true that "aerosol-cloud lifetime interactions are not typically modelled by GCMs". Many GCMs have included these effects for warm clouds, and even Table 2.4.6 shows this. This is one of many instances in Chapter 2 where the significance of the cloud-lifetime effect is downplayed. [Leon Rotstayn]	accepted
2-2038	A	62:12	62:14	I see now that perhaps all of the above discussion was not meant to discuss the cloud lifetime effect. Nevertheless, you seem to have tried to completely duck this issue and given a false impression in the above. Also, your previous interpretation of the Lohmann Feichter non-linearity as being due to changes in aerosol burden may simply be because you have not considered the 2nd indirect effect. [Joyce Penner]	accepted
2-2039	A	62:16		Section 2.9: This section contains some new and very useful ways of presenting the various RF estimates. But to avoid confusion and misunderstandings there is a need for a somewhat more thorough explanation of the various perspectives. In particular I'm thinking about the backward vs forward looking perspectives. In addition, the AIRF has now been introduced, and all these ways of presenting RF may confuse the readers. The "standard IPCC RF bar" which gives the current RF from pre-industrial times is often misunderstood by lay persons and even also by persons working in this field. A better explanation of what determines the current RF (i.e. the historical development of the emissions for the long lived gases) would be very helpful. It is important to explain that this can be related to i) atmospheric abundances or (which is new here) ii) to the emissions (or "drivers"). Finally, when GWPs are applied as weights to current emissions, it should be emphasized that this shows the future effects of current emissions (i.e. for one year), and that RF is integrated over a chosen time horizon. I think this picture of man-made RF (i.e. GWPxEmission) could be given more attention as it builds on the current emissions, and not the history of emissions and concentrations. [Jan Fuglestvedt]	Accepted. Statement to this effect added
2-2040	A	62:18	62:21	It should be made abundantly clear that RF refers to the radiative forcing for a clearly specified amount of any particular radiative constituent. There are several statements in the Chapter that describe RF as meaning "anthropogenic" forcing - in which case, it would not be possible to talk about "RF" for solar flux or volcanic aerosol changes. If some fraction of dust or sulfate is to be attributed to anthropogenic causes, it should then be explicitly labeled as anthropogenic radiative forcing (ARF) to avoid any misunderstanding as to what is, or is not, included in any particular RF value. Relevant	Accepted. Stement to this effect added

No.	Batch	Page:line From	Page:line To	Comment	Notes
				remote sensing measurements don't differentiate between such categories since everything in the atmosphere gets measured. The resulting ambiguity unnecessarily compromises the confidence one might otherwise be able to attach to a given radiative forcing. While may be of interest to split off an attributable ARF number, there are large uncertainties as to what fraction of a particular forcing is "anthropogenic" and what fraction is "natural". The basis on how that distinction is arrived at is of interest. [Andrew Lacis]	
2-2774	B	62:25	62:25	discussed in this chapter [Olivier Boucher]	accpeted
2-2041	A	62:25	62:43	Readers may question why stratospheric and tropospheric ozone forcing are being combined in Figure 2.9.1. Since they are due to entirely different processes and emissions, many will want to see them separately in this figure. [Susan Solomon]	Forcings now split
2-2042	A	62:25	:28	The RFs discussed this chapter, their uncertainty ranges, and efficacies are summarized in Figure 2.9.1 and Table 2.9.1. RFs from forcing agents have been combined into their main groupings. This is particularly useful for aerosol as its total direct RF is considerably better constrained than the RF from individual aerosol types (Section 2.4.5.7). Table 2.3.1 gives a further component breakdown of RF for the LLGHGs.See comments on Page 2-36, lines 2-3, above. [Stephen E Schwartz]	Quoted text – no comment
2-2043	A	62:30	62:30	Is the efficacy included in this figure? [Keith Shine]	No –statemnt added
2-2044	A	62:34	62:43	This is a nice, but conceptually challenging, summary. But, but why is the "indirect cloud effect" given so much weight in Fig. 2.9.1? What is the evidence for this? I couldn't locate any clear evidence for this in the text. [Jerry Mahlman]	Satatemnt added
2-2045	A	62:34	:43	General comments on the following paragraph: In TAR because of a) uncertainties in the RFs, b) the uncertainty in the linear additivity assumption, and c) the uncertainty of efficacies, the various RFs from the different mechanisms were not added. Many of the limitations discussed in Ramaswamy et al. (2001) still apply. However, efficacies are now better understood and quantified (see Section 2.8.5). Secondly the linear-additivity assumption has been more thoroughly tested (Section 2.8.3). Thirdly the uncertainties in the direct aerosol and cloud-albedo aerosol RFs are substantially reduced. However, it should still be noted that the caveats discussed in Section 2.8 apply. Adding the RF values shown in the upper panel of Figure 2.9.1 and combining individual uncertainties results in the probability density function of RF shown in the bottom panel of Figure 2.9.1 (different efficacies are not accounted for). This	Quoted text , no coment

No.	Batch	Page:line From	Page:line To	Comment	Notes
				summation gives a combined anthropogenic RF of 1.5 ± 1.0 W m –2 , which implies that it is very likely that humans have had a net warming effect on climate. [Stephen E Schwartz]	
2-2046	A	62:34		I commend the present document for explicitly summing the forcings and propagating the uncertainty in the total forcing. The summation of forcings has been advocated previously (and resisted by prior IPCC assessment panels); it might therefore be appropriate to explicitly acknowledge papers which carried out such summation and assessed the consequences with respect to understanding of total radiative forcing over the industrial period. Schwartz, S. E. and Andreae, M O. Uncertainty in climate change caused by aerosols. Science 272, 1121-1122 (1996). Boucher O. and Haywood J. (2001) On summing the components of radiative forcing of climate change. Climate Dynamics 18, 297-302. Schwartz S. E., Uncertainty requirements in radiative forcing of climate change. J. Air Waste Management Assoc. 54, 1351-1359 (2004). That said, the qualifying adjective "linear" in phrases such as "linear additivity" (line 34) should be struck, unless the authors have invented a new type of addition that is nonlinear. [Stephen E Schwartz]	Reference added
2-2047	A	62:42	62:43	Could the forcing that are not included in the figure change this important conclusion? Could a limit be provided on the magnitude of the other forcings? Need some additional discussion on forcings that are not include to support conclusion. [Fortunat Joos]	Accepted Statemnt added
2-2048	A	62:42	:43	Explicit comments on the following sentence: This summation gives a combined anthropogenic RF of 1.5 ± 1.0 W m –2 , which implies that it is very likely that humans have had a net warming effect on climate. The report should take the above statement of uncertainty range one or two steps further. Namely that the forcing given by these uncertainties ranges from 0.5 to 2.5 W m-2, that is a factor of 5 in forcing. While it might be hard to give a rigorous statement of what is meant by the ± in the above uncertainty, some words seem required. Is this one sigma? two sigma? That can make quite a difference. Examination of Figure 2.9.1 suggests that ± 1.0 W m-2 is 2 sigma (ordinate value ~ 0.14). On the other hand the caption to Figure 2.91 implies 90% confidence interval. That would correspond to 1.65 sigma and an ordinate value of ~0.25, which corresponds to an uncertainty range of 0.83 to 2.3 W m-2 or a factor of 3.4.But page 2-29 line 36 says ± 1 standard deviation. So these discussions need to be made consistent.The high sensitivity of the resultant factor uncertainty to the confidence	Text added to exaplin uncertaniy and sentences reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
				interval chosen suggests that much more explicit attention needs to be given to specifying what is meant by the ± and to the recommended value of the uncertainty.The recommended uncertainty should be compared to the required accuracy; see comment above re pages 2-3. Further points: The finding is that humans (better human activities) have had a net warming influence on forcing, not a net warming effect on climate. Again, the subject of this chapter is forcing, not response. It should thus be worded (changes in italics): This summation gives a combined anthropogenic RF of 1.5 ± 1.0 W m –2 , which implies that it is very likely that humans have exerted a net warming influence on climate. It should be further qualified to time period: This summation gives a combined anthropogenic RF of 1.5 ± 1.0 W m –2 , which implies that it is very likely that humans have exerted a net warming influence on climate over the time period ~1800 to present. Even better, human activities, not humans; humans aren't doing the forcing--it is changes in atmospheric composition that are due to human activities: This summation gives a combined anthropogenic RF of 1.5 ± 1.0 W m –2 , which implies that it is very likely that human activities have exerted a net warming influence on climate over the time period ~1800 to present. Note: the use of "human activities" rather than "humans" is consistent with the language of Question 1, page 2-93. [Stephen E Schwartz]	
2-2049	A	62:45	63:18	This chapter is very useful but I would strongly recommand to move it before, as the LOSU concept is used several times before. [Cathy Clerbaux]	Accepred- layout rejigged
2-2050	A	62:45	63:19	It would be helpful to be able to differentiate the diferent types of uncertainties into relevant categories. For example, the first order question might be how much dust and sulfate aerosol is in the atmosphere. Is knowing the size distribution and height distribution a problem? Are there significant uncertainties in refractive indices and radiative transfer modeling? How accurately is it possible to separate anthropogenic from natural contributions? Do natural and anthropogenic aerosol components have the same size? [Andrew Lacis]	Some clarification added but space requirements mean these are not discussed at length
2-2051	A	62:45		This presentation of how uncertainty was dealt with is very good. I would consider moving it to a place earlier in the chapter, before discussing RF. [Anthony Patt]	Accepted
2-2052	A	62:45		It remains unclear what precisely is meant by 'well established' and 'sufficient evidence'. This should be explicitly explained, or, if applicable, a reference to a AR4-paragraph on	Accepted – statements clarified -

No.	Batch	Page:line From	Page:line To	Comment	Notes
				the interpretation of such words should be included. Does 'well established' require some minumum of papers? Or is it still a subjective measure? Does 'sufficient evidence' means that a sufficiently high fraction of some group of experts supports the result? Which fraction, which group? Or is this also a subjective measure? [Peter Siegmund]	
2-2053	A	63:0		Section 2.9.3: How were the RFs in Table 2.9.1 obtained? The interactions listed in this section are not complete. For example, tropospheric O3 and OH influence sulfate formation, the presence of sulfate affects nitrate formation, and heterogeneous reactions influence O3 and OH concentrations. [John Seinfeld]	Section expanded to take these into account
2-2054	A	63:1	63:18	I agree with this analysis. [Jerry Mahlman]	Thank you
2-2775	B	63:6	63:6	90% confidence interval: can you explain why? Do you mean that 90% of the model would fall in this interval? [Olivier Boucher]	Explained more carefully
2-2055	A	63:8		insert. "and" after "uncertainties" [Hartmut Grassl]	accepted
2-2056	A	63:10	63:11	There is another reference here to Section 2.8.5 relating to the cloud-lifetime effect, but there is no sign of it in Section 2.8.5. [Leon Rotstayn]	2.8.5. updated to discuss these effects
2-2057	A	63:22	63:22	This figure is really interesting, but in its present state I would recommend it be deleted as there is insufficient supporting evidence for the values that have been adopted in the figure. It will be widely used and reproduced, so we need to know where, for example, the separation of the NOx emission effects come from [Keith Shine]	We agree that the figure is really interesting (and more informative for policymakers), that why we included (cf. Comments below). However, we agree that it clearly needs more work to be acceptable. This includes adding uncertainty ranges and an improved figure caption giving the approriate references. An improved and better referenced version is included in the SOD.
2-2058	A	63:22		Section 2.9.3: This approach is novel and very useful for the general understanding and, I assume, also for policymakers. But in order to successfully convey the message an improved presentation is needed. A reference to Schindell et al. 2005 in GRL (An emission based view…) is needed (already included in the reference list). [Jan Fuglestvedt]	Agree, see reply to comment 2057.
2-2059	A	63:22		A major failing of this entire Chapter is the inadequate treatment of "emissions" and the	The reply to this should probably just

No.	Batch	Page:line From	Page:line To	Comment	Notes
				relationship between emissions and atmospheric concentrations. This whole section is confused as a result. See my General" comment [Vincent Gray]	refer to his General comment.
2-2060	A	63:22		Global-Mean RF by Emission Precursor. Good idea! [Joanna Haigh]	Thanks.
2-2061	A	63:25	63:25	A minor suggestion: Add ("positive") after "CH4 emissions" and "(negative)" after "Nox emissions" [Jan Fuglestvedt]	Too detailed, can be seen from the figure.
2-2776	B	63:34	63:34	Fossil carbon emissions of non-CO2 gaseous compounds/species …. [Olivier Boucher]	OK
2-2062	A	63:34	63:42	The list of bullet points does not match very well with the figure and does not give a good overview. I suggest making it more clear that this is the RF responses to the drivers. I guess the point is to illustrate the links and a table (driver vs response) could work better. (Such a table would also give a good general overview.) (See page 291 of Fuglestvedt et al. 2003 for an example of a similar table). The colors of the responses in the figure is difficult to separate from each other. [Jan Fuglestvedt]	The bullet points are neede to describe which indirect effects are included in the analysis shown in teh figure 2.9.2. The introduction to the section has been rewritten to make this more clear.
2-2063	A	63:34	63:41	It appears to me that a number of these constituents are small players in this assessment, thus warranting less weight in your deliberations. [Jerry Mahlman]	They may be small, but showing that they are indeed small can be an important message. By omitting them readers will know that they have been considered and are small.
2-2064	A	63:34		2.9.3 - the spatial and temporal variations in O3 needs to be added to the list, and hence also Nox in the stratosphere which depletes ozone be very significant amounts (30%) during large geomagnetic storms especially in winter when vertical transport plays a very important role. [Alan Rodger]	Agree, Nox in the stratosphere (from N2O emissions) which affects tratospheric ozone should not omitted. The second bullet point and the first sentence after the bullets were contradicting each other.
2-2065	A	63:36	63:36	bromine and tempeture changes also contribute to the stratospheric ozone change. [Keith Shine]	Agree in the case of bromine, tempertature would be partly a kind of climate feedback (as well as ozone feedback).
2-2066	A	63:38		I believe HCFCs should be added here [Derek Cunnold]	In principle yes, the general term halocarbons are now used.
2-2067	A	63:44	63:49	These are small players. [Jerry Mahlman]	See reply to 2063

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2068	A	63:44	63:44	"A number of the principal RFs (e.g., N2O, land-use and mineral dust) do not affect other agents, thus their RFs are the same as those presented in Table2.9.1." I do think this is a correct statement. Taking the example of mineral dust,it certainly affects other agents. Mineral dust has been found to provide surface for heterogeneous reactions, which can change the concentrations of a number of key atmospheric species, e.g., O3, SO2, NO2, HNO3, organic compounds, etc (Bian and Zender, 2003; Falkovich et al., 2004; Phadnis and Carmichael, 2000; Underwood et al., 2001). Most of these species are closely related to climate forcing. Even land-use change has an impact on the concentrations of radiatively active species, such as CO2, CH4, etc. Bian, H. and C. S. Zender, 2003: Mineral dust and global tropospheric chemistry: Relative roles of photolysis and heterogeneous uptake, J. Geophys. Res., 108, D21, 4672, doi:10.1029/2002JD003143. Falkovich, A. H., G. Schkolnik, E. Ganor, and Yinon Rudich, 2004: Adsorption of organic compounds pertinent to urban environments onto mineral dust particles, J. Geophys. Res., 109, D02208, doi:10.1029/2003JD003919. Phadnis, M. J. and G. R. Carmichael, 2000: Numerical Investigation of the Influence of Mineral Dust on the Tropospheric Chemistry of East Asia, J. Atmos. Chem. 36, 285–323. Underwood, G.M., C.H. Song, M. Phadnis, G.R. Carmichael, 2001: Heterogeneous reactions of NO2 and HNO3 on oxides and miniral dust: A combined laboratory and modeling study, J. Geophys. Res., 106, D16, 18055-18066. [Xiaobin Xu]	Agree that N2O does affect other stratospheric ozone. Text changed. Land use induced changes in emissions or uptake are covered by the individual emissions The indirect role of mineral dust has been judged to be too uncertain to include in teh figure 2.9.2. It is mentioned in the last paragraph of section 2.9.3 as a mechanism that has a potential indirect effect.
2-2069	A	63:53		Section 2.9.4. This section is absolutely vital for ch9 when it comes to using the temporal patterns of aerosols and WMGHGs to deduce that recent warming is due primarily to WMGHGs. Ch9 asserts that while the absolute magnitude of the aerosol forcing is highly uncertain, the temporal pattern is robust. By making that assumption they then do a reverse calculation to determine how much of the recent warming is due to WMGHGs. My argument is that there is no evidence presented in Ch2 to demonstrate that the temporal pattern of aerosol RF is well understood. Quite the contrary: you conclude that "Aerosol and ozone RF time-histories remain too uncertain to ascertain an accurate time-evolution of RF beyond the examples given in Figure 2.9.3." So how come Ch9 appears to think otherwise? [Kenneth Carslaw]	Noted. Will cross-check with Chap. 9 about consideration of the absolute uncertainty of RF in their arguments.
2-2070	A	63:53		The paper cited in the caption of Figure 2.9.3 does not have corresponding figures. Something should be wrong. [Kiminori Itoh]	Noted. Paper is in "submitted" stage in GRL.
2-2777	B	63:55	63:55	Aren't flask measurements a sort of in-situ measurements as well?	Noted. Will revise.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Olivier Boucher]	
2-2071	A	64:1	64:27	Nice analysis. [Jerry Mahlman]	Thank you!
2-2778	B	64:2	64:2	can you spell out ODS? [Olivier Boucher]	Noted. Ozone Depleting Substances.
2-2072	A	64:2	64:2	Change "peeked" to "peaked" [Patrick Hamill]	Noted with thanks.
2-2073	A	64:2	64:2	peeked -> peaked? [Reto Knutti]	Noted with thanks.
2-2074	A	64:2	64:2	…the combined RF of all ODS appears to have peeked, at 0.32" I think it should be "…the combined RF of all ODS appears to have peaked at 0.32 [Xiaobin Xu]	Noted with thanks.
2-2075	A	64:2		correct: "pecked" to "peaked" [Hartmut Grassl]	Noted with thanks.
2-2076	A	64:2		ODS not defined. Ozone depleting substances? [Stephen E Schwartz]	Noted.
2-2077	A	64:4	64:5	- the decrease since TAR in the halocarbon RF shown in Table 2.9.1, is due to re-evaluation rather than a trend." What to say here is that the difference between the value 0.33 in AR4 and the value 0.34 in TAR is a result of re-evaluation, not due to a trend in the halocarbon RF. However, if we use the expression like "the decrease since TAR in the halocarbon RF", we are imply that there had a negative trend in the halocarbon RF, aren't we? My suggestion: "the lower halocarbon RF in AR4, as shown in Table 2.9.1, is due to re-evaluation rather than a trend since TAR. [Xiaobin Xu]	Accepted.
2-2078	A	64:7	64:7	"reasonably well known" - I would agree for the past 20 years or so, but I think it hard to defend for the pre-satellite era. [Keith Shine]	Accepted.
2-2079	A	64:7	64:15	In this paragraph, "timeseries", "time histories" and "time-histories" are used many times. This usage confused me very much. Do you mean the same thing by "timeseries" and "time histories"? If so, "timeseries" should be used throughout. If not, it is better to use another word for "time histories" or give an explanation to the term that is not so easy to understand. [Xiaobin Xu]	Noted. Will use "timeseries" to denote species' amounts, "time history" to refer to implementation of the forcing agents in GCMs, and "time evolution" for RF change with time.
2-2080	A	64:10		delete the comma after "RFs" [Hartmut Grassl]	Noted.
2-2779	B	64:14	64:14	time evolution (without dash)	Noted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Olivier Boucher]	
2-2081	A	64:19	64:24	"As an example of the timeseries of the RF and surface forcing due to the principal agents, the global-and-annual-mean time series, as implemented in the MIROC AOGCM (Takemura et al., 2005), is illustrated in Figure 2.9.3. As for the present day RF, past RF is also dominated by the LLGHGs (see also Figures 2.3.3 and 2.9.1). The surface forcing, in contrast, is dominated by the negative effect of the aerosols and the LLGHGs have a much smaller positive effect." I have two points here. First, I think the word "dominated" in the line 22 is not appropriate. If you compare the RF curves for "Well-Mixed GHG" and "Total Aerosol Indirect" in Figure 2.9.3, you can hardly see significant differences between the absolute values of both, except for the recent years. For some periods (e.g., 1950-1975) the negative forcing of Total Aerosol Indirect was even slightly stronger than the positive forcing of Well-Mixed GHG. Second, the "(see also Figures 2.3.3 and 2.9.1)" does not help and makes confusion. In Figure 2.3.3, only data of LLGHG are displayed. The readers cannot compare RFs of LLGHG with other agents. In Figure 2.9.1, RFs of both LLGHG and other agents are displayed, but no timeseries of the RFs, therefore, the readers cannot get information about what is dominating in a certain period. So, it is better not to take these two figures as supporting materials. [Xiaobin Xu]	Noted. "Dominated" will be replaced by "exceeds" and will state that this is clearly so only for the last 2 decades or so, based on this particular model result (as derived from the revised plot). Will drop reference to the other two figures in this context.
2-2082	A	64:21	64:21	The paper referred to (Takemura at el., 2005) does not give the trop O3 RF history and thsese data are not avaiable in the literature, as far as I can see. It would be an advantage if these data were available for the reader. [Jan Fuglestvedt]	Noted. The authors give the value for only "ozone".
2-2083	A	64:21	64:22	Syntax. Re-write sentence for clarity. [Patrick Hamill]	Noted.
2-2084	A	64:26		delete the comma before "Forster" [Hartmut Grassl]	Noted.
2-2085	A	64:33		See comment 7 above about the spatial and temporal variation in solar effects [Alan Rodger]	Not sure what is the issue being referred to.here?
2-2086	A	65:0	69:	Section 2.10: There are several metric for comparing different emissions. Instead of just listing them, it will be very useful to know possible IPCC recommendations regarding which of them should be used and when. Let me suggest to include a new Table summarizing all available climate change metrics with comments regarding their limitations and advantages. Many possible metrics can easily create a confusion among policy-makers and even atmospheric scientists, who are not experts in climate change. [Mikhail Danilin]	The text has been made more explicit with regard to advantages and disadvantages with the various metrics. A recommandation for contiation of the use of GWP as the preferred metric is made given that a concensus about dangerous anthropogenic interfernce with the climate system has not been given.

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2087	A	65:0	69:	Section 2.10 does not appear as well integrated into the executive summary as the other sections. Perhaps delete this if space is tight. Alternatively add a discussion on the different metrics explaining why "GWP remains the recommended metric" (p. 3) [Roger Davies]	Section 2.10 is needed because it gives the updated GWPs. A summary explaining why GWPs are still recommended is included.
2-2088	A	65:0		This is a careful description of GWP that works well for the "well behaved"GHGs, and is a very useful tool for diagnosing the time-integrated response for the LLGHGs. [Jerry Mahlman]	OK
2-2089	A	65:1	65:1	I think there is an inadequate reflection of the debate on the utility of GWPs in this section [Keith Shine]	Agree, se also comment 2-2091.
2-2090	A	65:1		Section 2.10: I think this section represents an improvement compared to earlier IPCC reports. However I suggest separating out (from the extisting text) a sub-section on direct GWPs (starting at top of page 66) since the first page is general and does not specifically address direct GWPs. I also suggest that the uncertainty in the GWPs are presented explicitely. [Jan Fuglestvedt]	OK.
2-2091	A	65:1		After a large number of papers with evaluations of the GWP and its use have appeared in the literature (e.g. papers by Smith&Wigley, Wigley, O'Neill, Lashof, Godal, Shine et al., Frolking et al., Fuglestvedt et al., etc) it would be natural to expect that this would be reflected in this IPCC report; even if some of these were briefy mentioned in TAR. This is especially important now that preparations for post 2012 climate agreements have started. If GWPs are adopted again it is important that there is a better understanding of what the GWP does and what it doesn't and that a choice of GWP as the metric for climate agreements is a conscious choice. Thus, a small section reflecting the scientific debate and the strong and weak aspects of this metric is strongly needed here. And it doesn't have to be very long. [Jan Fuglestvedt]	Ok, included in revised version of section 2.10.1
2-2092	A	65:1		There needs to be a section discussing the relationship between emissions, conecntrations and radiative forcing before this section. Since there is no firm relationship between emissions and concentration for any of the greenhouse gases the GWP concept is surely dubious [Vincent Gray]	Lifetimes are given in Table 2.10.1, For CO2 a ref. To the Bern CO2 model is given. For the indirect GWPs, a full model is needed, and there are references to the papers in section 2.10.3
2-2093	A	65:1		The IPCC should be commended for its attempt to provide some guidance on comparing the impact of various emissions, and using economic valuation to guide decisions (2.10.3.2.2). However, this section merely skims the surface of this important topic and offers very limited examples. Such an important topic deserves more extensive treatment, as well as specific advice for various sectors. The work of the aviation sector to address	Agree, The use of econmic valuation is potentially important and require a more detailed dsicussion. This is taken care of by IPCC WGIII, and thus we introduce the concept of including

No.	Batch	Page:line From	Page:line To	Comment	Notes
				interdependencies amongst emissions, and to valuate the impact of these emissions would be an excellent example to cite. Also, the discussion of emissions impacts should acknowledge the need to balance concerns between near-term health impacts and long-term climate change impacts and offer some guidance to policy makers on how to explore and address this balance. [Lourdes Maurice]	economic factors in section 2.10.1, but for examples we only refer to that report in section 2.10 . cf. reply to comment 2-2192
2-2094	A	65:5	65:27	Too complicated : suggestion to remove this part. [Cathy Clerbaux]	Don't agree. Text retained. The formal framework is needed for the putting the new metrics into perspective.
2-2095	A	65:5	65:9	The first sentence is long and complicated. The text in parentheses could be deleted. I suggest split and re-write. [Jan Fuglestvedt]	Accepted. Text rewritten
2-2096	A	65:9	65:9	isnt the equation a general formulation of an emissions metric? [Keith Shine]	Accepted
2-2097	A	65:11	65:12	Formula for AM: The subscript r+i may be difficult to read and may cause misunderstanding. I suggest using p (for perturbation) instead of r+i. [Jan Fuglestvedt]	Not changed. I is defined as a perturbation in the text. Letters i or p would be of equal size anyway.
2-2098	A	65:11	65:11	I should have noted this before I think, buy there is some confusion about using infinity as the integration limit. I would think infinity is appropriate, but what becomes confusing is on line 43 where you say the GWP assumes g(t)=1 … surely it is 1 for t<TH and zero for t>TH .. in which case line 45 is redundant. Perhaps this is a different way of looking at the same thing? [Keith Shine]	Accepted, line 45 is removed and line 43 changed.
2-2099	A	65:22	65:22	The choice of indicator (RF, dT, SLR, damage etc) along the cause effect chain needs more attention; before impact function is mentioned. See discussion in the following paper which could also be referenced: den Elzen, M, JS Fuglestvedt, N Höhne, C Trudinger, J Lowe, B Matthews, B Romstad, C de Campos and N Andronova, 2005. Analysing countries' contribution to climate change: Scientific and policy-related choices. Environmental Science and Policy, (In Press; available online). [Jan Fuglestvedt]	Due to space limitation a detailed discussion of impact function(indicator) can be included. Text retained.
2-2100	A	65:26	65:26	'with its inherent' should be 'with their inherent' [Tami Bond]	Accepted.
2-2101	A	65:31	65:31	I think references to the first GWP papers should be given here; i.e. the Nature paper by Lashof and Ahuja and the chapter by Shine et al in the first IPCC report. [Jan Fuglestvedt]	Accepted. Ref to Shine et al. Is included.
2-2102	A	65:33	65:33	Perhaps it would be better to use the symbols x to refer to a gas x uniquely and use the	Belive this does not make it clearer.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				symbol r to refer to the decay function or pulse response function. In other words, replace the terms in bracket by [r_x] and [r_r] [Fortunat Joos]	Text retained.
2-2103	A	65:33		I again suggest (as I have in reviews of previous IPCC assessment reports) that the IPCC abandon use of GWP referenced to the AGWP of CO2 and report only AGWP [units W m-2 yr]. Reason AGWP's will not shift every time further research refines the forcing per kg of CO2 and/or the CO2 residence time profile; the use of systematic units is to be encouraged throughout science. Use of GWP referenced to CO2 is akin to measuring the density of substances relative to water, and then having to change their density if further research refines the density of water. Except of course that the AGWP of CO2 is a lot less well known than that of water. Note that the present review draft states that "The AGWP values [of CO2] will be updated when revised pulse response functions for CO2 are available (October 2005)" and hence all GWP's are held hostage as a consequence. I have been advocating this at least in reviews of AR2 and AR3. Sooner or later IPCC will come around to it, as it has with several other points I have made in earlier reviews. A more intrinsic reason for abandoning GWP's ratioed to CO2 and using AGWP's exclusively is in the dependence on time horizon. The GWP ratioed to CO2 is the ratio of two integrals, each of which is the integral of the forcing of a substance over a time horizon. The dependence of GWP on time horizon is thus dependent on the impulse profile in both numerator and denominator, meaning that the GWP of a substance can either increase or decrease (or both) with time horizon, depending on how the impulse profile of the substance compares to that of CO2. As most gases have lifetimes shorter than that of CO2 their GWP's decrease with increasing time horizon. But there are exceptions such as CFC-114, having GWP which reaches a maximum and then decreases, and CFC-115, having GWP which monotonically increases with time horizon. The situation becomes much more acute with respect to short lived substances such as aerosols for which the increase of the denominator, but not the numerator, with increasing time horizon results in a GWP that varies roughly inversely as time horizon, as discussed below with regard to page 2-67. In contrast AGWP's are monotonic, and ultimately reaching a constant value as the material is depleted from the reservoir. At the very least the time-dependent AGWP of CO2 should be precisely specified so that the AGWP's of other substances can be back calculated from the tabulated GWP's. But better to give the AGWP's and abandon the GWP's at least in scientific documents such as WG1 assessments; the economists and Kyoto emissions traders can always calculate them from AGWP's. [Stephen E Schwartz]	Although we agree that there are good arguments for using only the AGWPs, we find that there are stronger reasons to keep the relative GWP. The purpose of metric values are only to compare emissions so eventually it is the relative effect that is to be used. We also fear that the users will be confused if they are to use AGWPs with complicated units and "scary" numerical values (e.g. AGWP for CO2 is something like $8.8x10^{-14}$ Wm^{-2} yr for 100 yr timehorizon). Thus the GWPs are retained. AGWPs for CO2 for 20, 100 and 500 years time horizons are given in the text.
2-2104	A	65:35		Add blank between 'xa' and 'is' [Cathy Clerbaux]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2105	A	65:35		Please, write the term TOA in words. [Ilkka Savolainen]	TOA was only confusing here as radiative forcing has been defined earlier in the chapter. TOA removed.
2-2106	A	65:38	65:38	Correct "nominator" as "numerator" [Ramachandran Srikanthan]	Accepted
2-2107	A	65:42	65:45	The g(t) function and TH are related. g(t) = 1 for t < H and g(t)= 0 for t>H. [Jan Fuglestvedt]	Accepted, cf comment 2092.
2-2108	A	65:46	65:46	The word "response" may be replaced by "sensitivity" or "efficacy" since this is about GWP and not metrics in general. [Jan Fuglestvedt]	Accepted, have changed to response
2-2109	A	65:46	65:46	I think "sensitivity" is better than "response" [Keith Shine]	Accepted, have changed to response
2-2110	A	65:50	65:	A few sentences explaining the application of GWP is needed here; i.e. forward looking on impacts of current emissions, choice of integrated RF, the choice of horizon and what GWP weighted emissions tell us. [Jan Fuglestvedt]	Agree, added in section 2.10.1
2-2111	A	66:0		Indirect GWPs are clearly less robust as a diagnostic tool, and are messy to deal with. I suggest a lesser emphasis on them, simply because the degraded products of GHGs are much less likely to be of quantitative value in the quest for the RFs of all applicable substances. [Jerry Mahlman]	Historically indirect GWPs are a part of an IPCC assessment, and although are knowledge is not perfect it has improved since the TAR. We believe that appropriate caveats are given in the text about theit usefulness for policymaking. Direct GWPs will get more attention in teh SOD with a separate section, and a revised Table.
2-2112	A	66:1		I suggest splitting out a new section at the top of this page entitled "Direct GWPs" [Jan Fuglestvedt]	OK
2-2113	A	66:2		374 (?) [Hartmut Grassl]	An explanation of teh background CO2 levels used for AGWP calculations have been included.
2-2114	A	66:5	66:9	It would be very useful for the readers and users of IPCC results if the response function for CO2 could be given explicitely. [Jan Fuglestvedt]	OK, footnote to Table 2.10.1
2-2115	A	66:6	66:6	F. Joos, M. Bruno, R. Fink, T. F. Stocker, U. Siegenthaler, C. Le Quéré, and J. L. Sarmiento. An efficient and accurate representation of complex oceanic and biospheric models of anthropogenic carbon uptake. Tellus, 48B:397-417, 1996.	A cross reference to Chapter 8 where the model is described has been added. A ref. To Joos et al., 1996 is then not

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Fortunat Joos]	needed here.
2-2116	A	66:6	66:6	Specify: The Bern carbon-cycle model as used in the SAR (F. Joos, M. Bruno, R. Fink, T. F. Stocker, U. Siegenthaler, C. Le Quéré, and J. L. Sarmiento. An efficient and accurate representation of complex oceanic and biospheric models of anthropogenic carbon uptake. Tellus, 48B:397-417, 1996.) [Fortunat Joos]	The Bern CC model is used and described in Chapter 8. Thus a cross ref. To Table 8.8.2 is included
2-2117	A	66:8	66:9	A whole sentence is highlighted! [NADIA GAMBOA]	Removed
2-2780	B	66:11	66:11	Delete second occurrence of Montzka et al. [Olivier Boucher]	OK
2-2118	A	66:11		delete: "Montzka et al.," [Hartmut Grassl]	OK
2-2120	A	66:12		up too >> up to [Cathy Clerbaux]	OK
2-2121	A	66:12		correct: "too" to "to" [Hartmut Grassl]	OK
2-2122	A	66:12		too" should be "to [Xuepeng Zhao]	OK
2-2123	A	66:13	66:14	correct: "b" to "be" at the end of line 13 and delete "e" at the beginning of line 14 [Hartmut Grassl]	OK
2-2124	A	66:13		b e >> be [Cathy Clerbaux]	OK
2-2125	A	66:19	66:19	Use adjustment time instead of lifetime? [Jan Fuglestvedt]	OK
2-2126	A	66:20	66:20	"47"- has an explicit decision been made to include this particular 47, and exclude some gases (two of my own favourites, SF5CF3 and C10F18 are missing, but there are probably many examples)? If so, what criteria have been used in making this decision? It is important, as this particular list might even be adopted for a post-Kyoto agreement and gases missing from the list could be excluded from the protocol, at least if Kyoto and SAR processes are followed. [Keith Shine]	Agree that this is important. We have gone through the list and added compounds that we believe are potentially important. To give a strict criteria is very difficult since it depends on the physical properties of each compound (RF efficency and lifetime) but also on potential future emissions which is very difficult to assess.
2-2127	A	66:21		Give unceratinty bands for GWPs and some background text for the unceratinty	Agree. Discussion included. Ref to

No.	Batch	Page:line From	Page:line To	Comment	Notes
				estimation. Give also reference to the Bern carbon cycle model and to its incertainty with some text explaining the main contributors. [Ilkka Savolainen]	Bern CC model, see comment 2115.
2-2128	A	66:23	66:23	On what scientific grounds have the authors chosen to use the particular time horizons of 20, 100 and 500 years in the calculations of GWPs (Table 2.10.1)? [Odd Godal]	The three timehorizons for the GWPs are given for historical reasons. However, all the input data needed to calculate GWPs with other timehorizins are given in the Chapter.
2-2129	A	66:23		No reference to Table 2.10.1 in the text [Cathy Clerbaux]	Not correct. Page 66 line 20.
2-2130	A	66:25		Section 2.10.2. This section is called 'Indirect GWPs'. However, not all of the GWPs listed in this section are indirect (e.g. aerosols). [Tami Bond]	Agree, GWPs for aerosols and aerosol precursors are presented in a separate subsection 2.10.3.
2-2131	A	66:25		Section 2.10.2. GWPs for short-lived species are discussed. Many of these contain statements that the GWP depends on the location and timing of emission. A useful perspective might result from discussing this issue in the beginning of the section, and combining a discussion of all the studies that result in regional GWPs. [Tami Bond]	Agree, GWPs varying with location (and time) is discussed at teh end of Section 2.10.1
2-2132	A	66:25		The role of precursors of trop O3 and aerosols (e.g. BC) has recently received a lot of attention in the literature; both their physical and chemical roles in the climate system and their potential roles in policymaking (e.g. Hansen et al., Akimoto, Jacobson, Rypdal et al., Derwent et al. etc). Thus, it would be useful with a somewhat more substantial evaluation of the status of the knowledge on the GWPs for these gases. The fact that these emissions are included in figure 2.10.1. is also an argument in favor of allocating some more space to these species. [Jan Fuglestvedt]	About one page of text is devoted to these compounds. Given the total number of pages allocated to teh Chapter mores space could not be spent on this (cf. Comment 2111). However, we have looked at the text again and tried to clearify it and put it into context.
2-2133	A	66:29	66:30	This sentence is difficult to understand. I think I know what it means (that one does not need to calculate the second-order indirect effects) but it is not clear. [Tami Bond]	OK, text changed
2-2134	A	66:31	66:32	"...and changes in concentrations of OH radical with the main effect of enhancing the lifetime of methane..." A "change" can be a decrease or an increase. If the concentrations of OH radical are increased, then the effect will not be "enhancing the lifetime of methane". I suggest (1) to use "decreases" instead of "changes" or (2) to use "changing" instead of "enhancing". [Xiaobin Xu]	Agree. Text changed
2-2135	A	66:32	66:32	I suggest replacing "enhancing" by "changing" since this may work in both directions (e.g. Nox vs CO).	OK

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Jan Fuglestvedt]	
2-2136	A	66:33	66:33	"much higher" - you havent defined the uncertainty of the direct GWPs as far as I can tell. [Keith Shine]	Agree. Cf. Reply to comment 2127
2-2137	A	66:35	66:35	The sentence "Their usefulness to…" may be a bit too strong. What is meant could be explained better. [Jan Fuglestvedt]	Agree, text changed.
2-2138	A	66:36	66:36	"…alternative methodologies have..." is, as far as I can see, addressing a different issue than indirect effects and could be deleted. [Jan Fuglestvedt]	Agree, text changed
2-2139	A	66:39	66:45	A clear distinction between carbon of fossil and non-fossil origin needs to be given here. [Jan Fuglestvedt]	Agree, text changed
2-2140	A	66:41	66:41	As far as I can tell, the new water vapour value from Section 2.3.8.1 has not been incorporated in the GWP [Keith Shine]	Agree, the new values will be incorporated in the revised GWP for methane.
2-2141	A	66:41	:43	The reason to neglect the CO2 from CH4 oxidation into the GWP is not that CO2 is already included in other carbon inventories. The reason actually is that CO2 from biogenic CH4 is recycled through the biosphere. Since >80% of atmospheric CH4 is thought to originate from biological sources (wetlands, rice paddies, ruminants, termites etc.), the effect of the remaining <20% from fossil CH4 sources is only very small, as argued by Lelieveld and Crutzen (1992) and Lelieveld et al. (1998). This argumentation has been adopted in subsequent studies and in the IPCC reports. [Jos Lelieveld]	OK. Text revised to to state that CO2 from CH4 from fossil fuels are not included in the GWP for methane.
2-2142	A	66:43	66:45	The value of 23 for methane is not just indirect effects, is it? [Tami Bond]	No, Total effects. Text changed
2-2143	A	66:43	66:43	Typo. "As" not "AS" [Patrick Hamill]	OK
2-2144	A	66:43	66:43	…AS in TAR this" -> "…As in TAR this [Xiaobin Xu]	OK
2-2145	A	66:43		correct: "AS" to "As" [Hartmut Grassl]	OK
2-2146	A	66:43	:46	The value of 23 (actually 22) for the GWP of methane including indirect effects, as adopted by previous IPCC reports, originates from Lelieveld and Crutzen (Lelieveld, J. and P.J. Crutzen, Indirect chemical effects of methane on climate warming, Nature 355, 339-342, 1992), and was re-affirmed by Lelieveld et al. (1998) using a coupled chemistry-climate model. In previous IPCC reports (incl. SAR and TAR) the simplified GWP model of Wigley was used, and for methane this model was tuned to the model results of	The methane GWP is updated according to revised AGWP for CO2, revised perturbation lifetime for CH4 (cross ref to Ch. 7), Updated indirect effect of strat H2O and production of trop ozone due to enhanced methane.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Lelieveld et al. [Jos Lelieveld]	The latter is be based on TAR estimates and a ref. To TAR is included.
2-2147	A	66:44	66:44	"uses a value" of what? Methane GWP?? [Keith Shine]	OK, Text changed
2-2148	A	66:44		delete: one full stop [Hartmut Grassl]	OK
2-2149	A	66:47	66:51	Need to mention that contribution to CO2 is not included + a clear distinction between carbon of fossil and non-fossil origin needs to be given. [Jan Fuglestvedt]	OK
2-2150	A	66:49	66:49	delete one instance of Collins et al. [Reto Knutti]	OK
2-2151	A	66:49		delete: "Collins et al.," and insert a bracket before "2002" [Hartmut Grassl]	OK
2-2152	A	66:50	66:50	'…for CO emissions, the range…' should be '…for CO emissions, which range…' [Tami Bond]	OK
2-2153	A	66:50		correct: "the" to "they" [Hartmut Grassl]	Ok, se above
2-2154	A	66:53	67:4	A clear distinction between carbon of fossil and non-fossil origin needs to be given here. [Jan Fuglestvedt]	Agree. A general statement about this for CH4, CO and NMVOCs is included in the introduction to section 2.10.3
2-2155	A	66:53		Section 2.10.2.3. Values for 10 different NMVOCs are given. Is it possible to estimate a composite GWP for NMVOCs, as was done in the 1990 assessment report? [Tami Bond]	It can be done if speciation of NMVOCs are known. We believe taht thsi is beyond the scope of this report.
2-2156	A	67:0		This page offers a lot of text, considering the inclusion of relatively small contributions that these "fringe candidates" bring to the integrated RF challenge. A number of them are apparently negligible in the long-term global sense. [Jerry Mahlman]	Historically they belong to an IPCC report. The fact that they are small does not mean that they do not contribute to changes that are significant. Eg. Nox leads to trop ozone formation which is a large bar in fig. 2.9.1, but due to teh effect on methane (neg. RF) teh net effect is small.
2-2157	A	67:9	67:9	According to Figure 2.9.2, the nitrate aerosol creation is the dominant effect of NOx emissions, but it is not mentioned in the text. [Keith Shine]	Agree, should be mentioned.
2-2158	A	67:9		Nox does not have a short lifetime in the upper stratosphere in winter [Alan Rodger]	This refers to Nox emitted as Nox which is not a source of Nox to teh

No.	Batch	Page:line From	Page:line To	Comment	Notes
					upper stratosphere.
2-2159	A	67:12	67:12	"…and with a strict definition" is unclear. [Jan Fuglestvedt]	OK, text changed
2-2160	A	67:14	67:14	I suggest adding a reference to a recent paper by Shine et al that gives a good overview of the problems related to Nox. Shine, Keith P., Terje Berntsen, Jan S. Fuglestvedt and Robert Sausen, 2005. Scientific issues in the design of metrics for inclusion of oxides of nitrogen in global climate agreements. Proceedings of the National Academy of Sciences (PNAS), (Vol 102, No 44, 15768-15773). Reference to table 2.10.2. is missing. [Jan Fuglestvedt]	OK
2-2161	A	67:19		insert a semicolon and "see" before "Table" [Hartmut Grassl]	OK
2-2162	A	67:24	67:24	"such as Nox" could be added after "short lived species". [Jan Fuglestvedt]	Not needed. Text retained
2-2163	A	67:26	67:50	Why not include the GWP values for H2 and aerosol and aerosol precursors in Table 2.10.2? [Xiaobin Xu]	The structure of Table 2.10.2 (methane ond ozone effects) does not fit the aerosols. Thus the numbers are given in the text. Due to all the caveats regarding aerosol GWPs we believe that putting the numbers in the text next to the discussion of teh caveats would decrease the chances of misuse of the numbers.
2-2781	B	67:33	67:33	GWP(100) --> GWP_100 [Olivier Boucher]	OK
2-2164	A	67:36	67:44	A paper by S. Schwarts with GWPs for SO2 should be cited here: Does fossil fuel combustion lead to global warming? Schwartz, S. E., Energy Internatl. J. 18, 1229-1248 (1993). [Jan Fuglestvedt]	This is an historically important paper, but does not add to the current assessment
2-2165	A	67:36	68:12	SUBSTANTIAL COMMENT (1/5): Suggestion to drop references to central values of GWPs for short-lived gases and bottom half of figure 2.10.1. REASONING: The concept of GWPs is a simplified (but useful) tool that is tailored to the direct application in the policy context which makes this issue much more politically sensitive than the presentation of other research results. Thus, special care in terms of the presentation of results seems warranted. The citation of central values for GLOBAL Warming Potentials (GWPs) of short-lived species, in particular aerosols, is misleading and dubious from a scientific point of view. In summary, there are four reasons, why the citation of central GWP values for the short-	The globally averaged GWPs for short-lived species are retained, and the bottom half of figure 2.10.1 is kept, with the exception that the indirect aerosol effects are removed from the figure. Cf. Also reply to comment 2-2179. Through strong caveats in the text we do not say that "that GWPs for short-lived gases and aerosols are

No.	Batch	Page:line From	Page:line To	Comment	Notes
				lived species should be dropped. This affects as well Figure 2.10.1, given that the bottom half of the figure gives the impression that GWPs for short-lived gases and aerosols are generally suited and sufficiently developed to be applied to today's emissions. (...continued) [Malte Meinshausen]	generally suited and sufficiently developed to be applied to today's emissions", but by calculating the CO2-eq. emissions as given in Fig. 2.10.1 policy makers can get an indication of the total potential effects of current emissions. If we do not show this estimates and the figure, we risk the situation we have had since the TAR where the relative sice of current RF (as given in Fig. 9 of the technical summary of TAR, corersponding to figure 2.9.1 of 4AR) has been widely misused as a measure to judge the relative importance of mitigating differnet climate agents.
2-2166	A	67:36	68:12	(comment continued 2/5): The four reasons are: (1) 'SOURCE-LOCATION DEPENDENT' PATTERNS: the direct radiative effect of a pulse emission of short-lived gases and aerosols is regionally constrained, roughly speaking to the downwind region of the emission source - opposed to LLGHGs, where the location of the emission source does not matter in terms of WHERE the forcing will occur. (Of course, there is the exception of some indirect effects, such as the one on the methane lifetime, where the global pattern of forcing is rather independent of the short-lived gases' source location.) (2) 'SOURCE-LOCATION DEPENDENT' MAGNITUDES: Not only the pattern, but as well the magnitude of the impact will strongly depend on the source region. For example, sulphate aerosol emissions in high latitude winter conditions are most likely to have very different effects compared to emissions in the tropics. Again, this is different to LLGHGs, where the magnitude does basically not depend on the source location. (...continued) [Malte Meinshausen]	Agree that there is a strong dependency on location of the GWPs for short-lived species. We state clearly in the text that if it is decided to include short-lived components in climate agreements, regional GWPs must be established and agreed upon (section 2.10.1, last paragraph)
2-2167	A	67:36	68:12	(comment continued 3/5):(3) TIME-DEPENDENT: the effect is strongly dependent on the TIMING of emissions. For example, aerosol precursor emissions during rainy weather conditions are likely to have a very different radiative forcing effect compared to those in winter smog conditions or in a clear-sky summer week. Again, this is different to LLGHGs, where the radiative forcing does not depend on the timing of the emission. (4) NO OFFSETTING: GWPs are used in the current political framework to build weighted sums of emissions. Therefore, it is misleading to provide negative GWPs and positive GWPs that relate to quite different forcing mechanisms that won't simply offset	3. This short-term time dependency is probably a minor problem since almost any mitigation measure would be through instalation of some technical equipment to clean the emissions or by fuel switch. Both measures would then be applied independent of the weather.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				each other. Going beyond forcing, the climate impacts are quite different, which further invalidates the implicit assumption that negative GWPs of some short-lived gases and aerosols could offset positive GWPs. (...continued) [Malte Meinshausen]	4. By definition GWPs based in global mean RF rank the impact in terms of gloabl mean warming (cf. Section 2.8). As discussde in sections 2.8.2 and 2.8.3 there might not be offsets locally, but model experiments indicate resonable global offsets (or evidence that different RFs can be added linearly). In terms of global temp. Respone to equal global mean RF (efficacy, section 2.8.6 and figure 2.8.1) all reported efficacies (except one model study for stratospheric ozone) report efficacies of ±40%, which is of the same order as the reported uncertainty in GWPs for LLGHG (IPCC, SAR, page 119). Thus the lack of offset geographically is not a major problem.
2-2168	A	67:36	68:12	(comment continued 4/5):For a similar reason, the recent IPCC/TEAP report (Velders et al. 2005) didn't state net radiative forcing of ozone depleting substances, because there "are two distinct climate forcing mechanisms that do not simply offset one another". One could argue that the current text does not advise the aggregation of different emissions into GWP-weighted sums. However, the text & figures have to be written with the knowledge that this is precisely what the main recipients of the report, the policy-makers, do with GWPs. Or does IPCC want to state a preference to include short-lived gases into an enlarged Kyoto basket for post-2012? I guess not. (...continued) [Malte Meinshausen]	See above.
2-2169	A	67:36	68:12	(comment continued 5/5):For these reasons, the provision of central values for globally averaged effects of short-lived gases and aerosols is misleading and should be avoided for the second order draft. Figure 2.10.2 should be adapted accordingly, by keeping the weighted 2000 emissions to the long-lived gases. This is not to say that the research into the radiative effects of short-lived gases and aerosols (and their recent substantial progress) shouldn't be highlighted. Citing ranges to cover the region- as well as time-dependent radiative forcing impacts of short-lived gases might be the way to proceed. As already stated at some point in the text, "the GWP metric is not well suited for handling short-lived gases or aerosols ..." (FOD Ch. 2.page.68.line21). (end of comment)	The suggestion to "Citing ranges to cover the region- as well as time-dependent radiative forcing impacts of short-lived gases might be the way to proceed" is a good, and we have adopted that for the NOx, SO2, BC and OC bars in figure 2.10.1. One ranges are given (as for mineral dust and 1. indirect aerosol effect in the RF

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Malte Meinshausen]	barchart in TAR (Fig 9 in TS of TAR)
2-2782	B	67:36		For SO2 can you also provide the conversion factor from SO2 to sulfate to make sure the calculation is reproducible. Although it falls in section 2.10.2 the GWP by BC is not an indirect GWP. [Olivier Boucher]	The conversion factor is provided in the text. The organization of the sections has been changed so that SO2 and BC/OC is included in a new section 2.10.4 Aerosols and aerosol precursors
2-2170	A	67:36	:53	GWP's of aerosols and aerosol precursors: Previous IPCC reports have not given GWPs values for aerosols or aerosol precursors. Since the TAR significant progress has been made in the understanding of the radiative effects of aerosols (Section 2.4). Bond and Sun (2005) have calculated GWPs for the direct effect of black carbon aerosols (i.e., neglecting the semi-direct effect and surface albedo effects). Based on previously published results for the lifetime of BC and a normalized RF of 1800 W/g, they derive GWP values of 2200 and 680 for time horizons of 20 and 100 years. The uncertainty range for the GWP100 estimate is 210–1500, and for GWP20 690–4700. The main sources for the relatively large uncertainties are model assumptions about transport and removal of particles and optical properties. A global mean GWP for SO2 from fossil fuel combustion (including only the direct effect of sulphate aerosols) can be estimated based on the model results from the AEROCOM experiments summarized in Tables 2.4.3, 2.4.4 and 2.4.5 in this report. Using the modelled global sulphate loading of 3.12 mg m –2 , and all sky RF of –0.37 W m –2 , and a residence time of 4.1 days, GWP values of –161, –48, and –15 are estimated for time horizons of 20, 100 and 500 years respectively. Care should be taken when applying GWPs for BC or SO2 since as with other short lived species the GWPs for BC could vary significantly depending on location and time of the emissions. [Stephen E Schwartz]	This is not a comment, just a copy of text from FOD
2-2171	A	67:36		The application of GWP to aerosols is not new and has been advocated going prior to AR2: Schwartz, S. E., Does fossil fuel combustion lead to global warming? Energy Internatl. J. 18, 1229-1248 (1993). Available from http://www.ecd.bnl.gov/steve/Fossil.pdf As noted in Schwartz (1993) and also above [comment re Page 2-65, line 33 ff re GWP's], there are intrinsic concerns when applying the GWP concept, referenced to the long-lived CO2, to the short-lived aerosols. The aerosol concentration resulting from an emission pulse decays to zero in a few weeks, and hence the integrated forcing (AGWP) reaches a constant, whereas the AGWP of CO2 taken as reference continues to increase over the time scale of decades to centuries typical of time horizons of concern. Consequently the	We agree that AGWPs for short lived species are constant (for different time horizons), but disagree that the decrease of the GWP with increasing timehorizon is an artifact. The decrease of the GWPs for short lived spieces does show that as we enhance the time horizon of our climate policy, we should put more emhasis on long lived species (like CO2).

No.	Batch	Page:line From	Page:line To	Comment	Notes
				GWP for sulfate aerosol decreases (in magnitude) from –161, to –48, and to –15 for time horizons of 20, 100 and 500 years respectively. The decrease is an artifact of dividing by ever increasing CO2 AGWP and has nothing to do with the aerosol forcing. Schwartz (1993) used the AGWP approach to conclude that the strong immediate forcing of sulfate aerosol led to an immediate net cooling influence resulting from the combustion of fossil fuel containing sulfur impurity. The net warming influence of the greenhouse effect of CO2 was found to overcame the cooling influence of the sulfate aerosol only after some 30 to 60 years, depending on assumptions. While the AGWP concept is useful in consideration of forcings of greatly differing time scales, without further explanation the GWP formalism conceals, rather than reveals the essential physics of the situation. Here reference is made to Figure 2.10.1 of the present review draft. The thrust of the figure is that integrated forcing by sulfate aerosol for a pulse emission, all of which is realized in a week, is equal to and opposite to ~68% of the 100 year forcing of CO2, and that total integrated aerosol forcing associated with fossil fuel combustion (likewise exerted in a week) is equal to and opposite to ~50% of that 100-year CO2 forcing. Another way of putting this is that the aerosols from emissions over the past week are offsetting 100% of some decades worth of CO2 emissions. In other words the only reason the net forcing from fossil fuel combustion isn't twice as large as it is now is because of the continued emission of aerosols. None of this comes through in the discussion of the figure. If it did, it would have enormous implications. [Stephen E Schwartz]	We agree that it is an intrinsic problem with GWP that it is often understood by policy makers so that to sets of emissions that are equal in terms of CO2 equivalents will give equal climate change (ie. Global temp. Change) at all times and in particular towards the end of the time horizon. This is increasingly wrong when shrt-lived species are compared with long lived. We have tried to make this point clear in the revised section 2.10.1
2-2172	A	67:36	:53	Further comments on Page 2-67, lines 36-53: 1. Technical comment: The results for SO2 have to be highly qualified. Per kg of S, SO2, SO4?. 2. Numerical values cited. I fail to find the several numbers 3.12 mg m –2 , –0.37 W m –2 , and 4.1 days elsewhere in the chapter or in the Kinne (2005) reference. Uncertainties need to be attached to the above numbers. Based on the discussion above re page 36, lines 2-3, I suggest ±40%, for an overall uncertainty of 70% in the AGWP of sulfate aerosol. This uncertainty is comparable to but considerably greater than the uncertainty indicated in Figure 2.10.1, ±5/13 = ±38%, so perhaps some discussion is in order as to the basis for estimation of the uncertainty indicated in that figure. 3. Uncertainty in CO2 emissions shown in Figure 2.10.1. The question is raised as to why there is an uncertainty of roughly 10% on the bar for CO2? Surely the fossil emissions of CO2 are much better known than that. (I trust that this quantity is indeed fossil CO2 emissions. 12/44 * 28 Pg yr -1 = 7.6 Pg yr-1. Yes? This should be stated.) If the uncertainty in the CO2 bar derives from the forcing per CO2 or from uncertainty in the impulse profile of CO2, that uncertainty needs to be placed in the bars for the other substances because they are ratioed to CO2, and not on the bar for CO2 emissions, which	1. Agree, GWPs are per kg of SO2 2. Numbers are taken from revised versions of Tables 2.4.3 and 2.4.4 Agree that uncertainties must be given. 3. According to personal communication with Thomas Boden, teh uncertainty in FF CO2 emissions are +/- 10%. 4. Since GWP100 is more central to policy making through its adaptation for the KP, we keep the order. 5. New refs to correct tables are included. 6. The usefullness of GWPs for short-lived species has now been discussed in the introduction (2.10.1). We agree that SO2 and SO4 predates BC work,

No.	Batch	Page:line From	Page:line To	Comment	Notes
				are known to greater precision, I would think. 4. Language comment: At line 42, the order of the uncertainty range should be inverted so that time horizon 20 precedes time horizon 100 as in the previous sentence: The uncertainty range for the GWP20 estimate is 690–4700 and for the GWP100, 210–1500. 5. References to Tables 2.4.4 and 2.4.5 in the subject paragraphs should be struck; they do not refer to sulfate. 6. The discussion SO2 and sulfate, which predates the more recent work of Bond on BC, should precede the discussion of the BC work.I offer the following language: Although previous IPCC reports have not given GWPs values for aerosols or aerosol precursors, there is no reason in principle why the GWP concept cannot be applied to aerosols and aerosol precursors, provided attention is paid to implication of the greatly differing atmospheric residence times of the aerosols and greenhouse gases on the GWP as a function of time horizon (Schwartz, 1993; Fuglestevdt et al., 2003]. The residence time of the aerosols is about a week whereas that of the greenhouse gases is decades to centuries. This means that the time-integrated forcing of a pulse emission of aerosol (or aerosol precursor), that is the AGWP of this emission, reaches its long-time limit within a few weeks, whereas the time integrated forcing of a pulse emission of a long-lived greenhouse gas, including the reference gas CO2, continues to increase over decades to centuries. A consequence of this is that the GWP of an aerosol (or precursor) (ratio of the AGWP of the aerosol to that of CO2) decreases as the time horizon taken for the integration increases. This fundamental difference between the GWP's of aerosols and LLGHGs limits the utility of GWP's for aerosols and favors the utility of AGWP's for aerosols and for any other short-lived substances whose climate forcing is expressed over a short time period. [Stephen E Schwartz]	however, BC is a heating agent and is thus deemed more policy relevant. Thus the order of the presentation is retained. AGWP vs GWPs for short-lived species, see reply to comment 2171 above.
2-2173	A	67:36	:53	Schwartz (1993) used the AGWP approach to conclude that the strong immediate forcing of sulfate aerosol led to an immediate net cooling influence resulting from the combustion of fossil fuel containing sulfur impurity. The net warming influence of the greenhouse effect of CO2 was found to overcame the cooling influence of the sulfate aerosol only after some 30 to 60 years, depending on assumptions. A global mean GWP for SO2 IT SHOULD BE MADE EXPLICIT THAT THE GWP IS PER KG OF SO2, IF INDEED THAT IS THE CASE from fossil fuel combustion (including only the direct effect of sulphate aerosols) can be estimated based on the model results from the AEROCOM experiments summarized in Table 2.4.3 in this report. Using the modelled global sulphate loading of 3.12 mg m –2 , and all sky RF of –0.37 W m –2 , and a residence time of 4.1 days, UNCERTAINTIES SHOULD BE GIVEN AND REFERENCE SHOULD BE MADE TO THE SOURCES OF THESE QUANTITIES	It has been specified that AGWPs derived for the AR4 report and given in section 2.10.2 is used for the GWPs for the short-lived species. "A global mean GWP for SO2 IT SHOULD BE MADE EXPLICIT THAT THE GWP IS PER KG OF SO2, IF INDEED THAT IS THE CASE" Done

No.	Batch	Page:line From	Page:line To	Comment	Notes
				GWP values of –161, –48, and –15 are estimated for time horizons of 20, 100 and 500 years respectively. In these calculations the AGWP's of CO2 are SPECIFY, respectively. Since the TAR significant progress has been made in the understanding of the radiative effects of aerosols (Section 2.4). Bond and Sun (2005) have calculated GWPs for the direct effect of black carbon aerosols (i.e., neglecting the semi-direct effect and surface albedo effects). Based on previously published results for the lifetime of BC and a normalized RF of 1800 W/g, they derive GWP values of 2200 and 680 for time horizons of 20 and 100 years. The uncertainty range for the GWP20 estimate is 690–4700, and for GWP100 210–1500. The main sources for the relatively large uncertainties are model assumptions about transport and removal of particles and optical properties. Care should be taken when applying GWPs for BC or SO2 since as with other short lived species the GWPs for BC could vary significantly depending on location and time of the emissions. [Stephen E Schwartz]	
2-2174	A	67:36		2.10.2.8 GWP weighted emissions A simple method to compare future climate impacts of current emissions is to multiply current emissions of all climate agents with their GWP values to obtain equivalent CO2 emissions. This is consistent with the Kyoto Protocol through its adoption of GWPs with 100 years time horizon to compare emissions of different climate agents. Figure 2.10.1 shows the equivalent CO2 emissions for all climate agents (or groups of agents) considered in this report. Here the length of the bar for the substances other than CO2 represents the 100-year GWP of present annual emissions expressed as equivalent emissions of CO2. Uncertainties in the estimates of the equivalent CO2 emissions originate both from uncertainties in lifetimes and optical properties (through the GWP values) as well as uncertainties in the current global emissions. It should be noted that the compounds with long lifetimes (in particular CO2) contribute significantly more to the total with this perspective than in the frequently cited "IPCC RF bar- chart diagram" (Figure 3 of the Summary for Policymakers in the TAR, updated in Figure 2.9.1 in this report). The integrated forcing by sulfate aerosol for a pulse emission, all of which is realized within a few weeks following emission, is equal and opposite to ~68% of the 100 year forcing of CO2; similarly, the total integrated aerosol forcing associated with fossil fuel combustion (likewise exerted in a few weeks) is equal and opposite to ~50% of that 100-year CO2 forcing. Another way of putting this is that the aerosols from emissions over the past week are offsetting 100% of some decades worth of CO2 emissions. In other words the only reason the net forcing from fossil fuel combustion isn't twice as great as it is now is the continuing emissions of aerosols and aerosol precursors. Strong caveats and cautions apply when comparing uncertain emissions from the short lived species to those of the LLGHGs; the Kyoto protocol only considers LLGHG	This is just a copy of text from FOD without a comment from the reviewer.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				species. These GWPs have small uncertainties and do not depend on the location of the emission source. Further they are all positive. Decisions on how to treat negative GWPs, GWP variation by source region and uncertain GWPs would need to be made to use these for policy decisions. [Stephen E Schwartz]	
2-2175	A	67:37		see comment 8 above [Alan Rodger]	Comment 8 ??
2-2176	A	67:46	67:50	Since the calculations are not published in the peer reviewed literature it is important that enough information is given here if the reader needs to reproduce the values. Thus, the general equation and chosen input values. Then teh users can apply new/own parameter values. (This applies for SO2 and OC.) [Jan Fuglestvedt]	Agree. All relevant parameters used in the calculations of the GWPs are given in the text.
2-2177	A	67:55	68:12	I think you do the report a disservice by sticking this all the way in the back. If one purpose of the report is to better inform policy-makers, engineers, and the like, who want to know "what to do" (e.g. should I reroute aircraft to reduce contrails or should I invest in lower fuel burn or should I invest in reducing NOx) then looking at integrated future effects of a unit of emissions MUST be given more prominence. As it stands, you have a document that is written by scientists, for scientists. Please raise the bar so that this communicates what is important to people who can do something about the problem. Of course there are a lot of uncertainties once you connect the forcing to the response (and then to the health, welfare and ecological consequences), but the bottom line is that policies are being made today. Airplanes (and other things) are being designed and built today. You can either influence the direction of these programs or not. Stopping at an intermediate measure (RF) that can (and has been) misinterpreted is just as dangerous as producing highly uncertain estimates of reponses. I am not suggesting that we whould stop doing either -- because the problem is so important -- rather I am suggesting that we should give more equal weight to both. [Ian Waitz]	Rejected. But will be referenced at front of document
2-2178	A	68:0		Kieth Shine is a great guy, and a very creative scientist. But, at this end stage of my review of Chapter 2, I really do not wish to be exposed to yet another rather global warming diagnostic... [Jerry Mahlman]	The GTP concept has certain policy implications that helps to put the short-comings of the GWP concept in context. Thus the section is retained.
2-2179	A	68:2	68:12	It seems to me to be very difficult to justify the inclusion of very short lived species and aerosols in figure 2.10.1 based upon the present discussion and the available published literature. I would suggest removing that from this figure and the related discussion. [Susan Solomon]	The justification of including GWPs of short lived species has been extended in the introduction to section 2.10., more details on the derivation of the GWPs are given in section 2.10.4.

No.	Batch	Page:line From	Page:line To	Comment	Notes
					By only including the RF of short-lived species in the "backward looking perspective" of the standard IPCC RF bar chart (fig. 2.9.1), the role of the aerosols can easily be overestimated. At present no other "forward looking" concept than GWP has attained comparable status. The GWPs are simple to calculate based on new published results from a range of models in the AEROCOM experiment and published GWP formulas (eg. Schwarts, 1993). Including the "CO2-equivalnt " emissions in the figure would give policy makers an indication about the potential effects of including these species in future mitigation strategies.
2-2180	A	68:5	68:8	The sentence "It should be noted…" could be deleted. Instead a better explanation of the different perspectives (backward vs forward looking) needs to be given earlier in section 2.9 [Jan Fuglestvedt]	This discussion belongs in section 2.10.2.8 (new section number i SOD). The text in this section is reviesd to make point clearer.
2-2181	A	68:12	68:12	Two recent papers discussing the issue of negative RF and dependence on region could be given here: Rypdal, K, T Berntsen, J Fuglestvedt, A Torvanger, K Aunan, F Stordal and L Nygaard, 2005. Tropospheric ozone and aerosols in climate agreements: scientific and political challenges. Environmental Science and Policy, 8 (1): pp. 29-43. AND: Shine, K P., T Berntsen, J Fuglestvedt and R Sausen, 2005. Scientific issues in the design of metrics for inclusion of oxides of nitrogen in global climate agreements. Proceedings of the National Academy of Sciences (PNAS) (Vol 102, No 44, 15768-15773) [Jan Fuglestvedt]	Accepted.
2-2182	A	68:14	68:14	Figure 2.10.1. is very good and informative. I suggest one additional figure (e.g. 2.10.1.b) for a time horizon equal to 20 years. Choosing TH=100 is a choice of perspective that has a strong effect on our picture of the current man made forces upon climate. To illustrate that this is not the only possible perspective (although it is adopted in the Kyoto Protocol) it would be very good to include a similar figure for TH=20. The choice of time horizon in the Protocol is, to my knowledge, not based on any published conclusive scientific discussion, and it may be argued that a time horizon other than 100 years could be chosen	With more space this would have been a good idea. However, since the appropriate numbers are given in the text and other tables, it will be possibel for readers to derive this picture themselves.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				in the formulation of climate policy in future agreements. It may be expected that this choice has significant implications for the composition of the emission reductions and further on the resulting climate impacts. Thus, an illustration of the effect of this choice would be very useful here. [Jan Fuglestvedt]	
2-2183	A	68:14	68:14	To the extent that space limitations allow this, the GWP values and underlying assumptions should be given. If an Annex if difficult, then at least this could be given in the figure text of figire 2.10.1 (or a footnote). [Jan Fuglestvedt]	The underlying numbers for derivation of the GWPs for the short-lived species are now presented in the text. In the case of indirect effects through ozone, methane lifetime and strat. Water vapor the GWPs are taken directly form studies presented in the litterature.
2-2184	A	68:16	68:16	Perhaps, given some of the misuse of the RFI as an emissions metric, some comment ought to be made about it? [Keith Shine]	Accepted.
2-2185	A	68:20	68:21	This statement regarding the GWP metric should be expanded. I agree that GWP does not accurately reflect the effects of short-lived gases or aerosols, but these effects should be outlined. The metrics discussed in the rest of this section do not improve upon the GWP in representing these short-lived species, either. [Tami Bond]	The problems using GWPs for short.lived species have now been discussed in the introduction (last paragraph of Section 2.10.1). The discussion of the GTP metric, which shift the focus towards long term effects put reduced emphasis on short lived species compared to the GWP. This was in the FOD (page 69, line 3) but has been made clearer in the SOD.
2-2186	A	68:20	68:21	The wording "..the GWP is not well suited" may sound a bit too strong since the GWPs are used in fig 2.10.1. "Problematic" could be a better word here [Jan Fuglestvedt]	Agree. Text changed.
2-2187	A	68:32	68:32	The word "committing" may be too strong. I suggest "assuming that policy makers will use …" instead. [Jan Fuglestvedt]	The whole section discussing pulse vs. Sustained GWPs has been removed.
2-2188	A	68:36	68:38	Define 'considerably' in this context. My understanding is that the variation in efficacy is often lower than the uncertainty in location, for example. (At any rate, it is not an order of magnitude.) [Tami Bond]	The word considerably is removed and a quantative number is given.
2-2783	B	68:36	68:36	"considerably": is that consistent with the message brought in section 2.8.5? [Olivier Boucher]	OK, see comment 2782

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2189	A	68:39	68:39	'Shout East Asia' is presumably 'South East Asia' [Tami Bond]	OK
2-2190	A	68:39	68:39	Shout should be South here. [Drew Shindell]	OK
2-2191	A	68:39		correct: "Shout" to "South" [Hartmut Grassl]	OK
2-2192	A	68:43	69:14	In my opinion, the material presented in chapter 2.10.3.2 does not well reflect the policy-relevant scientific studies that have been published on this topic. The material is not comprehensive, in particular concerning alternative metrics that uses an economic approach. See several papers in Climatic Change 58, 2003 and, most importantly, references therein. I also think the material is not well balanced. Why have the authors chosen to devote more than twice as much space on the GTP study, section 2.10.3.2.1 as compared to all economic approaches on this subject (section 2.10.3.2.2)? [Odd Godal]	Agree. Since our FOD was written, the FOD of AR4 from WGIII has been out for review. The metrics based on econmics are discussed in that report, and thus section 2.10.3.2.2 have been deleted, and a reference to WGIII has been included in the introduction.
2-2193	A	68:43		It is not clear whether the Working Group is recommending Global Temperature Potential to supplant GWP. An advantage of warming potentials based on forcing is that they are readily evaluated and compared. A disadvantage, not noted, of the GTP is the requirement that it takes a run (or more likely an ensemble of runs) with one or more coupled ocean atmosphere models to determine the GTP. I suggest that other than providing job security for the coupled AOGCM community carrying out such runs for all the compounds listed in Table 2.10.1 would not be worth the effort. If there should be any movement toward the GTP, let me offer the hope that the results be expressed as absolute GTP's, not ratioed to the AGTP of a reference material. But perhaps better that the Working Group squelch this early. Maybe the whole concept should be dismissed. [Stephen E Schwartz]	A recommandation of any given metrci would have to be based (among other issues) on how well it provides equivalence between a chosen climate impact (i.e. an interpretation of what is dangerous interference of the climate system). As long as this does not exist we are not in a position to make a definite recommendation of one metric. However, we believe that readers of the IPCC report should be made aware that there are alternatives to the GWP. The GTP does not require AOGCM runs (see Shine et al. 2005a), mainly because the poorly known climate sensitivity is assumed to affect the numerator and the denominator equally (as for GWPs). A comment of this is included in the text.
2-2194	A	68:44		The definition for GTP should be emphasized [MARCOS S. P. GOMES]	The GTP is defined through an equation. For more details on hwo to

No.	Batch	Page:line From	To	Comment	Notes
					calculate GTPs the readers are refered to Shine et al. 2005.
2-2195	A	68:47		year missing for Shine et al. [Cathy Clerbaux]	OK, ref to Shine et al. In this sentence has been removed.
2-2196	A	68:51	68:51	I suggest inserting the following after "compound x": The GTP's are given as transparent and simple formulaes with a small number of input paraters required for calculation. [Jan Fuglestvedt]	OK.
2-2784	B	69:1	69:1	Delete "be". Isn't climate efficacy already included in that metrics?? [Olivier Boucher]	In principle Yes, but the simple formulas applied by Shine et al. (2005) did not include variations in the efficacy.
2-2197	A	69:1		delete: "be" before "easily" [Hartmut Grassl]	OK
2-2198	A	69:5	69:5	I suggest adding " and thus gives an interpretation of what the GWPs does" after "GWP" at the start of line 5. [Jan Fuglestvedt]	We find that this clearification is not needed.
2-2199	A	69:8	69:14	It is an improvemnet that alternative approaches from outside the field of natural sciencs are given. However this section is too short and a better explanation of the work by Manne and Richels is necessary. Furthermore, there are also other main works in the literature that should be cited; e.g. by Kandlikar. [Jan Fuglestvedt]	Section deleted. Cf. Reply to comment 2192
2-2200	A	69:8	69:14	I suggest a link to relevant sections in WG II and WGIII. [Jan Fuglestvedt]	OK, included in section 2.10.1
2-2201	A	69:8		It seems that section 2.10.3.2.2 is a bit out of place - the discussion is interesting but there is not enough information on the proposed approach. It could be presented in more detail in WG2 or WG3 [MARCOS S. P. GOMES]	Section deleted. Cf. Reply to comment 2192
2-2202	A	69:8	:14	GWP values for CH4 are additionally affected by and must therefore take into consideration the background concentrations of non-GHG reactive gases CO, NOx and OH that are a function of the emissions scenario assumed (Hayhoe, K., A. Jain, H. Kheshgi and D. Wuebbles. 2000. "Contribution of CH4 to Multi-Gas Reduction Targets The Impact of Atmospheric Chemistry on GWPs", Non-CO2 Greenhouse Gases: Scientific Understanding, Control and Implementation, J. van Ham (Ed.), p. 425-432. [Katharine Hayhoe]	It seems taht this comment is relevant for section 2.10.2.1 (in the FOD). We agree with the point made, but since there are no update on indirect GWPs for methane since the TAR, and this point has been discussed in previous IPCC reports, we do not include this discussion in the 4AR.
2-2203	A	69:8		Help! Give us all a break and throw this out before someone argues that it is a truly	Section deleted. Cf. Reply to comment

No.	Batch	Page:line From	Page:line To	Comment	Notes
				relevant concept for this Chapter 2 assessment. [Jerry Mahlman]	2192
2-2204	A	69:13		correct: "value" to "values" [Hartmut Grassl]	Section deleted
2-2205	A	70:0	70:	In the line 63 there are the words "in press" with highligthing. [NADIA GAMBOA]	They should be
2-2206	A	70:1		References: add Oltmans, S. and 21 others (2005) Long-term changes in tropospheric ozone, Atmos. Environ., submitted. [Anne Thompson]	accepted
2-2207	A	70:1		References: add Richter, A., Burrows, J., Nüb, H., Granier, C., and Niemeier, U. (2005) Increase in tropospheric nitrogen dioxide over China observed from space, Nature, 437, 129-132. [Anne Thompson]	Rejected, too local
2-2208	A	70:1		References: add Staehelin, J., Thudium, J., Buehler, R., Volz-Thomas, A., and Graber, W. (1994) Trends in surface ozone concentrations at Arosa (Switzerland), Atmos. Environ., 28, 75-87. [Anne Thompson]	Rejected, too old, cited in TAR
2-2209	A	70:1		References: add A. M. Thompson, J. C. Witte, R. D. McPeters, S. J. Oltmans, F. J. Schmidlin, J. A. Logan, M. Fujiwara, V. W. J. H. Kirchhoff, F. Posny, G. J. R. Coetzee, B. Hoegger, S. Kawakami, T. Ogawa, B. J. Johnson, H. Vömel, G. Labow, Southern Hemisphere Additional Ozonesondes (SHADOZ) 1998-2000 tropical ozone climatology. 1. Comparison with TOMS and ground-based measurements, J. Geophys. Res., 108, 8238, doi: 10.129/2001JD000967, 2003. [Anne Thompson]	Rejected, beyond scope of chapter
2-2210	A	70:1		References: add R. D. Diab, A. Raghunandran, A. M. Thompson, V. Thouret, Classification of tropo- spheric ozone profiles over Johannesburg based on MOZAIC aircraft data, Atmos. Chem. Phys., 3, 713-723, 2003. [Anne Thompson]	Rejected too detailed
2-2211	A	70:1		References: add R. D. Diab, A. M. Thompson, K. Mari, L. Ramsay, G. J. R. Coetzee, Tropospheric ozone climatology over Irene, South Africa from 1990-1994 and 1998-2002, J, Geophys. Res., 109, D20, D20301, doi: 10.129/2004JD004293, 2004. [Anne Thompson]	Rejected, not relevent to forcing
2-2212	A	70:1		References: add S. Solomon, D. W. J. Thompson, R. W. Portmann, S. J. Oltmans, and A. M. Thompson, On the distribution and variability of ozone in the tropical upper troposphere: Implications for tropical deep convection and chemical-dynamical coupling, Geophys. Res. Lett., in press, 2005.	Rejected, beyond scope of chapter

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Anne Thompson]	
2-2785	B	70:17	70:17	Delete (Paper xxx) [Olivier Boucher]	accepted
2-2213	A	70:25	70:31	Two references by Anderson et al. are given, but two others are missing. The missing references are Anderson et al. (2003b) and Anderson et al. (2005b). The former is already cited in the Chapter (page 41, line 7) and the latter should be cited in the discussions of satellite aerosol retrievals of fine-mode fraction, as indicated in my comments above. These references are: Anderson, T. L., R. J. Charlson, S. E. Schwartz, R. Knutti, O. Boucher, H. Rodhe and J. Heintzenberg (2003b) Climate forcing by aerosols - A hazy picture, Science, 300, 1103-1104. Anderson, T. L., Y. Wu, D. A. Chu, B. Schmid, J. Redemann and O. Dubovik (2005b) Testing the MODIS satellite retrieval of aerosol fine-mode fraction, J. Geophys. Res., 110, doi:10.1029/2005JD005978. [Theodore Anderson]	accepted
2-2214	A	70:31	70:31	The words "in press" are highlighted. [NADIA GAMBOA]	noted
2-2215	A	70:38	70:39	Add the reference: Bakan, S., et al., Contrail frequency over Europe from NOAA-satellite images, Ann. Geophys., 12, 962-968, 1994. [Mikhail Danilin]	rejected
2-2216	A	70:54	70:54	CH4 must be written with 4 as subscript. [NADIA GAMBOA]	accepted
2-2786	B	70:57	70:57	The paper is in press now. I think its conclusion should be assessed more than it is at the moment and put in perspective with the model estimates; [Olivier Boucher]	accepted
2-2217	A	70:57	70:57	The words "in revision" are highlighted. [NADIA GAMBOA]	accepted
2-2787	B	70:58	70:58	Tanré [Olivier Boucher]	accepted
2-2218	A	71:3	71:3	The word "accepted" is highlighted. [NADIA GAMBOA]	accepted
2-2219	A	71:16	71:16	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2220	A	71:32	71:32	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2221	A	72:10	72:10	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2788	B	72:51	72:51	Defresne should be Dufresne [Olivier Boucher]	accepted
2-2222	A	72:54	72:54	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2789	B	73:11	73:11	thourhgout --> throughout [Olivier Boucher]	accepted
2-2223	A	73:31	73:32	Add the reference: Danilin, M.Y., et al., Aviation fuel tracer simulation: Model intercomparison and implications, Geophys. Res. Lett., 25, 3947-3950, 1998. [Mikhail Danilin]	rejected
2-2224	A	73:41	73:41	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2225	A	74:3	74:3	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2790	B	74:10	74:10	Tanré [Olivier Boucher]	accepted
2-2791	B	74:23	74:23	inthe --> in the [Olivier Boucher]	accepted
2-2226	A	74:39	74:39	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2227	A	74:41		Add to the References : Facchini, M.C., M. Mircea, S. Fuzzi and R.J. Charlson (1999) Cloud albedo enhancement by surface-active organic solutes in growing droplets. Nature, 401, 257-259. [MARIA CRISTINA FACCHINI]	rejected
2-2228	A	74:52	74:52	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2229	A	75:2	75:2	The words "in press" are highlighted. [NADIA GAMBOA]	noted
2-2230	A	75:41		ADD to the references : Fuzzi, S., S. Decesari, M.C. Facchini, E. Matta, M. Mircea and E. Tagliavini (2001) A symplified model of the water soluble organc component of atmospheric aerosol. Geophys. Res. Lett. 20, 4079-4082 [MARIA CRISTINA FACCHINI]	rejected
2-2231	A	75:50	75:50	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2232	A	75:52	75:52	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2792	B	75:59	75:59	Delete (Paper xxx) [Olivier Boucher]	accepted
2-2233	A	76:0	76:	In the line 63 there are the words "in press" with highligthing. [NADIA GAMBOA]	accepted
2-2793	B	76:5	76:5	Tanré Deuzé Bréon [Olivier Boucher]	accepted
2-2794	B	76:11	76:13	2001a and 2001b [Olivier Boucher]	accepted
2-2234	A	76:14	76:14	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2795	B	76:16	76:16	Office [Olivier Boucher]	accepted
2-2796	B	76:18	76:18	chracteristics --> characteristics [Olivier Boucher]	accepted
2-2797	B	76:55	76:55	Delete second occurrence of Perlwitz [Olivier Boucher]	accepted
2-2235	A	76:57	76:57	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2236	A	77:59	77:59	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2798	B	78:24	78:24	Tanré [Olivier Boucher]	accepted
2-2237	A	78:44		Acadamies >> Academies???? [Cathy Clerbaux]	accepted
2-2799	B	78:48	78:48	effective [Olivier Boucher]	accepted
2-2238	A	78:48		correct: "efective" to "effective" [Hartmut Grassl]	accepted
2-2239	A	78:56		Papers cited here to be added to the references for Chapter 2. Jeong, M.-J., Z. Li, 2005: Quality, Compatibility and Synergy Analyses of Global Aerosol Products derived from the Advanced Very High Resolution Radiometers and Total Ozone Mapping Spectrometers,, J. Geophy. Res., 110, D10S08, doi:10.1029/ 2004JD004647. Jeong, M.J , Z. Li, D.A. Chu, and S-T. Tsay, 2005: Quality and Compatibility Analyses of	Rejected

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Global Aerosol Products Derived from the Advanced Very High Resolution Radiometers and the Moderate Imaging Spectroradiometer, J. Geophy. Res., 110, D10S09, doi:10.1029/2004JD004648 Li, Z., and L. Kou, 1998: Atmospheric direct radiative forcing by smoke aerosols determined from satellite and surface measurements, Tellus (B), 50, 543-554. Li, Z., A. Trishchenko, 2001: Quantifying the uncertainties in determining SW cloud radiative forcing and cloud absorption due to variability in atmospheric condition, J. Atmos. Sci., 58, 376-389. Taubman, B.A., L. Marufu, B. Vant-Hull, C. Piety, B. Doddridge, R. Dickerson, Z. Li, 2004: Smoke Over Haze: Aircraft Observations of Chemical and Optical Properties and the Effects on Heating Rates and Stability. J. Geophy. Res., 109, D02206, doi: 10.1029/2003JD003898. Vant-Hull, B., Z. Li, B.F. Taubman, R. Levy, L. Marufu, F.L. Chang, B.D. Doddridge, R.D. Dickerson, 2005: Smoke over Haze: Comparative Analysis of Satellite, Surface Radiometer and Airborne In-situ Measurements of Aerosol Optical Properties and Radiative Forcing over the Eastern US, J. Geophy. Res., D10S21, doi:10.1029/2004JD004518. [Zhanqing Li]	
2-2800	B	79:31	79:31	Tanré [Olivier Boucher]	accepted
2-2801	B	79:35	79:35	Tam --> Tanré [Olivier Boucher]	accepted
2-2802	B	79:37	79:39	There is some confusion between Kaufman et al, PNAS, 2005 (cited incorrectly as ACPD) and Kaufman et al, GRL, 2005 [Olivier Boucher]	accepted
2-2240	A	79:37		the actual publication is in "Proc. Nat. Acad. Scie., 102, 11207-11212, 2005 " In the text in several locations there is a reference to adifferent Kaufman et al 2005 reference namely: Kaufman Y. J., O. Boucher, D. Tanré, M. Chin, L. A. Remer & T. Takemura, Aerosol anthropogenic component estimated from satellite data, Geoph. Res. Lett., VOL. 32, L17804, doi:10.1029/2005GL023125, 2005 [Yoram Kaufman]	accepted
2-2241	A	79:39	79:39	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2242	A	79:40	79:40	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2243	A	80:11	80:11	The word "submitted" is highlighted.	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[NADIA GAMBOA]	
2-2244	A	80:22	80:23	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2245	A	80:22	80:23	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2803	B	80:26	80:26	Include Koren et al. GRL 2005 [Olivier Boucher]	accepted
2-2246	A	80:30	80:30	Missing reference, referred to in Table 2.4.6 on page 102 and in Figure 2.4.4 on page 127: Kristjánsson, J. E., 2002: Studies of the aerosol indirect effect from sulfate and black carbon aerosols. J. Geophys. Res., 107, D15, 4246, doi:10.1029/2001JD000887. [Jón Egill Kristjánsson]	accepted
2-2247	A	80:32	80:32	N2O must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2248	A	80:35	80:35	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2249	A	80:53	80:53	CO2 and CH4 must be written with 2 and 4 as subscripts respectively. [NADIA GAMBOA]	accepted
2-2250	A	80:53	80:53	CH4 must be written with 4 as subscript. [NADIA GAMBOA]	accepted
2-2251	A	80:56	80:56	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2252	A	81:2	81:2	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2253	A	81:14	81:15	Add the reference: Lewellen, D.C., and W.S. Lewellen, The effects of aircraft wake dynamics on contrail development, J. Atmos. Sci., 58, 390-406, 2001. [Mikhail Danilin]	accepted
2-2254	A	81:18	81:18	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2804	B	81:30	81:31	update [Olivier Boucher]	accepted
2-2255	A	81:31	81:31	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2256	A	82:11	82:11	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2257	A	82:16	82:16	The word "submitted" is highlighted.	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[NADIA GAMBOA]	
2-2258	A	82:23	82:23	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2805	B	82:30	82:30	frocing [Olivier Boucher]	accepted
2-2259	A	82:31	82:31	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2260	A	82:54	82:54	Matthias should be replaced with Mattis. [Felicita Russo]	accepted
2-2261	A	82:60	82:60	(This comment actually applies to lines 61-62 to which this Excel sheet forbids access). Check spelling of author's name: MacFarling Meure? Correct as necessary in citations in the text and in figure captions. [Keith Lassey]	accepted
2-2262	A	82:60	82:60	(This comment actually applies to lines 61-62 to which this Excel sheet forbids access). Identify this reference as a PhD thesis. [Keith Lassey]	accepted
2-2263	A	83:3	83:3	The words "in press" are highlighted. [NADIA GAMBOA]	noted
2-2264	A	83:8	83:9	Add the reference: Meerkoetter, R., et al., Radiative forcing by contrails, Ann. Geophys., 17, 1080-1094, 1999. [Mikhail Danilin]	Rejected
2-2806	B	83:9	83:9	Delete (Paper xxx) [Olivier Boucher]	Accepted
2-2265	A	83:28	83:28	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	Noted
2-2266	A	83:30	88:14	References for above comment: Mikami, M., G.-Y. Shi, I. Uno, S. Yabuki, Y. Iwasaka, M. Yasui, Te. Aoki, T.Y. Tanaka, Y. Kurosaki, K. Masuda, A. Uchiyama, A. Matsuki, T. Sakai, T. Takemi, M. Nakawo, N. Seino, M. Ishizuka, S. Satake, K. Fujita, Y. Hara, K. Kai, S. Kanayama, M. Hayashi, M. Du, Y. Kanai, Y. Yamada, X.-Y. Zhang, Z. Shen, H. Zhou, O. Abe, T. Nagai, Y. Tsutsumi, M. Chiba, and J. Suzuki, 2005: Aeolian Dust Experiment on Climate Impact: An Overview of Japan-China Joint Project ADEC, Global Planetary Change, accepted. Murayama, T., 2001: Formation of ice cloud from Asian dust particles in the upper troposphere. Proc. of SPIE. 4153, 218-225. Sakai, T., Nagai, T., Nakazato, M., and Matsumura, T., 2004: Raman lidar measurement of water vapor and ice clouds associated with Asian dust layer over Tsukuba, Japan.	reejcted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Geophys. Res. Lett. 31, L06128, doi:10.1029/2003GL019332. Sassen, K., 2005: Dusty ice clouds over Alaska, Nature, 434, 456. [Masao Mikami]	
2-2267	A	83:34	83:34	The word "submitted" is highlighted. [NADIA GAMBOA]	noted
2-2268	A	83:37	86:37	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2807	B	83:39	83:47	2005a/b/c/d , update text accordingly [Olivier Boucher]	accepted
2-2269	A	83:39	83:47	There are two Ming et al. 2005a and two 2005b, change to a,b,c,d [Reto Knutti]	accepted
2-2270	A	83:40	83:40	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2271	A	83:42	83:42	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2272	A	83:44	83:44	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2273	A	83:47	83:47	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2274	A	83:48	83:48	Add the reference: Minnis, P., Reply, J. Climate, 18, 2783-2784,2005. [Mikhail Danilin]	accepted
2-2275	A	84:10	84:10	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2276	A	84:12	84:12	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2277	A	84:20	84:20	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2278	A	84:34	84:36	The reference "Nakajima et al., (1996) is wrong. The paper that should be quoted in page 27 line 26 is "Nakajima et al., 1996: Use of sky brightness measurements from ground for remote sensing of particulate polydispersions. Appl. Opt., 35, 2672-2686. [Tadahiro Hayasaka]	accepted
2-2279	A	84:41		Add to the References : Nenes, A., R. Charlson, M.C. Facchini, M. Kulmala, A. Laaksonen and J. H. Seinfeld (2002): Can chemical effects on cloud droplet number rival the first indirect efffect? Geophys. Res. Lett., 29, 1848, doi:10.1029/2002GL015295. [MARIA CRISTINA FACCHINI]	reejcted

No.	Batch	Page:line From	To	Comment	Notes
2-2280	A	84:46	84:46	"N2O and O2/N2": the subscripts must be written. [NADIA GAMBOA]	accepted
2-2808	B	85:41	85:41	May I suggest that you include and discuss Pham et al, Changes in atmospheric sulfur burdens and concentrations and resulting radiative forcings under IPCC SRES emission scenarios for 1990–2100, JGR, 2005 here [Olivier Boucher]	accepted
2-2281	A	85:42	85:43	Please add the following reference: Philipona, R., B. Dürr, A. Ohmura, and C. Ruckstuhl, 2005: Anthropogenic greenhouse forcing and strong water vapor feedback increase temperature in Europe. Geophys. Res. Lett., 32, L19809, doi:10.1029/2005GL023624. [Rolf Philipona]	reejkcted
2-2809	B	85:44	85:44	radioactive --> radiative [Olivier Boucher]	accepted
2-2810	B	86:27	86:27	May I suggest that you include and discuss Quaas J., O. Boucher (2005), Constraining the first aerosol indirect radiative forcing in the LMDZ GCM using POLDER and MODIS satellite data, Geophys. Res. Lett., 32, L17814, doi:10.1029/2005GL023850 as well. [Olivier Boucher]	accepted
2-2282	A	86:28	86:28	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2811	B	86:49	86:49	delete first occurrence of page numbers [Olivier Boucher]	accepted
2-2283	A	86:54	86:54	The words "Vol. in press" are highlighted. [NADIA GAMBOA]	accepted
2-2812	B	87:18	87:18	and et al [Olivier Boucher]	accepted
2-2284	A	87:44	87:44	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2285	A	87:60	87:60	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2286	A	88:18	88:18	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2287	A	88:22	88:23	Please note that the (final) author list of Sausen et al. 2005 is incomplete as given here. Some additional authors were added in the last stages of the paper [David Lee]	accepted
2-2288	A	88:32	88:32	The words "in press" are highlighted. [NADIA GAMBOA]	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2289	A	88:35	88:35	change authors to 'Schulz. M, S. Kinne, S. Guibert, C. Textor' [Christiane Textor]	accepted
2-2290	A	88:39	88:39	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2291	A	88:40	88:41	add the reference: Schumann, U., Aviation, atmosphere and climate - what has been learned, in Proced. The AAC conference (Friedrichshafen, Germany, 2003), Air pollution research report 83, 349-355, 2004. [Mikhail Danilin]	rejected
2-2813	B	88:46	88:46	Sekiguchi et al (JGR, 2003) is missed here. [Olivier Boucher]	accepted
2-2292	A	88:47	88:47	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2814	B	88:57	88:57	present-day [Olivier Boucher]	accepted
2-2293	A	89:3	89:3	Update the following reference: Shine, K., Comments on Contrails, Cirrus Trends, and Climate", J.Climate, 18, 2781-2782, 2005. [Mikhail Danilin]	accepted
2-2294	A	89:3	89:3	The words "in press" are highlighted. [NADIA GAMBOA]	noted
2-2295	A	89:19	89:19	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2296	A	89:20	89:20	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2297	A	89:22	89:22	The words "in press" are highlighted. [NADIA GAMBOA]	accepted
2-2815	B	89:27	89:27	Tanré [Olivier Boucher]	accepted
2-2298	A	89:38	89:38	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2816	B	89:43	89:43	radioactive --> radiative [Olivier Boucher]	accepted
2-2817	B	89:47	89:47	Delete "Discuss". It is published in ACP not ACD. Page and volume numbers are correct. [Olivier Boucher]	accepted
2-2299	A	89:51	89:51	The word "accepted" is highlighted. [NADIA GAMBOA]	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2300	A	89:53	89:53	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2818	B	89:54	89:54	and e. al [Olivier Boucher]	accepted
2-2301	A	89:57	89:58	Add the reference: Stubenrauch, C.J. and U. Schumann, Impact of air traffic on cirrus coverage, Geophys. Res. Lett., 32, L14813, doi.10.1029/2005GL022707, 2005. [Mikhail Danilin]	Accepted
2-2302	A	90:5	90:6	Add the reference: Sussman, R., and K.M. Gierens, Differences in early contrail evolution of two-engine versus four-engine aircraft: Lidar measurements and numerical simulations, J. Geophys, Res., 106, 4899-4911, 2001. [Mikhail Danilin]	Rejected
2-2303	A	90:55	90:55	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2819	B	91:15	91:15	Delete "ATMOS .. SCIENCE" [Olivier Boucher]	accepted
2-2304	A	91:48	91:48	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2305	A	91:50	91:50	CO2 must be written with 2 as subscript. [NADIA GAMBOA]	accepted
2-2820	B	92:8	92:8	Include Wild et al Science 2005 [Olivier Boucher]	accepted
2-2821	B	92:41	92:41	update [Olivier Boucher]	accepted
2-2306	A	92:41	92:41	The word "submitted" is highlighted. [NADIA GAMBOA]	accepted
2-2307	A	92:51	92:52	Add the reference: Zhang, Y., A.Macke, and F.Alberts, Effects of crystal size spectrum and crystal shape on stratiform cirrus radiative forcing, Atmos. Res., 52, 59-75, 1999. [Mikhail Danilin]	rejected
2-2308	A	93:1	93:28	Perhaps the first question to be asked and answered is: What is the greenhouse effect? Following a physical descriptiion of the greenhouse effect it will become obvious that any gas that absorbs thermal radiation is a greenhouse gas, and that its ability to contribute to the greenhouse effect depends on the strength of its absorption bands, its atmospheric concentration, and its local temperature difference with the ground surface. [Andrew Lacis]	Noted: Box 1 deleted because material included in Q1.3
2-2309	A	93:1		Question 2.1: (Suggestion from DW, since DF was author)This answer is written clearly and at a good level. However it may be stretching the guideline length of 2 IPCC pages	Accepted: Box 1 deleted because material included in Q1.3

No.	Batch	Page:line From	Page:line To	Comment	Notes
				for text plus figure. Since the ZOD was prepared a new question 1.3 on "What is the natural greenhouse effect" has been added, and that also contains some discussion of what a greenhouse gas is, and what the greenhouse effect is. Perhaps Box 1 of Question 2.1 could be dropped, and a reference made instead to Question 1.3, as a way of reducing the length of Q2.1 ? [David & David Wratt & Fahey]	
2-2310	A	93:4		replace: "increasing" by "changing" [Hartmut Grassl]	Accepted
2-2311	A	93:5		Insert after "contribution" the words "is thought to" [Vincent Gray]	Noted: Changed to 'known contribution'
2-2312	A	93:10		Insert after "is" "thought to be" and after "that", "probably" [Vincent Gray]	Noted: Changed to 'warming influence' and 'known changes'
2-2313	A	93:16	93:19	comment: wrong ranking [Hartmut Grassl]	Noted: Gases aren't ranked
2-2314	A	93:16	93:23	These definitions conflict. For consistency, a greenhouse gas must be defined as one which absorbs IR but not visible light (so O3 is not one), or the definition of greenhouse effect must be revised. [Howard Roscoe]	Noted: Section moved to Q1.3
2-2315	A	93:16		To really be a greenhouse gas, it must not only absorb infrared radiation but do so more efficiently than it absorbs solar radiation. [Mark Lawrence]	Noted: Section moved to Q1.3
2-2316	A	93:21	93:27	Everything in the universe that has a definable temperature emits radiation at the maximum allowable rate as prescribed by the Planck function. For solid surfaces, this thermal emission is proportional to T**4 as given by the Stefan-Boltzmann law. Tenuous substances, such as gases, also emit radiation according to the Planck law, but the thermal emission can occur only at those wavelengths at which the gas absorbs, and in proportion to the absosrptivity of the gas at those wavelengths. In general, the atmospheric greenhouse effect works like thermal insulation around a hot steam pipe to reduce the rate of heat energy escape from the steam pipe. The only significant difference is that thermal insulation restricts energy transport carried by conduction, while absorbing gases in the atmosphere restrict energy transport by radiation. However, because of the fluid nature of the atmosphere, heat energy can also be transported by convective means, which happens when atmospheric temperature gradients become too steep. [Andrew Lacis]	Noted: Section moved to Q1.3
2-2317	A	93:23		Delete "naturally" [Vincent Gray]	Noted: Section moved to Q1.3

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2318	A	93:24		Replace "balance" with "difference" [Vincent Gray]	Noted: Section moved to Q1.3
2-2319	A	93:26		delete: "expected to" and correct: "lead" to "leads" [Hartmut Grassl]	Noted: Section moved to Q1.3
2-2320	A	93:27	93:27	A necessary condition for greenhouse operation is that a significant fraction of the absorbed solar energy occurs at the ground, as is the case for Earth. In the process of transporting the absorbed solar energy out to space, a monotonic temperature gradient is established within the atmosphere. This is because layers near the top of the atmosphere can not be supplied with as much thermal energy from neighboring layers as layers within the deeper atmosphere. For global energy balance, the layers at the top of the atmosphere, from which energy gets radiated out to space, must maintain an effective temperature that is capable of radiating away the absorbed solar energy. As a result of the atmospheric temperature gradient, temperatures at ground surface and at the bottom of the atmosphere will be warmer than the effective radiating temperature of the top atmospheric layers. The temperature difference between the surface temperature and the effective radiating temperature is a measure of the strength of the atmospheric greenhouse effect. [Andrew Lacis]	Noted: Section moved to Q1.3
2-2321	A	93:28	93:28	For the Earth, which reflects about 30% of the incident solar radiation, the effective radiating temperature has be about 255 K (or -18C). Since the global mean surface temperature of the Earth is approximately 288 K, the greenhouse strength of the Earth's atmosphere is about 33 K. The strength of the atmospheric greenhouse effect is determined by the thermal opacity of the atmosphere. Competitive energy transport by convection acts to diminish the atmosphere's greenhouse efficiency. (If it were not for convection, the surface temperature of the Earth would be some 20C warmer than it actually is.) To a lesser extent, the stratospheric absorption of solar radiation by ozone also tends to diminish the efficiency of the Earth's greenhouse effect. [Andrew Lacis]	Noted: Section moved to Q1.3
2-2322	A	93:29	93:31	comment: wrong, please delete [Hartmut Grassl]	Noted: Section moved to Q1.3
2-2323	A	93:29	93:31	The statement that the greenhouse effect is a misnomer is trite, pointless, inaccurate, and totally unnecessary. First, there are many words in the English language that take on specific technical meanings that are significantly different from their common everyday meaning when the words are used in a technical sense. Second, the atmosphere actually does have a "lid" that stops energy transport by convection. It is well understood that there is effectively zero energy that is convected from the ground through the stratosphere to outer space. Third, many modern greenhouses use plastic sheeting which lets in solar radiation but also absorbs outgoing thermal. Hence, this paragraph is inaccurate,	Noted: Section moved to Q1.3

No.	Batch	Page:line From	Page:line To	Comment	Notes
				contributes to confusion, and otherwise serves no useful purpose, and should therefore be deleted. Effort should instead be directected toward ensuring that there is a sound technical definition presented for the greenhouse effect. [Andrew Lacis]	
2-2324	A	93:29	:31	The term greenhouse effect as used here is a misnomer because the walls of a true greenhouse pass both solar and infrared radiation. An actual greenhouse warms by trapping air within its boundaries so as not to lose the warming from solar radiation to the surrounding air. The statement that "the walls of a true greenhouse pass both solar and infrared radiation" while strictly true (they do pass some infrared) is misleading; in fact the glass walls absorb much of the thermal infrared. That does not detract from the fact that most of the action of the greenhouse is to prevent convection. But no sense diminishing the credibility of IPCC the report by such statements. See Wood, 1909 R. W. Wood: Note on the Theory of the Greenhouse, Philosophical magazine (more properly the London, Edinburgh and Dublin Philosophical Magazine and Journal of Science), 1909, vol 17, p 319-320. [Stephen E Schwartz]	Noted: Section moved to Q1.3
2-2325	A	93:30	93:31	"An actual greenhouse warms by trapping air within its boundaries so as not to lose the warming from solar radiation to the surrounding air." Trapping air alone cannot prevent the greenhouse from losing warming. [Xiaobin Xu]	Noted: Section moved to Q1.3
2-2326	A	93:31		actually the energy is lost more to the overlying air (dry convective mixing) than to the surrounding air in most cases [Mark Lawrence]	Noted: Section moved to Q1.3
2-2327	A	93:35		It needs to be explained how emissions of greenhouse gases lead to concentrations of greenhouse gases. The radiative forcing is due to concentrations, not emissions. [Vincent Gray]	Accepted: sentence added
2-2328	A	93:36	93:40	Add H2O as a principal greenhouse gas of human emissions on lines 36 and 40. [Mikhail Danilin]	Rejected: incorrect
2-2329	A	93:36		Replace "four" with "five"; insert after "gases", " water vapour (H2O)" [Vincent Gray]	Rejected: water vapor is discussed separately in answer
2-2330	A	93:39		Insert after "activities" Methane concentrations are currently falling" [Vincent Gray]	Accepted
2-2331	A	93:39	:41	Fossil fuel use in transportation, building heating and cooling, and the manufacture of cement and other goods has increased carbon dioxide. Deforestation has also increased carbon dioxide by reducing the total uptake of carbon dioxide by plants. [Stephen E Schwartz]	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2332	A	93:44		Insert after 1750) "Changes in water vapour emissions due to human activities are currently not adequately measured or understood" [Vincent Gray]	Rejected: Unnecessary detail
2-2333	A	93:54	94:5	It should be emphasized that radiative forcing by ozone is especially sensitive to the height in the atmosphere where the ozone change takes place. An increase in ozone high in the stratosphere results in surface cooling because the ozone absorbs solar UV radiation that would otherwise be atsorbed in the troposphere. The ozone greenhouse effect is strongest for ozone added near the tropopause where the local temperatures have the greatest contrast with the surface temperature. [Andrew Lacis]	Accepted: statement about different role of ozone in stratosphere and troposphere added.
2-2334	A	93:55		"Ozone…is not emitted directly into the atmosphere *in substantial amounts*" - it really is emitted directly by a few minor processes (sparks and corona) [Mark Lawrence]	Rejected Thought not needed in this context.
2-2335	A	94:4		reword "release carbon monoxide…" to "release gases such as carbon monoxide, hydrocarbons, and nitrogen oxides" [Mark Lawrence]	Accepted
2-2336	A	94:7	93:12	It should be emphasized that water vapor amounts in the atmosphere do not change arbitrarily, but rather are governed by the temperature sensitive Clausius-Clapeyron equation. Because of this thermodynamic relationship, a warmer atmosphere holds more water vapor, and a colder atmosphere holds less water vapor. As a result, changes in atmospheric water vapor in response to atmospheric temperature changes act to contribute a strong positive feedback to magnify the initial temperature perturbation. And of course, water vapor is also the source for cloud formation which can produce additional positive and negative feedbacks. [Andrew Lacis]	Accepted Comment added about water in a warmer atmosphere.
2-2337	A	94:7	94:12	Need to spell out the distinction between direct human generation of atmospheric water-vapour and the major indirect contribution. For instance small anthropogenic changes in the temperature at the water/air interface caused by increase in GHG concentration, change the rate of evaporation. Energy from the radiative forcing is taken up by the latent heat of evaporation. Warmer air retains a higher concentration of water-vapour in non-condensed state, so accelerating the shift in radiative forcing due to the greenhouse effects of the water vapour. This is a powerful non-linear feedback mechanism in the climate-change dynamic. Temperature rise at the water/air interface is partially suppressed by the effects of evaporation as the latent heat extracts energy from the sub-system. Only some of that latent heat is subsequently released to energise the weather system. The increased volume of water-vapour that does not condense and release its latent heat, remains in gaseous form and constitutes a store of energy produced by the radiative forcing, but	Rejected A discussion of the mechanisms controlling water vapor abundances is not appropriate here.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				having no effect on the temperature. [David Wasdell]	
2-2338	A	94:9		Insert after "activities" "are thought to" [Vincent Gray]	Rejected. This is sufficiently certain in this context.
2-2339	A	94:10		Insert after "water vapour". Since changes in water vapour are inadequately understood or measured they are not included in Table 2.3.1" [Vincent Gray]	Rejected. A citation to the chapter text is not appropriate here. The level of understanding on this issue is not important in this context.
2-2340	A	94:14	94:21	I really like the "questions" that have been included in the chapters. These questions serve a pedagogic function. Given this purpose, we should get the terms right. An "aerosol" is a suspension of particles in a gas. The atmospheric sciences community uses "aerosols" to refer to "particles" in the vernacular. Certainly, "aerosols" has become too well accepted in this community to turn back the clock to correct usage. Even so, in this pedagogic section, I think that the student should be introduced to the correct usage of the word "aerosol" and that "aerosols" would be properly referred to as "aerosol particles". With that disclaimer on common usage, the pedagogic section could then continue with the term "aerosols". [Scot Martin]	Accepted
2-2341	A	94:18	94:18	Add after "..., and black carbon." Anthropogenic activities such as surface mining and industrial processes add to the dust load in the atmosphere. [Sabine Wurzler]	Accepted
2-2342	A	94:18	:20	These statements while true are misleading. The major consequence of deforestation was the input of CO2 into the atmosphere from the standing biomass. Best estimates of the comparison of this biomass CO2 with that from fossil fuel combustion suggest that the biomass injection rate exceeded the fossil injection rate until about 1910, and that the resultant burden of excess CO2 from biomass exceeded that from fossil fuel until about 1975. [Stephen E Schwartz]	Accepted. (Comment refers to last section) The current level of natural terms will be stressed in last section.
2-2343	A	94:21		Add at end . "Aerosols can provide both warming and cool.ing effects on the climate, and they could even counteract the warming effects of greenhouse gases to give a net cooling. [Vincent Gray]	Rejected. Comment on warming and cooling is already included. The thought of 'counteract' is inappropriate in this context.
2-2344	A	94:25	94:35	A general discussion of radiative forcing would be useful. The atmosphere is always undergoing radiative forcing of one kind or another, and it is never ever actually in equilibrium with the applied radiative forcing. The largest radiative forcing occurs diurnally with the rising and setting of the sun whereby the solar irradiance may very from more than 1000 W/m2 at high noon to zero at night. There is a corresponding	Rejected. The suggested level of detail is not appropriate in this context. More detail on radiative forcing is included in the questions from Chapter 1.

No.	Batch	Page:line From	To	Comment	Notes
				change in temperature that may be larger than several tens of degrees C, although over ocean surfaces, the diurnal temperature change may be near zero. There is also a seasonal change in solar irradiance that may be as large as several hundred W/m2, and the corresponding temperature change, at some locations, may be even larger than the diurnal temperature change. Taken as a global average, the Earth receives about 20 W/m2 more during January than it does during July as a reslt of the Earth's orbital eccentricity. But, renmarkably, the global mean surface temperature in January (with more incident solar energy) is actually several degrees colder than it is during July. [Andrew Lacis]	
2-2345	A	94:26	:29	This definition is imprecise and erroneous. Radiative forcing is a change in an energy flux (energy per area and time). It is thus not, conventionally, a change in "energy available to the global Earth-atmosphere system" but a change in one or another components of energy flux. It is certainly not " measured in units of 'energy per unit area of the globe'" (which would be Joules per square meter). The fact that non-scientists will be reading this report is no justification for this sort of inaccurate dumbing down. Such imprecision would ultimately only redound to the discredit of the report. [Stephen E Schwartz]	Accepted. Section rewritten
2-2346	A	94:28		correct: "of the globe" to "of time" [Hartmut Grassl]	Accepted Section rewritten
2-2347	A	94:29	94:29	Even when spelled out, Watts has upper case W. [Howard Roscoe]	Accepted
2-2348	A	94:32		correct: "a warming of climate" to "a mean warming" (delete: "of climate") [Hartmut Grassl]	Accepted
2-2349	A	94:33		add: "mean" before "cooling" [Hartmut Grassl]	Accepted
2-2822	B	94:34	94:35	Can't we make that sentence even stronger? [Olivier Boucher]	Rejected. Sentence removed from this section.
2-2350	A	94:36	94:36	All of these radiative forcings are explicitly modeled in typical climate GCMs, but none of these radiative forcings are contributors to what is generally described as the measure of radiative climate forcing, anthropogenic or otherwise. Perhaps it is ironic that the quantity that is defined as the measure of radiative forcing is not even a diagnostic output quantity in climate GCM operation. In fact, a significant effort is required in order to obtain a well defined measure of radiative forcing from typical climate GCMs since a number of physical processes and model feedbacks need to be shut down or constrained so as to avoid feedback contamination. [Andrew Lacis]	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2351	A	94:37	95:16	A radiative climate forcing (RCF) is a sustained externally applied perturbation that alters the quasi-equilibrium radiative energy balance of the climate system, and thus drives the climate to a new equilibrium. There is a characteristic time scale associated with RCF in that the radiative perturbations need to be sustained long enough to interact with the relatively large heat capacity of the atmosphere and the much larger heat capacity of the ocean. Thus, day-to-day sunrise to sunset changes in solar radiation are far too rapid to qualify as radiative climate forcing, but the slower changes in solar irradiance occuring over the course of the 11-year sunspot cycle and the long-term solar irradiance changes inferred from changes in cosmogenic isotopes and other indicators of solar activity, are examples of naturally occuring RCF that alters the prevailing global energy balance and drives the climate to a new equilibrium. Sporadic volcanic eruptions that inject sulfur bearing gases and particulate matter into the stratosphere are another example of naturally occuring RCF. [Andrew Lacis]	Noted
2-2352	A	94:41		Replace "changes" with "increases" [Vincent Gray]	Accepted
2-2353	A	94:42		Insert "are thought to" at the beginning [Vincent Gray]	Rejected. Not needed here.
2-2354	A	94:44	94:44	Through interaction with" is vague and probably confucing. Why not say "by reflection of solar radiation, and absorption and emission of thermal radioation [Howard Roscoe]	Accepted
2-2355	A	94:44		Replace "cause" by "influence" [Vincent Gray]	Accepted
2-2356	A	94:45	94:46	I suggest to change the order of "The direct radiative forcing from all aerosol types is slightly negative. Some aerosols cause a positive forcing while others cause a negative forcing." to "Some aerosols cause a positive forcing while others cause a negative forcing. The direct radiative forcing from all aerosol types is slightly negative." [Xiaobin Xu]	Accepted
2-2357	A	94:48		delete: "significant" [Hartmut Grassl]	Accepted
2-2358	A	94:48		Add at end " which could exceed the positive effects of greenhouse gases" [Vincent Gray]	Rejected Not needed here.
2-2359	A	94:52	94:53	correct: "ice surfaces" to "snow" (delete: "surfaces") [Hartmut Grassl]	Accepted
2-2360	A	94:53		correct: "global" to "Earth's" [Hartmut Grassl]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2361	A	95:1	95:1	Revise the 1st sentence which is too general in its current form as follows: Aircraft produce contrails at cruise altitudes which persist in ice super-saturated regions. [Mikhail Danilin]	Rejected. Supersaturated is too technical in this context.
2-2362	A	95:3	95:5	The conclusion that the contrail forcing is expected to increase "substantially in the future" is not well supported in the previous text. The authors should define what they mean by substantially. Compared to the forcing calculated for the long-lived greenhouse gases, the contrail forcing would still be expected to be a small contributor. Contrail formation is determined by aircraft flights through supersaturated regions of the atmosphere. Thus, future contrail coverage and subsequent radiative forcing will depend on the growth rate of commercial aviation, the geographical location of that growth, the flight altitudes of new aircraft, and future temperature/humidity of the ambient atmosphere at cruise altitudes. [Steven Baughcum]	Accepted. Sentence deleted.
2-2363	A	95:3	95:5	Given the current level of scientific understanding it seems to be premature to make such a statement unless this assessment aims to provide results of operational sensitivity studies and/or define what is meant by the phrase "increase substantially in the future", especially if this conclusion is based upon the assumption that flight tracks will remain unchanged. [Lourdes Maurice]	Accepted. Sentence deleted.
2-2364	A	95:5	95:5	I suggest to drop the word ''substantially", since effects of contrails and CO2 are assumed to be proportionally to the fuel burned and hence there is no need to say contrail/cirrus effects will grow faster than those from CO2. [Mikhail Danilin]	Accepted Sentence deleted
2-2365	A	95:15		Add after "activities" "but a number of possible feedbacks on solar activity, which are currently little understood, could increase its infliuence" [Vincent Gray]	Rejected. Not needed in this context.
2-2366	A	95:15		Insert after "is" "likely to be" and before "exceeds" "probably" [Vincent Gray]	Accepted Add 'estimated'
2-2367	A	95:17	95:17	CO2, CH4, and N2O are naturally occuring greenhouse gases that have become important contributors to global warming because their atmospheric concentrations are steadily increasing because of fossil fuel burning and agricultural practices due to human activity. CFCs and HCFCs are long-lived GHGs of entirely anthropogenic origin. They are all strong absorbers of thermal radiation and thus contribute to the growing thermal opacity of the atmosphere and strength of the greenhouse effect. The anthropogenic effect on ozone is more subtle in that through atmospheric chemistry, CFC increases act to decrease stratospheric ozone while CO and CH4 increases act to increase tropospheric ozone, with both effects contributing to global warming. Fossil fuel burning and industrial activity	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				have also increased the atmospheric aerosol load, with non-absorbing sulfate and nitrate aerosols tending to counteract global warming by reflecting solar radiation, while strongly absorbing black carbon aerosols add to global waming. [Andrew Lacis]	
2-2368	A	95:18	95:18	Human activities have also lead to land use changes whereby forested areas have been replaced y pastures, crop lands, and urban developments. Typically, this has resulted in replacing low-albedo forested areas with higher surface albedos - a negative radiative forcing. On the other hand, soot deposition over snow covered areas reduces the surface abedo to produce a positive radiative forcing. Other radiative climate forcings attributable to human activity include production of cirrus-like aircraft contrails, the increase in stratospheric water vapor due to oxidation of CH4, and increase in tropospheric water vapor due to irrigation practices. Altogether, the radiative climate forcing attributable to anthropogenic GHG increase since 1750 is about 3 W/m2. [Andrew Lacis]	Noted
2-2369	A	96:0		Table 2.3.1: Mention in a footnote that the mixing ratios are shown at the surface (ie., altitude=0). [Mikhail Danilin]	Noted.
2-2370	A	96:0		Table 2.3.1. Not immediately obvious that "changes since TAR" are the same as "Change since 1998" as differences might be due to change in concentration of gas (since 1998) or change in estimation method (since TAR). [Joanna Haigh]	Noted.
2-2371	A	96:0		Table 2.3.1. CFC-11 change since 1998 must be - 3 % [Ralf Koppmann]	Noted.
2-2372	A	96:0		Tables: Additional table -- Table 1 from S. Solomon, D. W. J. Thompson, R. W. Portmann, S. J. Oltmans, and A. M. Thompson, On the distribution and variability of ozone in the tropical upper troposphere: Implications for tropical deep convection and chemical-dynamical coupling, Geophys. Res. Lett., in press, 2005. [Anne Thompson]	Rejected. Not sufficently related to RF, ozone assessment will look at these issues
2-2373	A	96:3		Replace "LLGHGs" with " long-lived greenhouse gases" [Vincent Gray]	Noted. This is understood to be the case from text.
2-2374	A	96:7		It is obligatory to quote 95% confidence limits. All the quoted errors in the Table should be doubled. [Vincent Gray]	Reject.
2-2375	A	96:8	96:9	Unclear sentence. [Cathy Clerbaux]	Noted.
2-2376	A	96:21		Blank missing between '46' and 'ppt'	Noted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Cathy Clerbaux]	
2-2377	A	96:23		Explain the significance of the many dashs , "-" in the third and fifth columns. Why are some figures in BOLD? [Vincent Gray]	Noted.
2-2379	A	97:0		Table 2.4.1: Excellent and useful summary of aerosol-sensing satellites [Theodore Anderson]	Accepted, thanks
2-2380	A	97:0		Table 2.4.1: Some MODIS retrievals use different #'s of channels. Maybe each retrieval technique should be separated. [Robert Levy]	Rejected, too detailed
2-2381	A	97:0		Table 2.4.1.: add a reference to GLAS, e.g., Spinhirne et al., 2005, GRL; add data years for CERES; add a "spatial resolutions" column. [Hongbin Yu]	Accepted, reference and data years for CERES added
2-2382	A	97:1		Table 2.4.1.: SeaWiFS aerosol product is not included in the table. The information of this aerosol product is as follows: The period of operation: 1997-present. Spectral bands: 0.765, 0.865 mm. Products: taer, Å. Comment & Reference: 0.510mm and 0.865mm retrieval gives tl=0.865 and Å over ocean. Bi-modal aerosol size distribution assumed (Wang et al., 2005). [Xuepeng Zhao]	Accepted, Thanks for the detailed information
2-2383	A	97:1		The aerosol product in the reference of Higurashi et al.(2000) is based on the AVHRR observations rather than the OCTS observations. Thus, AVHRR aerosol product produced by Higurashi et al.(2000) should be added in the Table. (Full references listed in separate supplemental doc file) [Xuepeng Zhao]	Accepted, an additional line for AVHRR added for Higurashi et al. (2000). The reference for OCTS corrected to Nakajima and Higurashi (1998)
2-2384	A	97:3		correct: "mono-model" to "mono-modal" in line 2 [Hartmut Grassl]	Accepted
2-2385	A	97:3		insert a space between "the" and "8" in line 7 [Hartmut Grassl]	Accepted
2-2386	A	97:3		correct: "Apr 03" to "Apr 2003" in line 11 [Hartmut Grassl]	Accepted
2-2387	A	98:0		Table 2.4.2: Excellent and useful summary of satellite-based estimates of DRE [Theodore Anderson]	Accepted, thanks
2-2823	B	98:0		Can you check the latest numbers with Nicolas as clear-sky DRE is not given any longer in the paper?	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Olivier Boucher]	
2-2388	A	98:0		In Table 2.4.2, units for DRE should be given. [Steven Sherwood]	Accepted
2-2389	A	98:0		Table 2.4.2. Footnotes should be added to this table. [Xiaobin Xu]	Rejected
2-2390	A	98:0		Table 2.4.2: (1) Brief description for Remer and Kaufman (2005) is totally irrelevant. (2) What is difference between Boucher and Tanre (2000) and Bellouin et al. (2003)? (3) Description for Chou et al. (2002) is missing. (4) Loeb and Kato (2002) report DRE in low latitudes. How can the number be comapred with others? (5) Add missing data years. [Hongbin Yu]	Noted, 1) description changed, 3) description for Chou et al added 5) missing years added
2-2391	A	98:1		Table 2.4.2. The word "aerosol" should be inserted before "Direct Radiative Effect (DRE)". [Philippe Tulkens]	Accepted
2-2392	A	98:3	98:3	the unit is missing in table 2.4.2. [Wm-2] [Rolf Philipona]	Accepted
2-2393	A	99:0		Table 2.4.3: Excellent and useful summary of sulfate forcing estimates. Corrections/improvements: (i) Table caption should specify the wavelength of optical depth. (ii) Line C should have -206 (negative sign!) for NDRFM and lines N, O, R, S, T should show values of NDRFM, as this is simply the ratio of DRFTOA and LOAD and data for both these parameters are shown. (iii) Lines N, O, R, S, T should also show values of NDRF, since this is simply the ratio of DRFTOA to AOD, and data for both these parameters are shown. Adding these data would permit much more complete assessment of averages, range and stdev in the bottom 6 rows. [Theodore Anderson]	Accepted
2-2824	B	99:0		206 should read -206 [Olivier Boucher]	Accepted
2-2394	A	99:0		Table 2.4.3. Explain acronyms [Ralf Koppmann]	Accepted
2-2395	A	99:0		Table 2.4.3: The headers need to be defined [Robert Levy]	Accepted
2-2396	A	99:0		Table 2.4.3. The style of units should be consistent within a report, at least with a table. [Xiaobin Xu]	Noted, the style is now more consistent
2-2397	A	99:1	99:9	Table 2.4.3 Is the load total or anthro sulfate? Units on NDRFM should be Wm-2gSO4-1 [Tim Bates]	Accepted, It is stated that load and AOD is anthropogenic. Unit for NDRFM is corrected to Wg-1
2-2398	A	99:2		replace the semicolon by a colon after "AODant"	Rejected

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Hartmut Grassl]	
2-2399	A	99:4	99:5	table 2.4.3. reference for LSCE missing, also in tables 2.4.4. and 2.4.5. [Christiane Textor]	Noted
2-2400	A	100:0	100:0	The units in some of the columns are not very clear [Keith Shine]	Accepted, units modified and some are corrected
2-2401	A	100:0		Table 2.4.4: This table contains far too much detail and is essentially incomprehensible. The column headings are not adequately explained and the logical relationships between them are not apparent. [Theodore Anderson]	Noted, the table is modified
2-2402	A	100:0		Table 2.4.4 Table is hard to read and understand in its present form, explain acronyms [Ralf Koppmann]	Noted, the table is modified
2-2403	A	100:0		Table 2.4.4: Same here [Robert Levy]	Noted, the table is modified
2-2404	A	100:0		Table 2.4.4: The unit for "LOAD BC" is missing; "Chuang et al. 2002" should be "Chung and Seinfeld, 2002". [John Seinfeld]	Accepted
2-2405	A	100:0		Table 2.4.4. The style of units should be consistent within a report, at least with a table. [Xiaobin Xu]	Accepted
2-2406	A	100:0		Table 2.4.4. The underlines in this table seem unnecessary. [Xiaobin Xu]	Noted
2-2407	A	100:0		Table 2.4.4: I would suggest to add surface DRF. [Hongbin Yu]	Rejected
2-2408	A	100:1	100:11	Table 2.4.4 FFBC fossil fuel black carbon, [Tim Bates]	Accepted
2-2409	A	100:3	100:3	I liked the NDRF in Table 2.4.3 (it was also very useful in the TAR report). It would be useful in Table 2.4.4. When burdens vary so much due to differences in emissions, the NDRF shows the relative agreement or lack of agreement between models. [Tami Bond]	Noted
2-2410	A	101:0		Table 2.4.5: Excellent summary of current, model-based estimates of direct forcing by anthropogenic aerosols. For clarity, I request that the relation between "clear-sky" and "all sky" values carefully explained. If I understand correctly, "DRF TOA clear sky" represents an average over just the clear-sky portions of the earth whereas "DRF TOA all sky" represents an average over the entire earth. Thus, there are two factors that cause the "all-sky" values to be closer to zero (and these two factors cannot be separated from the data provided.) First, the effect of cloud-cover fraction acts to reduce the "clear-sky" forcing by spreading it out over a larger area. Second, the direct effect of aerosol in	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				cloudy regions is positive in many models, which makes the "all-sky" values less negative (closer to zero, smaller in magnitude). [Theodore Anderson]	
2-2411	A	101:0		Table 2.4.5: Are these values "globally averaged" and "temporally averaged?" [Robert Levy]	Noted, it is stated now in the aerosol section that RF is global and annual mean except otherwise explicitly stated
2-2412	A	101:0		Table 2.4.5: The number from Liao and Seinfeld [2005] is not correct. Liao and Seinfeld [2005] shows an anthropogenic TOA forcing of +0.01 Wm-2 and a surface forcing of -2.42 Wm-2 for internally mixed sulfate, nitrate, BC, OC, and aerosol water. A TOA forcing of -0.39 Wm-2 and a surface forcing of -1.98 Wm-2 are predicted if these aerosols are externally mixed. [John Seinfeld]	Accepted
2-2413	A	101:0		Table 2.4.5. The style of units should be consistent within a report. [Xiaobin Xu]	Accepted
2-2414	A	101:0		Table 2.4.5. The underlines in this table seem unnecessary. [Xiaobin Xu]	Noted
2-2415	A	101:1	101:6	Table 2.4.5 How is all sky defined with respect to direct radiative forcing? [Tim Bates]	Noted, clarified in the text
2-2416	A	102:0	102:	Table 2.4.6: The references for the Microphysics for Rotstayn & Penner (2001) are not the most appropriate, since Gregory & Rowntree (1990) describes the convection scheme, which has little microphysics (though I don't mind leaving it in if the authors want to). I suggest replacing that one with (Rotstayn, L. D., 1997: A physically based scheme for the treatment of stratiform clouds and precipitation in large-scale models. I: Description and evaluation of the microphysical processes. Quart. J. Roy. Meteor. Soc., 123, 1227-1282) and (Rotstayn, L. D., B. F. Ryan, and J. J. Katzfey, 2000: A scheme for calculation of the liquid fraction in mixed-phase stratiform clouds in large-scale models. Mon. Wea. Rev., 128, 1070- 1088.) [Leon Rotstayn]	Accepted
2-2417	A	102:0	102:	Table 2.4.6: The description of Rotstayn & Liu (2003) is misleading, because that study did not use prescribed sulfur loading as in the earlier one by Rotstayn & Penner (2001). The model included an interactive sulfur cycle. Also, the results should be stated as -1.39 (albedo) with a 12-35% decrease due to dispersion. [Leon Rotstayn]	Accepted
2-2418	A	102:0		Table 2.4.6. Why are some elements in last column in bold type? [Joanna Haigh]	Noted, the bold type are the estimates of cloud albedo that are included in Figure 2.4.4

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2419	A	102:0		Table 2.4.6: Inaccuracies regarding "Kristjansson (2002)": "E (for natural), I (for anthropogenic)" should be replaced by 'E for nucleation mode and fossil fuel BC, I for accumulation mode' [Jón Egill Kristjánsson]	Accepted
2-2420	A	102:0		Table 2.4.6: Correction regarding "Kristjansson (2002)": the following information is missing in the column "Cloud Types for Indirect Effect": 'warm and mixed phase; stratiform and detraining convective clouds' [Jón Egill Kristjánsson]	Accepted
2-2421	A	102:0		Table 2.4.6: Correction regarding "Kristjansson (2002)": "Detailed aerosol model included" should be replaced by 'Rasch and Kristjánsson (1998)'. The complete reference for that paper is: Rasch, P. J., and J. E. Kristjánsson, 1998: A comparison of the CCM3 model climate using diagnosed and predicted condensate parameterisations. J. Climate, 11, 1587-1614. [Jón Egill Kristjánsson]	Accepted
2-2825	B	103:0		Could you include Quaas et al. (2004), Quaas and Boucher (2005) and Quaas et al. (2005) in the table? [Olivier Boucher]	Accepted
2-2422	A	103:0		Table 2.4.6: Correction regarding "Storelvmo et al. (2005)": All the information here, including the figure -1.15 in fact refers to the paper Kristjánsson et al. (2005), which is in press at JGR (doi: 10.1029/2005JD006299). [Jón Egill Kristjánsson]	Accepted
2-2423	A	103:0		Table 2.4.6: Correction regarding "Storelvmo et al. (2005)": As mentioned in item #17 above, the information concerning Aerosol mixtures is incorrect. [Jón Egill Kristjánsson]	Accepted
2-2424	A	104:0	104:	Table 2.6.1 The extrapolation of aviation from 2000 to 2004 using a 3.2 %/year growth result is difficult to defend in light of the large impact of the events of 9/11/2001 on air traffic, particularly in the US where a large fraction of flights occur. The authors should determine a more defensible way to extrapolate their results from 2000 to 2004. Similarly, the assumption that fuel burn is a good surrogate for aircraft contrails is difficult to defend since most of the ice content in a persistent contrail probably comes from the ambient atmosphere, not from the aircraft emitted water. [Steven Baughcum]	considered
2-2425	A	104:0	104:	Table 2.6.1: I suggest to drop the last column completely, since a noticeable drop in aviation traffic after September 11, 2001 made a linear extrapolation of the 2000 traffic to 2004 invalid. Also, the footnote (e) should be removed. [Mikhail Danilin]	considered
2-2826	B	105:0		"Compared are estimates" ?	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Olivier Boucher]	
2-2426	A	105:6	105:6	0.7 is (1-albedo) [Keith Shine]	
2-2427	A	106:0	106:	Modify the line for Contrails in Table 2.9.1 as follows: 1st column: Contrails (direct effects), last column: re-evaluated direct contrail forcing [Mikhail Danilin]	accepted
2-2428	A	106:0	106:	add a footnote explaining that 2X(3X) means by a factor of 2(3), respectively [Mikhail Danilin]	accepted
2-2827	B	106:0		Can you remind the reader of the meaning of the uncertainty interval in this table? The range for the first indirect effect (-1.9 to -0.5) excludes some of the recent estimates by Qaas et al (2005), Quaas and Boucher (2005), and Storelvmo et al (JGR, submitted, 2005) which all point to a smaller aerosol indirect effect. [Olivier Boucher]	Accepted
2-2429	A	106:2		Explain the basis of the uncertainty figures for Table 2.9.1. If they are currently standard deviations you should double all of them to give 95% uncertainties. [Vincent Gray]	accepted
2-2430	A	107:0	107:	Table 2.9.2: For the Cloud lifetime effect, while I agree with the assessment of "very low" scientific certainty, it would be good to write "Some evidence from models and model-satellite comparisons" with a cross-reference to Ch. 7 (their lines 21-26 on page 68). [Leon Rotstayn]	accepted
2-2431	A	107:0	108:	Table 2.9.2: I suggest to drop the column consensus and rename the column ''Overall" as ''Confidence", since the Consensus column does not add here anything new and just complicates this Table. Indeed, consensus 1, 2, and 3 always correspond to Overall high, medium, and low, respectively. If this suggestion will be accepted, the related text in 2.9.2 (pp.2-62 - 2-63) should be revised. [Mikhail Danilin]	Accepted
2-2432	A	107:0		Table 2.9.2. Row: Tropospheric ozone. Column: uncertainties. Typo: "lightening" should be "lightning". [Philip Cameron-Smith]	accepted
2-2433	A	107:0		Table 2.9.2: I sort of understand the formula, but why does Direct scattering aerosols (A * 2) = Medium, why direct absorbing aerosols (A * 2) = Low? [Robert Levy]	Reworded – see 2-2434
2-2434	A	107:0		Table 2.9.2. It is strange that the uncertainty of A2 is sometimes medium and sometimes low. Similarly, B3 is sometimes low and sometimes very low. In principle, the overall uncertainty can have 3*3=9 values. Only 5 are used (by the way, this is weird, why never a 'A3' or 'C1'?). But given that at the moment 5 combinations are used (A1, A2, B2, B3,	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				C3), I suggest to use 5 rather than the present 4 categories for the overall uncertainty (e.g. A1=very high, A2=high, B2=medium, B3=low, C3=very low). [Peter Siegmund]	
2-2436	A	107:1		It is deplorable that there is no attempt to estimate changes in water vapour concnetration, whether anthropogenic or natural [Vincent Gray]	rejected
2-2437	A	107:1		Table 2.9.2., cloud albedo effect: Sekiguchi et al. (2003) showed there is an evident correlation of the effective particle radius and optical thickness of low clouds with the column number concentration of aerosol particles through a global statistical analysis of observations from AVHRR, POLDER, and ADEOS. This correlation was also used to estimate the aerosol forcing of the aerosol indirect effect relative to the preindustrial era. Thus, the comments for cloud albedo effect item in the table should be modified as follows: Certainties: Observed in case studies – e.g., ship tracks; GCMs model one; "and global observational statistics". Uncertainties: Lack of "direct" observational evidence of a global forcing. (Full references listed in separate supplemental doc file) [Xuepeng Zhao]	Considered
2-2438	A	107:2		correct: "From" to "from" in column 1, line 15 [Hartmut Grassl]	accepted
2-2439	A	107:2		correct: "From" to "from" in column 1, line 18 [Hartmut Grassl]	accepted
2-2440	A	108:0	108:	Table 2.9.2 Given the large uncertainties in RHI needed to assess contrail coverage and optical properties, it is very difficult to understand how the contrail uncertainties could be assessed as Medium. Current models tend to have a dry bias for upper tropospheric water vapor and do not incorporate explicit microphysics or subgrid transport processes needed to derive supersaturation from the physics. The parametric approaches now in use have significant simplifications/uncertainties and those in turn lead to signficant uncertainties in the calculated coverage and properties of contrails. [Steven Baughcum]	Accepted
2-2441	A	108:0	108:	Table 2.9.2: I disagree that we have the Medium Overall level for Contrails and Aviation Cirrus, since too many assumptions were made in calculating contrail coverage (because relative humidity is poorly known near the tropopause, subgrid-scale processes are overlooked in global calculations or oversimplified, a questionable regional normalization procedure was applied homogeneously over the whole globe). Additionally, optical properties of contrails and shapes of their crystals are unknown and their indirect impact on currus cloudiness is still a mistery. Also, in this line I suggest to remove contrail	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				coverage from the column Certainties and add the following text in the column Uncertainties: contrail coverage and optical properties. [Mikhail Danilin]	
2-2442	A	108:0		insert: "as well as" after "observations" in column 5, line 9 [Hartmut Grassl]	accepted
2-2443	A	108:0		Solar row: spelling of 'indicators' in col.5 and 'their' in col. 7 [Joanna Haigh]	accepted
2-2444	A	109:0	110:	Table 2.10.1 There is only little compounds on this table. Like 3rd report (The Scientific Basis, pp.388-390, Table 6.7 and 6.8), it is necessary to list up the many compounds in this table, because GWP and lifetime are most important factor for the environmental evaluation of compound. [Kazuaki Tokuhashi]	Accepted
2-2445	A	109:0		Table 2-10-1 is the same as Table 2.6 from IPCC-SROC report. I am not sure it is mentionned somewhere. [Cathy Clerbaux]	Not completely some updates
2-2446	A	109:0		The emission reporting by the countries to the UNFCCC uses the GWP values from the IPCC 1996 report (SAR). Please, write this fact in the table or in its footnotes and give the SAR GWP values of CH4 and N2O for information to the reader. [Ilkka Savolainen]	Accepted
2-2447	A	110:0	110:0	My understanding is that "Freon" is a DuPont trade name and should not be used in such tables [Keith Shine]	Accepted
2-2448	A	110:0	110:0	I would like to make a few suggestions for the GWP table: first, it will be helpful if these are consistent between the IPCC (2007), IPCC (2005 SROC report) and WMO/UNEP (2006) reports where appropriate, with any differences between them explained. Please also consider whether or not the radiative efficiencies for CO2, CH4, and N2O are current or require updating given trends in concentrations compared to IPCC (2001), and whether or not the carbon cycle removal function remains valid. Each of these important matters may be assisted by a footnote to document choices made. [Susan Solomon]	Accepted
2-2828	B	111:0		100 --> 100 years [Olivier Boucher]	Accepted
2-2449	A	111:0		Table 2.10.2. two lines Berntsen et al.: should there be a "-" ?? (-31 - (-42)) and - 8.6 - (-11) ?? [Ralf Koppmann]	Accepted
2-2450	A	111:0		Table 2.10.2 What do the "÷"s in this table mean?	Table redone

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Xiaobin Xu]	
2-2451	A	111:0		Why not include the GWP values for H2 and aerosol and aerosol precursors from page 67? [Xiaobin Xu]	Accepted?
2-2452	A	111:6		comment: note (b) is lacking [Hartmut Grassl]	Comment added
2-2453	A	112:0		Figures: Additional figure -- a map of the SHADOZ ozonesonde network (after figure 1 in Thompson et al., 2003). Please contact the reviewer for an updated image. [Anne Thompson]	Rejected
2-2454	A	113:0		What is the box non-radiative forcing for? [Cathy Clerbaux]	Noted. Figure will be reworked
2-2455	A	113:0		Fig.2.2.1. Indicate within each box which chapter of the report is addressing that issue. [Joanna Haigh]	Reejcted
2-2456	A	113:0		Figure 2.2.1: the terms RF, DRE, etc should be included in this figure [Robert Levy]	Rejected
2-2457	A	113:0		Fig. 2.2.1: This figure is somewhat confusing. In the pink box, the right oval should not be labeled "changes in climate system components. Rather indirect effects should point to things which are also climate forcing agents - otherwise they should not cause climate to change. The entire pink box should be labeled "climate forcing agents" and there should be not red ovals inside it. "Evapotranspiration flux" should not be here. The rightmost "feedback processes" diamond should point back to the light blue box, "climate perturbation and responses." Feedbacks are within the climate system and not forcing. [Alan Robock]	Noted. Figure will be reworked
2-2458	A	113:0		Fig. 2.2.1: Why is there a diamond labeled "non-radiative forcing?" If the forcing is not radiative, then how can it cause climate to change? [Alan Robock]	Noted. Figure will be reworked
2-2459	A	113:0		Fig. 2.2.1: Shouldn't natural variability/chaos be in here somewhere, to indicate that all climate change is not deterministic? [Alan Robock]	Noted. Figure will be reworked
2-2460	A	114:0		Figure 2.2.2. Confusing. There is no need for the figure since the RF assessment does not follow the concepts in this figure anyway. [Mian Chin]	Noted. Figure will be reworked
2-2461	A	114:0		I'd add some comment to fig.2,2,2 for the sake of clarity to describe the mechanism involved [Tiziano Colombo]	Noted. Accepted
2-2462	A	114:0		Figure 2.2.2: The figure is not sufficient to define the various kinds of forcing. In fact the	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				figure is not helpful. The various types of RF and how they differ and why they have been defined as well as which ones are most useful should be addressed at this point. [Patrick Hamill]	
2-2463	A	114:0		Figure 2.2.2. Without a detailed explanation this figure is not very helpful; explain the solid and dashed lines, explain the arrows. [Ralf Koppmann]	Noted
2-2464	A	114:0		Figure 2.2.2. Why is this figure right here? Most of these other forcings were barely discussed in the text. [Robert Levy]	Noted
2-2465	A	114:0		Fig. 2.2.2. Improve lettering on graphic [Melinda Marquis]	Accepted
2-2466	A	115:0	115:	I suggest to print the numbers and text along the right axis of the bottom panel in red to improve readability [Peter Van Velthoven]	Accepted
2-2467	A	115:0		Use other color codes for data for 2003 and 2004 from a different source? [Cathy Clerbaux]	Accepted
2-2468	A	115:0		Reference British Petroleum (2005) not found. [Cathy Clerbaux]	British petroleum reference added
2-2469	A	115:0		I'd add in the same panel a northern latitude station like Point Barrow showing a higher amplitude oscillation and therefore an higher quantity of carbon equivalent involved in the biosphere-atmosphere exchange, but the same increasing trend [Tiziano Colombo]	Noted ..good idea but will make the figure very cluttered ..intent here is to show trends in CO2 at typical NH and SH stations ...Ch 7 dicusses exchange
2-2470	A	115:0		Figure 2.3.1. Reverse the scale for delta13C(CO2) to match with text on page 10, lines 32-33 [Ralf Koppmann]	Accepted
2-2471	A	115:0		Figure #3.1 and 3.2. These two figures have several problems. They lose the time-perspective that the TAR had because they only start in 1970/1980, they mix information on concentration with information on rates, and they have inconsistent units. I would suggest a different arrangement as follows. First figure: CO2 increase, O2/N2 decrease, and d13C (all concentrations). Second figure: fossil fuel emissions and CO2 growth rate (both in PgC/y, starting in 1960) (essentially an update of Figure 3.3 from the TAR). [Corinne Le Quere]	Accepted ..figure will be changed
2-2472	A	115:0		Fig. 2.3.1 Data points in panel a not clear. Data are (not data is). Add "respectively" to "…data for 2003 and 2004 of …" [Melinda Marquis]	Accepted
2-2473	A	115:0		Fig. 2.3.1: O2 units should be "ppm," just like CO2. "permeg" is a new made-up unit	Noted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				that is not standard and is confusing, even if defined in the caption. [Alan Robock]	
2-2474	A	115:4	115:4	The reference by Manning et al (1997) cannot be responsible for data more recent than 1997. Clarify where the updated data can be attributed. [Keith Lassey]	Noted ...the reference refers to the techniques used to make the measurements. Time series made by the same techniques are not routinely published in literature but updated on web sites as explained in the text
2-2475	A	115:6	115:7	"(Data from the CDIAC website (Marland et al., 2005))" -- note 2 closing parentheses. However, as the website URL is included in the reference to Marland (2005), only "(Marland, 2005)" need be cited in this caption. [Keith Lassey]	Noted
2-2476	A	115:7	115:9	add the latitudes of Mauna Loa, Alert and Cape Grim - people unfamiliar with the field do not know where these stations are located [Peter Van Velthoven]	Accepted ...the site is Baring Head not Cape Grim
2-2477	A	115:7		It is confusing to assume a constant "airborne fraction", The airborne fraction is not constant. The top graph should be redrawn using actual known values of the airborne fraction [Vincent Gray]	Noted...ref to airborne fraction removed from the figure
2-2478	A	115:10		permeg(parts per million), but usually ppm is short for parts per million [Junying Sun]	Noted
2-2479	A	115:11	115:11	In the phrase "fossil-fuel burning, cement manufacture and gas flaring", the last of the three is redundant as gas is a fossil fuel and flaring is combustion. [Keith Lassey]	Noted
2-2480	A	115:12	115:12	"(Data from the CDIAC website (Marland et al., 2005))" -- note 2 closing parentheses. However, as the website URL is included in the reference to Marland (2005), only "(Marland, 2005)" need be cited in this caption. [Keith Lassey]	Noted
2-2481	A	116:0		Figure 2.3.2: It would be helpful to add a 5-year smoothed curve or some sort of regression fit to emphasize the longer term trend. This trend appears to be positive and possibly accelerating. [Theodore Anderson]	Noted ...this is described in the text
2-2482	A	116:0		Turn the plot [Cathy Clerbaux]	Noted
2-2483	A	116:0		Figure 2.3.2: I suggest to expand the timescale of this figure back to 1958. [Nicolas Gruber]	Noted ...this figure will be replaced

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2484	A	116:0		Figure 2.3.2 is rotated by 90 degrees [Patrick Hamill]	Noted ...this figure will be replaced
2-2485	A	116:0		Figure #3.2 If the 4AR is able to report on atmospheric CO2 increase based on 40 stations (rather than 2 for the TAR), this could be highlighted in Figure 3.2 by including estimates of CO2 growth rates with 2 and 40 stations. [Corinne Le Quere]	Noted ...this figure will be replaced
2-2486	A	116:0		Figure 2.3.2: This figure should be turned sideways and the font improved [Robert Levy]	Noted ...this figure will be replaced
2-2487	A	116:0		Fig. 2.3.2 Improve lettering on graphic. Draw smooth data curve. Remove plot heading on RHS - put in caption. [Melinda Marquis]	Noted ...this figure will be replaced
2-2488	A	116:0		Fig. 2.3.2: Obviously figure should not be sideways. And why to the values go to 2005, when the caption of this figure says it only goes to 2003 and the caption of 2.3.1 says CO2 observations only go to 2004? [Alan Robock]	Noted ...this figure will be replaced...data past 2004 waiting for final calibrations
2-2489	A	116:0		Figure 2.3.2. This graph should be turned 90 degrees anticlockwise [Xiaobin Xu]	Noted ...this figure will be replaced
2-2490	A	116:1	116:10	The caption states that annual increases (from 1 January to 1 January) are plotted, but there seem to be more than 1 point per year, as the curve is very smooth. Has a a polynomial been fitted to the data? [Peter Van Velthoven]	Noted ...this figure will be replaced
2-2491	A	116:2	116:2	Figure 2.3.2: Why are years on y-axis? This layout is difficult to look at. [Tami Bond]	Noted ...this figure will be replaced
2-2492	A	116:2	116:5	Rotate the figure 90 degrees - for readability [Peter Van Velthoven]	Noted ...this figure will be replaced
2-2493	A	116:6	116:8	If the curve is smoothed -- ie, interpolated between each 1 January -- then why is it necessary or appropriate to say that "the annual increases are from 1 January to 1 January the following year"? The midyear "annual increases" are surely from 1 July to 1 July? [Keith Lassey]	Noted ...this figure will be replaced
2-2494	A	117:0		Fig. 2.3.3 Change Y axis units to W m-2. Change "an" to "and." Add hyphen: non-halocarbon. Change comma to semi-colon: (Section 2.3; see also McFarling …" [Melinda Marquis]	Noted. Accepted
2-2495	A	117:1	117:7	Figure 2.3.3: I found this figure far too simplified. It does not show the cumulative effects to give the total RF of these compounds, as has been done in the TAR and O3 Assessments. It does not show any of the sbtleties of this issue, such as the rapid decline of CH3CCl3, the leveling off of the longer-lived CFCs, and the rapid growth of the newer	Accepted .Fuuther figures added

No.	Batch	Page:line From	Page:line To	Comment	Notes
				compounds. As noted elsewhere in this review, more figures are needed to show these relationships. [Ray Weiss]	
2-2496	A	117:2	117:3	Please indicate 1860 along the time axis - in view of the discussion in chapter 2, page 11, lines 46-50. This will facilitate comparison between the values for 1750 and 1860. [Peter Van Velthoven]	Accepted
2-2497	A	117:4		I'd add "(Long Lived Greenhouse Gases)" [Tiziano Colombo]	Accepted
2-2498	A	117:4		timeseries should be time series [Junying Sun]	Accepted
2-2499	A	117:5	117:5	…firn air, flask an in-situ measurements discussed" -> "…firn air, flask and in-situ measurements [Xiaobin Xu]	Accepted
2-2500	A	117:5		correct: "an" to "and" [Hartmut Grassl]	Accepted
2-2501	A	117:6	117:6	The caption is not clear about how the curves were obtained. It states "a 20-year spline fit has been used". From the text in chapter 2 page 11 line 51, one might derive that the 20-year spline has been fitted through 5-yearly values. Please include a better description of the fit in the caption. [Peter Van Velthoven]	Accepted
2-2502	A	118:1	118:11	It is not described in the caption what the triangles in panel a are. [Peter Van Velthoven]	Accepted
2-2503	A	118:5	118:10	Replace "change" with "trend". Is the meaning of "sigma" standard deviation. If yes, replace it. Also replace technique with simulation. [G. H. Sabin GUENDEHOU]	Noted
2-2504	A	118:5	118:10	This caption should reiterate the new calibration scale against which the mole fractions are cited (p. 12, lines 24-27). [Keith Lassey]	noted
2-2505	A	119:0		Fig. 2.3.5 Use sans serif font on graphic. Capitalize "Northern …" [Melinda Marquis]	noted
2-2506	A	119:0		insitu to in situ [Junying Sun]	Noted
2-2507	A	119:5		northern to Northern [Junying Sun]	Noted
2-2508	A	119:6		I'd add the explanation in parenthesis of the acronims ALE, GAGE and AGAGE [Tiziano Colombo]	Noted

No.	Batch	Page:line		Comment	Notes
		From	To		
2-2509	A	120:0		Figure 2.3.6: Figure is low quality: [Robert Levy]	accepted
2-2510	A	120:0		Fig. 2.3.6 Poor resolution JPEG will need to be replaced. Change ">" to colon. [Melinda Marquis]	accepted
2-2511	A	120:1	120:7	Figure 2.3.6: This is essentially the same figure as Figure 7.4.7. See separate comments regarding resolving overlap between Chapters 2 and 7 on the OH problem. [Ray Weiss]	noted
2-2512	A	120:5	120:7	It is not described in the caption, nor in the text, in what sense the 18 inversions differ. This is essential for the conclusion drawn from this figure in chapter page 18 lines 52-55.Please give details in the text or caption. [Peter Van Velthoven]	noted
2-2513	A	120:6		Remove ">" [Cathy Clerbaux]	Accepted
2-2514	A	120:6		delete: ">" after "estimates" [Hartmut Grassl]	Noted
2-2515	A	120:7		I'd add some note regarding the nature of the inversions cited in the panel to specify what are they meaning [Tiziano Colombo]	Noted
2-2829	B	121:0		Caption not clear. Do solid bars go from 0 to top or from the top of the hatched bar to the top? [Olivier Boucher]	Accepted. Drop
2-2516	A	121:0	:0	Figure 2.3.7: In the figure caption: "removed radiation forcing" should be defined. [Patrick Hamill]	Accepted. Drop
2-2517	A	121:0		Figure 2.3.7. No axis label. [Xiaobin Xu]	Accepted. Drop
2-2518	A	121:5	121:12	The figure text for fig 2.3.7. is confusing. The match with the explanation in the main text could be improved. (This is unclear and may cause misunderstandings: …"forcing that twould occur in the absence of anthropogenic emissions".) [Jan Fuglestvedt]	Accepted. Drop
2-2519	A	121:5	121:6	The word "pentad" is sufficiently unusual as to warrant the definition in parentheses -- viz, "(5-year periods)". [Keith Lassey]	Accepted. Drop
2-2520	A	121:5	121:12	Where is N2O: presumably among "Other Kyoto" gases? This gas has a very different origin from the fluorinated gases, so its grouping should be identified. [Keith Lassey]	Accepted. Drop
2-2521	A	121:7	121:12	This caption is not very enlightening for those readers (probably almost all of them)	Figure deleted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				unfamilair with the AIRF concept. The caption sentences beyond the first could usefully be recast more succinctly something like: "Solid bars indicate the 'net radiative forcing', which is the 'radiative forcing' defined in section 2.2 here viewed as the difference between the forcing due to the annual anthropogenic emission (the AIRF, the total height of the bar) and that due to removal of the anthropogenic excess (hatched bars). In effect, the AIRF is the gross forcing due to the anthropogenic emission itself balanced by the "removal RF" which is a consequence of removal processes seeking to restore natural abundance levels". (I think that the word "restore" is a useful one, as it is akin to a "restoring force" familiar in mechanics). [Keith Lassey]	
2-2522	A	121:9		In fourth sentence of caption (line 9) , add comma: "In order … forcing, AIRF compares…" [Melinda Marquis]	Figure deleted
2-2523	A	122:0		Fig. 2.3.8: It looks like there is something wrong with the TOMS data for the most recent 5 yr, and I think they should be removed. [Alan Robock]	Figure updated
2-2524	A	122:4		Fig. 2.3.8 Add semi-colon to first sentence of caption: "from five data sets; both ground-based…" [Melinda Marquis]	Accepted
2-2525	A	123:3	123:3	This figure is interesting, but could be linked better to the text. In particular some discussion of the outliers (Mickley, Shindell) could be useful to highlight the real uncertainties. Also, 'Studies donatedwith' presumably should be 'Studies denoted with'? [Tami Bond]	Figure updated
2-2526	A	123:5		Fig. 2.3.9 Add hyphen: pre-industrial. Change (add a space) : 'Studies donatedwith…." to "Studies denoted with …" [Melinda Marquis]	Accepted
2-2527	A	123:6		I'd add the meaning of TAR in parenthesis [Tiziano Colombo]	Rejected
2-2528	A	123:7	123:7	The caption probably meant to say "denoted" instead of "donated". [Andrew Lacis]	Accepted
2-2529	A	123:7		Figure 2.3.9. Caption. Typo: "donated" should be "denoted". [Philip Cameron-Smith]	Accepted
2-2530	A	123:7		insert a space between "donated" and "with" [Hartmut Grassl]	Accepted
2-2531	A	123:8	123:8	A minor remark: The basis for reducing instantaneous RF by 20% to obtain adjusted forcing could be given with a reference.	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Jan Fuglestvedt]	
2-2830	B	123:9	123:9	reported" or "erroneously reported [Olivier Boucher]	Noted, should be "reported" as stated.
2-2532	A	124:0		Figure 2.4.1: There does not seem to be much (if any) useful information in this figure. But if it is used, I would suggest defining "CDCN" which is used in the caption. [Patrick Hamill]	Figure updated
2-2533	A	124:0		Fig. 2.4.1 Improve lettering on graphic. [Melinda Marquis]	Accepted
2-2534	A	124:0		Figure 2.4.1 is terrific! [A. R. Ravishankara]	Accepted
2-2535	A	124:0		Figure 2.4.1: The definition of "semi-direct effect" here is inconsistent with that in section 2.4.6.1.5 (page 39, line 29-47). Need to modify this figure. [Hongbin Yu]	Accepted
2-2536	A	124:2	124:11	comments: figure lacks thermal infrared contribution to direct effect; cloud bases should be flatter [Hartmut Grassl]	Rejcted
2-2831	B	124:5	124:5	RF mechanisms: this is inconsistent with the chapter saying that some of these mechanisms are not considered as RF ! [Olivier Boucher]	Accepted – figure updated
2-2537	A	124:5		add: "of aerosol particles" after "mechanisms" [Hartmut Grassl]	accepted
2-2538	A	124:9		Figure 2.4.1. Caption. Possible typo: Should "CDCN" actually be "CDNC"? [Philip Cameron-Smith]	rejected
2-2832	B	125:0		ICART is missing [Olivier Boucher]	Noted, campaigns removed from the figure
2-2539	A	125:0		Figure 2.4.2. Not a very good figure. The first panel displays the MODIS Jan-Feb-Mar 2001 averaged tau with AERONET sites and locations of field experiments conducted during that season, and the second panel shows the MODIS Aug-Sep-Oct (I assume also 2001?) averaged tau with Lidar network sites and locations of field experiments conducted during the same season. What about those field experiments that were conducted in other months, which are left out from the figures? For example, INTEX-A in June-July 2004? Also, the ACE-Asia took place mostly in April 2001 and a few days in May 2001 – a period that was NOT in the months shown in Figure 2.4.2. There was only one ACE-Asia flight in March 2001. There have been several ship measurements for aerosols too, e.g., Aerosol 99, and they should be included as part of the in-situ measurements. And what about the IMPROVE, EPA, EMEP, CASTNet, …., all those	Noted, campaigns removed from the figure

No.	Batch	Page:line From	Page:line To	Comment	Notes
				surface measurement networks? The figure simply cannot display all the in-situ/ground-based measurements locations. [Mian Chin]	
2-2540	A	125:0		Figure 2.4.2. The dense AERONET sites superimposed on the MODIS tau have largely blocked the MODIS image of tau over land. [Mian Chin]	Noted
2-2541	A	125:0		Figure 2.4.2. I suggest: (1) Remove the AERONET and Lidar sites and the field experiment locations from the figure. They can be summarized in a table, listing their geographic regions, time and duration of the experiments, and probably the quantities measured. (2) Show 2 panels of MODIS and 2 panels of MISR global maps of tau – March/April and September/October – to demonstrate the dust outflow, biomass burning transport, and pollution influence, and to illustrate that the MODIS does not retrieve aerosol over bright surface while MISR has more difficulties over regions with frequent cloudiness. [Mian Chin]	Noted, campaigns removed from the figure
2-2542	A	125:0		Fig.2.4.2. Symbols for sites so numerous and large that it is difficult to see the coloured areas representing optical depth. [Joanna Haigh]	Noted, campaigns removed
2-2543	A	125:0		Figure 2.4.2: This figure is too busy. The field experiment sites can be summarized in a table, with references and duration of experiment. [Robert Levy]	Accepted
2-2544	A	125:0		Fig. 2.4.2 Improve color contrast for lettering on graphic. Except for commas within definition of "SMOCC," all commas in last sentence should be changed to semi-colons. [Melinda Marquis]	Noted, campaigns removed from the figure
2-2545	A	125:2	125:10	The MPLNET sites require updating. Current and past MPLNET sites should be included to match those given for AERONET. A listing of all MPLNET sites is provided in comments 12 - 33, and is current as of Nov 4, 2005. [Ellsworth Welton]	Accepted
2-2546	A	125:2	125:10	MPLNET Site: GSFC 39.01667 N 76.8667 W [Ellsworth Welton]	Accepted
2-2547	A	125:2	125:10	MPLNET Site: South_Pole 90.0 S 0.0 E [Ellsworth Welton]	Accepted
2-2548	A	125:2	125:10	MPLNET Site: Ny_Alesund 78.91667 N 11.933 E [Ellsworth Welton]	Accepted
2-2549	A	125:2	125:10	MPLNET Site: NCU_Taiwan 24.9667 N 121.1806 E [Ellsworth Welton]	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2550	A	125:2	125:10	MPLNET Site: Syowa 69.5 S 39.58 E [Ellsworth Welton]	Accepted
2-2551	A	125:2	125:10	MPLNET Site: Gosan 33.283 N 128.169 E [Ellsworth Welton]	Accepted
2-2552	A	125:2	125:10	MPLNET Site: Trinidad_Head 41.054 N 124.151 W [Ellsworth Welton]	Accepted
2-2553	A	125:2	125:10	MPLNET Site: Santa_Cruz 28.4725 N 16.2474 W [Ellsworth Welton]	Accepted
2-2554	A	125:2	125:10	MPLNET Site: COVE 36.8833 N 75.7 W [Ellsworth Welton]	Accepted
2-2555	A	125:2	125:10	MPLNET Site: Anmyeon 36.5333 N 126.3167 E [Ellsworth Welton]	Accepted
2-2556	A	125:2	125:10	MPLNET Site: Maldives 6.77667 N 73.1833 E [Ellsworth Welton]	Accepted
2-2557	A	125:2	125:10	MPLNET Site: PRIDE 18.21667 N 65.6 W [Ellsworth Welton]	Accepted
2-2558	A	125:2	125:10	MPLNET Site: Skukuza 24.972 S 31.585 E [Ellsworth Welton]	Accepted
2-2559	A	125:2	125:10	MPLNET Site: Mongu 15.25433 S 23.1505 E [Ellsworth Welton]	Accepted
2-2560	A	125:2	125:10	MPLNET Site: ACE_Asia_Cruise cruise track, cannot fit on graphic [Ellsworth Welton]	Accepted
2-2561	A	125:2	125:10	MPLNET Site: Dunhuang 40.038 N 94.7937 E [Ellsworth Welton]	Accepted
2-2562	A	125:2	125:10	MPLNET Site: KT_Airport_Miami 25.6478 N 80.4328 W [Ellsworth Welton]	Accepted
2-2563	A	125:2	125:10	MPLNET Site: Abracos_Hill 10.7667 S 62.3667 W [Ellsworth Welton]	Accepted
2-2564	A	125:2	125:10	MPLNET Site: NRL_Monterey 36.583 N 121.85 W [Ellsworth Welton]	Accepted
2-2565	A	125:2	125:10	MPLNET Site: MAARCO_UAE2 24.883 N 54.833 E [Ellsworth Welton]	Accepted
2-2566	A	125:2	125:10	MPLNET Site: SMART_UAE2 24.217 N 55.517 E [Ellsworth Welton]	Accepted
2-2567	A	125:2	125:10	MPLNET Site: XiangHe 39.75388 N 116.9619 E	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Ellsworth Welton]	
2-2568	A	125:5		For the sake of clarity I'd add at the beginning of the line " The optical depth …" [Tiziano Colombo]	Accepted
2-2569	A	125:15		correct: "Meditteranean" to "Mediterranean" [Hartmut Grassl]	Accepted
2-2570	A	125:15		spelling of 'Mediterranean' [Joanna Haigh]	Accepted
2-2571	A	126:0		Figure 2.4.3: Caption says panel (b) is over land only, but data is shown over land and ocean. Title for panel (c) says "Mean(StdDev)=-0.15 W/m2" but I think it should say "Mean=-0.15 W/m2". [Theodore Anderson]	Accepted
2-2572	A	126:0		Figure 2.4.3: Looks like that ocean (for MODIS) is plotted as well! [Robert Levy]	Accepted
2-2573	A	126:0		Fig. 2.4.3 In caption, not need to capitalize "Atmosphere" in section about part (e). [Melinda Marquis]	Accepted
2-2574	A	126:0		Figure 2.4.3(b): remake the color bar. There are two "0.1" and two "-0.1". [Hongbin Yu]	Accepted
2-2833	B	126:45	126:45	over land? [Olivier Boucher]	Accepted
2-2575	A	126:48		correct: "Atmospheric" to "atmospheric" [Hartmut Grassl]	Accepted
2-2576	A	127:0	127:	Figure 2.4.4: Since Rotstayn and Liu (2003) provided an updated calculation with the CSIRO model (including an interactive sulfur cycle) it seems more appropriate to use that study in place of Rotstayn and Penner (2001) in the Figure. The 2003 paper gave -1.39 W/m2, or a best estimate of -1.17 W/m2 with the inclusion of dispersion. [Leon Rotstayn]	Accepted
2-2577	A	127:0	127:	In caption of Fig. 2.4.4, I suggest beginning with "Cloud-albedo radiative forcing", since the caption should be clear by itself (without reference to the main text). Albedo could refer to surface albedo. [Leon Rotstayn]	accepted
2-2578	A	127:0		Figure 2.4.4. Skip the last three decimal places in the average and standard deviation given for the first 4 studies in panel b [Ralf Koppmann]	accepted
2-2579	A	127:0		Fig. 2.4.4 Remove background shading for annotated text on panels [Melinda Marquis]	accepted
2-2580	A	127:0		Fig. 2.4.4: Y-axis should be plotted with up being positive, that is 0 at the top and	accepted

No.	Batch	Page:line From	To	Comment	Notes
				negative numbers going down, to correspond to other radiative forcing plots. [Alan Robock]	
2-2581	A	127:24		Figure 2.4.4. Caption. I think the comma after the word "included" should be deleted. A space needs to be added after the period. [Philip Cameron-Smith]	accepted
2-2582	A	127:24		insert a space before "Studies" [Hartmut Grassl]	accpeted
2-2583	A	128:0		Figure 2.5.1: The color scale here is poor. A lot of light brown looks like a little bit of dark brown to my eyes! [Robert Levy]	Accepted
2-2584	A	128:0		Fig. 2.5.1 Close parentheis after defining SAGE. [Melinda Marquis]	Accepted
2-2585	A	128:0		Figure 2.5.1: The top panel of this figure contains a biome map that not not seem to be refered to in the caption or text. If it is not used to clarify some point it should be left out, otherwise a reference for this map needs to be given. [Ina Tegen]	Accepted. Top part will not be used
2-2586	A	128:5	128:9	comment: description incomplete (top part not described) [Hartmut Grassl]	Accepted. Top part will not be used
2-2587	A	129:0		Figure 2.7.1: What are the means and standard deviations in each panel? Are those differences significant? [Robert Levy]	Figure changed
2-2588	A	129:0		Fig. 2.7.1 Add comma to #: 7,538. [Melinda Marquis]	Figure changed
2-2834	B	130:0		Wm-2 rather than Wm2 [Olivier Boucher]	Figure changed
2-2589	A	130:0		Fig.2.7.2. Units of vertical axes should be W/m2/nm [Joanna Haigh]	Figure changed
2-2590	A	130:0		Fig.2.7.2. caption. Make it clear in caption that the curves in pink represent new calculations using model of Lean (2000) [Joanna Haigh]	Figure changed
2-2591	A	130:0		Fig. 2.7.2 Increase thickness of line through data. [Melinda Marquis]	Figure changed
2-2592	A	130:0		Axis of Figure 2.7.2 (top and middle). "W m^2 nm^-1" ? [Scot Martin]	Figure changed
2-2593	A	130:2		comment: ordinate should be W m ?² nm ?1 [Hartmut Grassl]	Figure changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-2835	B	131:0		are --> is ; Wang, Lean and Sheeley -> Wang et al. [Olivier Boucher]	Figure changed
2-2594	A	132:0		Fig. 2.7.4 Remove background shading for annotated text on panels and increase lettering size for annotation and axis labels. [Melinda Marquis]	Noted. revised
2-2595	A	132:0		Fig. 2.7.4: Caption needs to make clear that Amman et al. data set only starts in 1890. [Alan Robock]	Noted. Sentence inserted "Note that Ammann data begins in 1890
2-2596	A	132:4		correct: "um" to "µm" [Hartmut Grassl]	Noted
2-2597	A	133:0	134:	Figures 2.7.4 and 2.7.5. Do volcanic eruptions need two figures? They are not anthropogenic forcings! Or at least I hope not! [Robert Levy]	2.7.5 Figure deleted
2-2836	B	133:0		is it the same background strat aerosols for all 3 datasets? [Olivier Boucher]	Figure deleted
2-2598	A	133:0		What is the legend of the ordinates? [Tiziano Colombo]	Figure deleted
2-2599	A	133:0		Figure 2.7.5: How about x's to denote eruptions? [Robert Levy]	Deleted figure
2-2600	A	133:0		Fig. 2.7.5 Remove background shading for annotated text on panels and increase lettering size for annotation and axis labels. [Melinda Marquis]	Fig deleted
2-2601	A	133:1	133:9	The high frequency TOA radiative forcing oscillations appear to be unphysical. What are they supposed to be due to? [Andrew Lacis]	Figure deleted
2-2602	A	133:1	133:9	There are strong annual positive fluctuations in the black curve from Andronova et al. (1999).These should be removed or explained in the text (in chapter 2 page 54, lines 39-50) [Peter Van Velthoven]	Figure deleted
2-2603	A	134:0		Figure 2.8.1 I like this figure a lot, but would it be helpful to distinguish which point was from which study? [Eleanor Highwood]	Accepted. Figure changed
2-2604	A	134:0		Figure 2.8.1: The figure quality is poor. [Robert Levy]	Noted
2-2605	A	135:0	135:0	One of the most common missconceptions of climate sceptics is that changes in water vapour are not taken into account in models. This missconception is fed by the fact that	Noted. Figure altered

No.	Batch	Page:line From	Page:line To	Comment	Notes
				changes in tropospheric water vapour are - correctly - excluded from diagrams of radiative forcing. Therefore, please make sure to pedagocically explain why water vapour changes (except for the small direct anhropogenic effects) are not shown in Fig. 2.9.1. [Jouni Räisänen]	
2-2606	A	135:0	135:	Comments on Figure 2.9.1 (the poster-child for 2007-2014!) This is a highly influential figure. I commend the authors for making it. It is a great idea and, overall, I like what is in it. However, because it will be looked at so carefully and used for many purposed, some of which may not even be intended (remember the forcing figure from the last assessment!), I think that it is imperative to take a lot of care in making this figure. (a) It is essential to say upfront and in the figure that this is the global top of the atmosphere forcing. Also, it is very important to somehow note that regionality issues are NOT covered in this figure. (b) I suggest that the efficacy column be taken out from this figure. First, I am not sure about its applicability in all cases. Second, it is almost 1 and adds little value. Third, it distracts from the message. (c) The water vapor bar is very ill founded and not justified. If you want to highlight it, include it and show a realistic uncertainty! Can you really rule out zero? Also, it is mostly stratospheric water vapor, right? So, the question of anthropogenic versus natural will be important. [A. R. Ravishankara]	a) It's global tropopause all-sky forcing, will clarify. Reference to forcing figure from TAR – not clear. Thanks for counseling care. Regionall column serves purpose, not possible to put more detail and yet keep the figure simple. b) Rejected. The fact efficacy is close to 1 does make a key point. To illustrate Ravis' point c)Water vapour accepted
2-2607	A	135:0	135:0	I promise you that including anthropogenic water vapour in a figure like this will cause immense (and sometimes mischievous) confusion, as some will think it is referring to the water vapor feedback, from my FAR and SAR experience [Keith Shine]	Noted. Wording changed
2-2608	A	135:0	135:0	I am pleased to see the different time-scales directly referenced on this figure. [Ian Waitz]	Thank you
2-2609	A	135:0		Figure 2.9.1, lower panel. I strongly endorse the ideas of discussing total anthropogenic forcing and of presenting this as a statistical probability distribution. This figure includes a vertical line at a total forcing value of zero (W/m2) and the text (page 62, lines 34-43) uses this line to conclude that total anthropogenic forcing to date is very likely to have been positive. This conclusion is deemed important enough to mention very prominently in the executive summary (page 3, line 29 and also page 2-6, lines 21-22.) However, the zero line is not the only, and probably not the most important, threshold value to show and discuss. A vertical line should be added to the figure at +0.8 W/m2. This is the critical value of total forcing that emerges from the analysis of six "inverse" calculations	Noted. But rejected – already complicated enough. Text added though

No.	Batch	Page:line From	Page:line To	Comment	Notes
				by Anderson et al. (2003, Science, 300, 1103-1104.) Values of total forcing that are more negative than +0.8 W/m2 are inconsistent with every one of these "inverse" studies. Inconsistency, in this sense, implies that anthropogenic forcing cannot be the explanation of the observed, 20th century warming. The forward calculations (discussed in this chapter and summed in this figure) indicate that is it "likely" (but certainly not "very likely" that total anthropogenic forcing is greater than +0.8 W/m2 and is thus a plausible explanation for the observed 20th century warming. This is a more accurate and meaningful way to analyze the total forcing PDF. [See also Chap. 9, Section 9.2.4, page 19, line 3-4 where it is stated that "Top-down studies which use methods closely related to those used in climate change detection research, indicate that the magnitude of the net aerosol forcing is very likely less than -1.7 W/m2." This statement in Chap. 9 should be followed by a reference to the results of "forward calculations" in Chap. 2, shown in Table 2.9.1. I comment on this matter below.] [Theodore Anderson]	
2-2610	A	135:0		Suggestion to use different colors for CH4,N20,Halocarbons to improve clarity [Cathy Clerbaux]	Rejected
2-2611	A	135:0		Figure 2.9.1: I would really like to see Effective RF here: That is what is really important! [Robert Levy]	Rejected. Efficacy not well known enough yet
2-2612	A	135:0		Fig. 2.9.1 Change "disused" to "discussed." Add punctuation to "Bottom" sentence: ranges on top figure, correspond to 90% confidence intervals; these errors are distributed …" Change units on axes to W m-2. [Melinda Marquis]	Accepted
2-2613	A	135:0		Figure 2.9.1. The authors are to be commended for including a value for aerosol indirect forcing. The use in AR3 of an uncertainty range with no value for the forcing implicitly set that forcing to zero in summations. That said, the authors should include a value for dust forcing, unless they really mean that the best estimate is zero with asymmetric uncertainty bars; and if that is in fact what they intend they should explicitly say so. The caption to Figure 2.9.1 should specify the time period of the forcing, eg 1800 to 2000. [Stephen E Schwartz]	Noted. Thanks. Text clarifed
2-2614	A	135:0		lower panel, watts per square metre to W m-2 [Junying Sun]	Accepted
2-2837	B	135:5	135:5	disused -> discussed [Olivier Boucher]	Accepted
2-2615	A	135:5	135:9	The caption clarity could be improved [Cathy Clerbaux]	Accepted
2-2616	A	135:5	135:5	discused	accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Rolf Philipona]	
2-2617	A	135:5	135:5	…mechanism disused in this…" -> "…mechanism discussed in this… [Xiaobin Xu]	Accepted
2-2618	A	135:5		discussed' misspelled [Cathy Clerbaux]	Accepted
2-2619	A	135:5		correct: "disused" to "discussed" [Hartmut Grassl]	Accepted
2-2620	A	135:8		Redraw the lower graph using 95% uncertainty figures; but not "ranges" [Vincent Gray]	Rejected
2-2621	A	136:0	136:0	The direct methane bar looks the same on this figure as on Figure 2.9.1 - is this correct? Also, there should be a bar for halon/bromine emissions. Is the NOx nitrate bar credible. Why isnt black carbon 0.3 (seems much larger). And why are contrails included under aerosols? [Keith Shine]	Methane in Table 2.9.1 is 0.48, in Figure 2.9.2 it is 0.43, the difference due to lifetime effects through CO, NMVOC and Nox emissions. Agree. Halon/bromine should (and have been) added. Uncertainty bars have been included. The uncertainty of RF from NOx nitrate is seen to be quite large. BC. In fig. 2.9.2 the BC bar includes emissions of BC from biomassburning Contrails: Agree that contrails should not be under aerosols and precursors
2-2622	A	136:0		Fig.2.9.2. Nice figure! [Joanna Haigh]	Thanks.
2-2623	A	136:0		Figure 2.9.1 Is the biomass burning component of organic and black carbon folded in with the fossil fuel component to produce these bar charts? They are definied separately in table 2.9.1 [Eleanor Highwood]	BC. In fig. 2.9.2 the BC bar includes emissions of BC from biomassburning
2-2624	A	136:0		Fig. 2.9.2 Change units on axes to W m-2. [Melinda Marquis]	This should be consistent throughout the report.
2-2625	A	136:0		Figure 2.9.2. There is some confusion in the presentation of "NOx" and "SO2". NOx is	One of the main points with this figure

No.	Batch	Page:line From	To	Comment	Notes
				given as a 'short-lived gas' whereas SO2 is shown as an 'aerosol precursor'. Aerosol nitrate (grouped as 'short-lived gas') is the major NOx cooling agent and aerosol sulfate (grouped as 'aerosols') is the major sulfate cooling agent. It seems to me that NOx & SO2 should be grouped together and nitrate & sulfate should be grouped together. I realize that doing so would upset other aspects of the figure (i.e., O3 and HFC are gases). A minimalist fix would be to change "nitrate" to "aerosol nitrate". The fix I would favor would be to list NOx twice, i.e., once with short-lived gases (not showing nitrate there) and then again with aerosols (showing nitrate there). [Scot Martin]	was to show all effects of each primary component in one place, thus we will not introduce a second Nox bar. We will denote nitrate as nitrate aerosol., and remove the distinction between short-lived gases and aerosols.
2-2626	A	136:0		Fig. 2.9.2: Caption needs to say at what time these RFs apply - difference from 1750 to 2004? Also correct spelling: "discussed" and not "disused" [Alan Robock]	It is said that it is the difference between 1750 and 2004.
2-2627	A	136:0		Figure 2.9.2 is an extended version of an earlier (probably one of the most cited) key Figures of IPCC. In this Figure the radiative forcing is (different to earlier assessments) connected to anthropogenic primary compound emissions for all compounds. Many of the released compounds lead to radiative forcing via changes in atmospheric ozone, resulting in negative terms (CFCs and N2O (not assessed ?)) or positive terms (methane, CO, NOx, NMVOC's). Because of these rather complex features I suggest to add for comparison two columns: In the first (additional) column I propose to add the total of (negative) stratospheric ozone forcings, in the second the sum of (positive) tropospheric ozone changes. [Johannes Staehelin]	N2O is now asessed . We find that the figure is quite busy already and really don't want to include extra columns.
2-2628	A	136:0		Figure 2.9.2: In the RF for the emission of NMVOC,RF contribution of NMVOC through formation of secondary organic aerosols (SOA) should not be excluded. There have been many studies dealing with SOA. Here are a few recent articles: (1) Claeys, M., B. Graham, G. Vas, W. Wang, R. Vermeylen, V. Pashynska, J. Cafmeyer, P. Guyon, M. O. Andreae, P. Artaxo, W. Maenhaut, 2004: Formation of Secondary Organic Aerosols Through Photooxidation of Isoprene, SCIENCE VOL 303, 1173-1176. (2) Tsigaridis, K. and M. Kanakidou, 2003: Global modelling of secondary organic aerosol in the troposphere: a sensitivity analysis, Atmos. Chem. Phys., 3, 1849–1869. (3) Tsigaridis, K., J. Lathiere, M. Kanakidou, and D. A. Hauglustaine, 2005: Naturally driven variability in the global secondary organic aerosol over a decade, Atmos. Chem. Phys., 5, 1891–1904. (4) Lack, D. A., X. X. Tie, N. D. Bofinger, A. N. Wiegand, S. Madronich, 2004: Seasonal variability of secondary organic aerosol: A global modeling study, J. Geophys. Res., 109, D03203 doi:10.1029/2003JD003418.	We agree that in principle this is a secondary indirect RF mechanism that should be included in the figure. However, we feel that the state of our understanding is not at alevel where quantitative estimates of the RF can be given (None of the references in the comment gives RF for SOA). In the last sentence of section 2.9.3, a list of RF mechanisms deemed too uncertain to be included in the figure is listed.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Xiaobin Xu]	
2-2629	A	136:1		comment: Aerosols and precursors ? [Hartmut Grassl]	OK
2-2630	A	136:1		This figure does not seem to match the text nor table 2.9.1. Direct sulfate is given in the table as -0.4wm-2 but is less than this in the figure. The direct effect of organic carbon is given as -0.08Wm-2 in the table but is much bigger than this in the figure.Mineral dust is -0.2Wm-2 in the table but smaller in the figure. [Peter Stott]	Numerical values have been corrected to be consistent with the rest of the chapter.
2-2631	A	136:1		Figure 2.9.1 seems to differ from table 2.9.1 for CO2 - given as 1.63 in table 2.9.1 but less than 1.6 in fig 2.9.2. [Peter Stott]	CO2 in figure 2.9.1 and 2.9.2 should not be equal. In 2.9.2 atmospheric CO2 from emissions of fossil CO, CH4 and NMVOC is not included.
2-2632	A	136:1		Where would biomass burning appear in fig 2.9.2 ? Could it be included as a component of the relevant precursor ? [Peter Stott]	Biomass burning is included in each of the primary components (eg. NOx, CH4, CO, NMVOC, BC and OC)
2-2633	A	136:4		Replace "emissions" with " changes in concentrations" [Vincent Gray]	Not correct, Fig 2.9.1 is concentration based while 2.9.2 is emission based.
2-2634	A	137:0		To what extent are the various lines shown here representative of other models and our general understanding? Why do you show just one model? This figure is referred to extensively in Ch9 in the context of attribution. The temporal pattern of forcing is essential in this process, so it is important that as much effort is put into this figure as possible. A major failing of IPCC2001 was the failure to link the forcing chapter to the attribution chapter. There is plenty of information out there to greatly improve this link now. At the very least it is important to put the uncertainty range on these lines, consistent with the famous bar chart. [Kenneth Carslaw]	Noted. It has been difficult to get the radiative forcing estimates from the different models used in IPCC AR4. What we obtained is what is presented here. Although it is hard to make a statement about the represntativeness of the quantitative values of this model relative to others, the results here have a fair resemblance to the plots in TAR which were based on another model. The evolution is consistent with the general understanding for each agent (e.g., emission histories), at least on large spatial (say, global-mean) scales. Agree that the temporal evolution is important for the attribution, but reject the point that the relevant forcing information from the IPCC AR4 GCMs is out there. It is difficult to draw the uncertainty on this plot in contrast to

No.	Batch	Page:line From	Page:line To	Comment	Notes
					the bar chart because such estimates for different time periods have yet to be determined in a robust manner.
2-2635	A	137:0		Fig. 2.9.3 Move top Y axis label so it doesn't overlap with unit on Y axis. Add label to bottom Y axis. [Melinda Marquis]	Noted. Revised.
2-2636	A	137:0		Fig. 2.9.3: Both panels should have matching x-axis sizes and scales so they can be compared. [Alan Robock]	Noted. Revised.
2-2637	A	137:0		Why doesn't Figure 2.9.3 show solar forcing? [Steven Sherwood]	Noted. This model does not include the long-term variations in solar irradiance.
2-2638	A	137:7	137:7	The paper referred to in figure 2.9.3. does not give the trop O3 RF history and these data are not available in the literature. It would be an advantage if these data were available for the reader. [Jan Fuglestvedt]	Noted. The authors give the value for only "ozone".
2-2639	A	138:0		Fig. 2.9.4 Increase lettering size on axis labels, clean up plot heading text line spacing. Change units on axes to W m-2. [Melinda Marquis]	Noted. Will be revised.
2-2640	A	138:0		Figure 2.9.4: Are the forcings by greenhouse gases included in the calculations? [John Seinfeld]	Noted. Yes.
2-2641	A	138:0		Fig 2.9.4 would benefit from common colour scales [Peter Stott]	Noted.
2-2642	A	139:0		Figure 2.10.1. This figure is hard to understand. How can I extract the future climate impact from this information; maybe, the corresponding text should be modified in a way to make this clear. [Ralf Koppmann]	Accepted
2-2643	A	139:0		Fig. 2.10.1 Change inverse unit /yr to yr-1. [Melinda Marquis]	accepted
2-2644	A	139:0		Figure 2.10.1. There is something misleading about this figure. It suggests visually that warming over the next 100 years can be offset by emitting aerosol particles today. Of course, the aerosol particles fall out of the atmosphere in a few weeks. [Scot Martin]	Noted. Text and caption will clarify
2-2645	A	139:5		This whole Figure is confused, since "emissions" are not necessarily directly related to concentrations, or to radiative forcing. I suggest it should be deleted. [Vincent Gray]	rejected
2-2646	A	139:10	139:11	It would be an advantage if you could explain how the uncertainty ranges are established.	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Rms? [Jan Fuglestvedt]	
2-2647	A	139:10	139:11	Please, give the text on estimation on uncertainties of GWPs, refer to the main text, and give full details there. [Ilkka Savolainen]	accepted
2-2648	A	140:0	140:	Add label ''YEAR" to all horizontal axis. Mark the red curve in the middle panel as ''Halocarbons" (also, this curve may be a dashed black line instead of red) [Mikhail Danilin]	Noted. Figure will be updated.
2-2649	A	140:0		Please synchronize ice core data in figure with chapter 6. There are other records available, e.g. Siegenthaler et al, Tellus, 2005, Monnin, GBC, 2004, Flückiger et al. ... [Fortunat Joos]	Noted. Figure will be updated.
2-2650	A	140:0		Figure 2.1: What is the red curve in the middle panel? [Robert Levy]	Noted. Figure will be updated.
2-2651	A	140:0		Q. 2.1 Fig. 1 Increase line thicknesses. Change units on axes to W m-2. [Melinda Marquis]	Noted. Figure will be updated.
2-2652	A	140:0		Question 2.1, Fig. 1: This is the same as TAR. Can't it be updated to 2005? [Alan Robock]	Noted. Figure will be updated.
2-2653	A	140:5	140:5	The central panel of the figure has an undefined red line [Howard Roscoe]	Noted. Figure will be updated.
2-2654	A	141:0	141:0	I am pleased to see the different time-scales directly referenced on this figure. [Ian Waitz]	Noted
2-2655	A	141:0		Question 2.1, Figure 2. If the RF for LLGHGs (CO2, N2O, CH4) are listed separately, why not for aerosols? One bar displaying all aerosol direct RF is so misleading, as I have mentioned repeatedly. [Mian Chin]	Rejected. This figure is consistent with that used in Chapter 2.
2-2656	A	141:0		Efficacy values should be removed as they add nothing to the interpretation of radiative forcing values in the context of Question 2.11 [Joanna Haigh]	Accepted
2-2657	A	141:0		In Figure 2.9.1., since all the bars represent radiative FORCING, it would be more appropriate to refer to the aerosol forcings as "direct FORCING" or "indirect (cloud albedo) FORCING" instead of "direct EFFECT" or "indirect cloud albedo EFFECT", because effect encompases much more than forcing. Also, in addition to the bars showing the TOTAL aerosol forcings, I suggest to show the forcing from each of the main aerosol types (black carbon, organic carbon, sulfate, mineral dust), the same way as in Figure 2.9.2. Such a separation will easily explain why the the total forcing is so small, because the negative and positive forcings cancel out. Otherwise, the total forcing alone tends to	Rejected. This figure is consistent with that used in Chapter 2.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				conceal the real contribution of aerosols to radiative forcing. [Charles Ichoku]	
2-2658	A	141:0		Figure 2.2: See comment #58 [Robert Levy]	Noted
2-2659	A	141:0		Figure 2.2.: Aerosol (type) effects should be separated (e.g. biomass burning vs sulfate vs dust, etc). And should be called forcings? [Robert Levy]	Rejected. This figure is consistent with that used in Chapter 2.
2-2660	A	141:0		Q. 2.1. Fig. 2 Change units on axes to W m-2. [Melinda Marquis]	Acccepted
2-2661	A	141:0		Question 2.1, Figure 2. Is it really necessary to put the same figure twice in different places of a chapter/report? [Xiaobin Xu]	Noted
2-2662	A	141:2		comment: please omit the efficacy column as it is not explained [Hartmut Grassl]	Accepted
2-2663	A	141:3		watts per square metre to W m-2 [Junying Sun]	Accepted
2-2664	A	141:5		State the meaning of the Error Bars. Are they 90% errors, 95% errors, or, as with TAR "without statistical significance". 95% errors are preferable [Vincent Gray]	Accepted
2-2665	A	141:56		Figure 2 from Question 2.1 displays values of climate efficacy. The concept of climate efficacy should be also defined in the figure caption. [Philippe Tulkens]	Rejected. Efficacy column will be removed from figure.
2-2666	A	144:2	144:3	Question 2, Figure 2 -- what is the basis for determining that the scientific uncertainty surrounding cirrus clouds is moderate? The text of the report would indicate a higher level of uncertainty. [Lourdes Maurice]	Noted. This figure is consistent with that used in Chapter 2.
2-1	A	0:0		General comment on Chapter 2 The text of the draft Chapter 2 consists of 111 pages of pdf file. But much of the Chapter consists of iteration of information in previous IPCC Reports. All of this iteration could be avoided by use of appropriate references, and there is little need for any of it because Chapter 1 of the draft is a review of the history of the subject. The draft of Chapter 2 would benefit from being culled to remove iteration of information in previous IPCC Reports because that would avoid doubt concerning its authors' integrity. The packing of a report with irrelevant filler (so the report consists of too many words for many people to read it) provides doubt to the integrity of its authors.	TAR references there for conext. No further comment required

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Richard S Courtney]	
2-2	A	3:17	3:17	Replace the phrase, "give high confidence" with "agree with the suggestion" because the statement in the draft title is simply not true. The sentence is an assertion that derives from the extreme confusion concerning the difference between model emulation and empirical observation of reality that is repeatedly displayed in this chapter. The models' results are not reality: they are merely the outcome of understandings of reality that are built into the models. Studies of the models' results show the behaviour of the models, and studies of the real climate system show the behaviour of the real climate system. Differences between findings of these studies inform about the models and not the climate system, because the climate system is reality and the results of the model emulations are merely virtual realities. So, model predictions do not – and cannot – give confidence about anything that occurs in the real climate system. The model predictions can only indicate the confidence that can be applied to the understandings built into the models because agreement of those predictions with empirical data obtained from the real climate system can provide confidence that those understandings are correct. Richard S Courtney (exp.) [Richard S Courtney]	High confidence follows AR4 uncertanity guidelines,so phrase retained Do not agree with argument
2-3	A	3:21	3:21	Delete the sentence, "There is also some confidence … realistic climate change mechanisms" because this sentence in the draft title is simply not true. The sentence is an erroneous assertion that derives from the extreme confusion concerning the difference between model emulation and empirical observation of reality that is repeatedly displayed in the draft Report. The models' outputs are not reality: they are merely the outcome of understandings of reality that are built into the models. Climate behaviours predicted by the models show the behaviour of the models, and studies of the real climate system show the behaviour of the real climate system. This is because the climate system is reality and the results of the model emulations are merely virtual realities. So, climate behaviours predicted by the models do not – and cannot – give any degree of confidence about anything that occurs in the real climate system. The climate behaviours predicted by the models only indicate the confidence that can be applied to the understandings built into the models because agreement of those predictions with empirical data obtained from the real climate system can provide confidence that those understandings are correct. [Richard S Courtney]	Do not agree with argument
2-4	A	3:21	3:21	Delete the sentence, "There is also some confidence … realistic climate change mechanisms" because this sentence in the draft title is simply not true. The sentence is an erroneous assertion that derives from the extreme confusion concerning the difference	Do not agree with argument

No.	Batch	Page:line From	Page:line To	Comment	Notes
				between model emulation and empirical observation of reality that is repeatedly displayed in the draft Report. The models' outputs are not reality: they are merely the outcome of understandings of reality that are built into the models. Climate behaviours predicted by the models show the behaviour of the models, and studies of the real climate system show the behaviour of the real climate system. This is because the climate system is reality and the results of the model emulations are merely virtual realities. So, climate behaviours predicted by the models do not – and cannot – give any degree of confidence about anything that occurs in the real climate system. The climate behaviours predicted by the models only indicate the confidence that can be applied to the understandings built into the models because agreement of those predictions with empirical data obtained from the real climate system can provide confidence that those understandings are correct. [Richard S Courtney]	
2-5	A	3:39	3:40	Delete the assertion, "at its fastest rate ever observed in the last 2000 years" because this assertion depends on a selective choice of evidence: the assertion is supported by ice core studies (e.g. Neftel) but denied by stomata studies (e.g. Wagner). There is nothing special about "the last 2000 years", and selective use of the stomata data would permit the equally true assertion "at a rate that has repeatedly occurred in the last 2000 years". No report should include such selective use of data, but the assertion based on the stomata data is the more likely to be correct. The ice core data cannot indicate rates of change in atmospheric CO2 concentration similar to those at present (unless they are sustained for more than 160 years) because CO2 will diffuse from regions of high concentration through sealing firn, and the FAR says the ice takes 83 years to seal. But plants grow new leaves each year and adjust the sizes of their stomata with changing atmospheric CO2 concentration, and this permits the determination of rapid changes to past atmospheric CO2 concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). The disagreement of the stomata data with the ice core data is clearly seen in all published studies of the stomata data. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO2 concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO2 concentration is indicated by the stomata data at a time when the ice core data indicate only 20 ppmv rise. (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenberg et al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)).) [Richard S Courtney]	We have assessed, rather than selected evidence
2-6	A	3:45	3:45	"To avoid being very misleading it is necessary to make an insertion between "… TAR."	Do not agree with their asessmment or

No.	Batch	Page:line From	Page:line To	Comment	Notes
				and "Current levels …" that says: "However, since the TAR peer reviewed reconsideration of the evidence has shown that human activities are not making a substantial or significant contribution to increasing CO2 in the atmosphere. The annual pulse of anthropogenic CO2 into the atmosphere should relate to the annual increase of CO2 in the atmosphere if one is causal of the other, but their variations greatly differ from year to year. (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)). Also, the annual increase to CO2 in the atmosphere is the residual of the seasonal changes to CO2 in the atmosphere, and the Northern Hemisphere seasonal changes (decrease and increase) each year are approximately an order of magnitude greater than both the total annual increase and the total annual anthropogenic emission. (Rorsch et al. (2005)). [Richard S Courtney]	suggestion
2-7	A	3:50	3:50	Replace "second" with "third" because the statement in the draft is incorrect when the effect of water vapour is ignored as is the convention in this Chapter except for Section 3.2.8. 1. CO2 has RF of 1.63 Wm-2, 2. particles of sulphate aerosols combined with soot have RF of 0.55 Wm-2 (ref. Jacobson MZ, Nature, vol. 409, 695-697 (2000)) 3. methane has RF of 0.48 Wm-2. The authors of this chapter seem to be ignorant of the warming effect of sulphate aerosols combined with soot particles. [Richard S Courtney]	Water vapours role will be more clearly described in intro and put into context with other gases. See Aerosols section for a fuller context.
2-8	A	4:2	4:2	Page 2-4 Chapter 2 Line 2 of the draft says nitrous oxide is the "fourth most important greenhouse gas" and Page 2-3 Chapter 2 Lines 50 and 51 (wrongly) say methane is "the second largest RF contributor" (assuming that the effect of water vapour is ignored as is the convention in this Chapter except for Section 3.2.8.). But the draft does not state the third largest contributor. Before Page 2-4 Chapter 2 Line 2, the draft needs to be amended to include the RF of particles of sulphate aerosols combined with soot that is the second largest RF contributor. 1. CO2 has RF of 1.63 Wm-2, 2. particles of sulphate aerosols combined with soot have RF of 0.55 Wm-2 (ref. Jacobson MZ, Nature, vol. 409, 695-697 (2000)) 3. methane has RF of 0.48 Wm-2. 4. and nitrous oxide has RF of 0.16 Wm-2. The authors of this chapter seem to be ignorant of the warming effect of sulphate aerosols combined with soot particles. But their correct statement that nitrous oxide is the "fourth	Text will be changed. The draft does state the third largest contributor (CFC12). Don't agree with soot/suphate estimate

No.	Batch	Page:line From	Page:line To	Comment	Notes
				most important greenhouse gas" implies that they are choosing to deliberately ignore the warming effect of sulphate aerosols combined with soot particles. [Richard S Courtney]	
2-9	A	4:40	2:40	The sentence "Anthropogenic water vapour changes are likely to have provided a positive RF." is incorrect and should be replaced with "It is not known if anthropogenic water vapour changes have contributed a net positive or a net negative RF. The observations are consistent with the hypothesis that the water vapour feedback is positive if the possible effect on cloud cover is ignored. However, the increased moisture in the air at all elevations could be expected to increase cloud cover that reflects solar radiation and thus provide a negative feedback that may be of greater magnitude than the maximum possible positive radiative feedback from water vapour." Chapter 1 of the draft says an increase to reflective cloud cover of less than 2% would provide a greater negative feedback than the effect of a doubling of carbon dioxide with the maximum possible positive radiative feedback from water vapour. But such a small change in cloud cover is not yet capable of being observed. These issues are further complicated by our lack of understanding of the processes that govern the behaviours of clouds, including the processes that form clouds. Hence, the net feedback effect of water vapour remains a subject requiring urgent research and, therefore, the sentence needing replacement in the draft is very wrong. [Richard S Courtney]	Text clarified to say which water vapour effects we are talking about
2-10	A	4:45	4:46	There is a need to check if the +0.55 Wm-2 RF of sulphate aerosols combined with soot particles (ref. Jacobson MZ, Nature, vol. 409, 695-697 (2000)) has been accounted as part of the stated value of -0.2 ± 0.2 Wm-2 for the RF of Direct Aerosols. If the RF of sulphate aerosols combined with soot particles has not been accounted, then the statements on lines 45 and 46 need to be amended in the light of the needed correction to the accounting. It is essential that this be done because it seems the authors of this chapter are ignorant of the warming effect of sulphate aerosols combined with soot particles. [Richard S Courtney]	Paper will be cited
2-11	A	4:55	4:55	Between "… be :" and "sulphate …" insert "sulphate aerosols combined with soot particles 0.55 Wm-2" The authors of this chapter seem to be ignorant of the warming effect of sulphate aerosols combined with soot particles (ref. Jacobson MZ, Nature, vol. 409, 695-697 (2000)),. [Richard S Courtney]	Paper will be cited

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-12	A	5:10	5:12	Delete everything on lines 10 to 12 because they are complete and utter nonsense. If "the scientific understanding is low" then a "best estimate" has little if any value. And no estimate provided by computer models of the climate system has any value of any kind when "the scientific understanding is low" because the models are merely formulations of understandings of the climate system. [Richard S Courtney]	comment does not appear to be objective or scientific in nature
2-13	A	5:19	5:19	Replace the phrase "Observations and models indicate that" with "Observations indicate and models emulate that" because the models cannot "indicate" anything except their agreement with (or failure to agree with) the real world for poorly understood climate mechanisms such as "aerosols and aerosol-cloud interactions". The fact that the climate models do emulate the observations is important because it provides some confidence that our very limited understanding of the effects of "aerosols and aerosol-cloud interactions" may be correct. But the climate models are – and can only be – formulations of existing understandings of the real climate. They can be used 1. to test those understandings against empirical data, 2. to explore the limitations of those understandings against empirical data, 3. and to assess the possible behaviours of the climate system according to those understandings but that is very different from "indicating" an effect of the real climate system that is very imperfectly understood. And there exists very little understanding of the effects of "aerosols and aerosol-cloud interactions". (The entire draft report seems to display a great lack of understanding that virtual realities are not reality). [Richard S Courtney]	Do not agree with their asesssment. Observations AND model analyses constitute a good diagnostic strategy. Text retained
2-14	A	5:35	5:35	For accuracy and completeness between "… not globally." and "These effects …" it is necessary to insert; "But local heating contributes to urban heat islands (UHIs) that distort estimations of changes to mean global temperature." Many studies indicate that the urban heat island effect is a substantial contributor to the apparent warming trend in the data sets for mean global temperature. For example, Kalnay and Ming determine that land-use change and urbanisation account for a significant portion of the surface temperature increase of the last century. They determine an effect that is at least twice as great as has been previously estimated for the United States (Kalnay and Cai (2003)) (ref. Kalnay E, and M Cai, Nature, vol. 423, 528–531 (2003)). And Brandsma et al. have demonstrated that urban heat island biases in surface temperature data are not confined to cities but may spread to surrounding rural locations thus causing urban heat island effects much larger in magnitude than was previously thought (Brandsma et al. (2003)). (ref. Brandsma, T., G. P. Konnen, and H. R. A.	Discussed in chapter 3, not the remit for our chapter, we will refer to their discussion

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Wessels, 2003. International Journal of Climatology, vol. 23, 829–845 (2003)) [Richard S Courtney]	
2-15	A	6:2	6:2	For accuracy and completeness after "… numbers were not." it is necessary to insert the following additional paragraph; "However, the effects of solar flares on the atmosphere (e.g. on cloud nucleation) are not known and, therefore, more research to investigate the effects of solar behaviour on climate is needed before the magnitude of the solar effects on climate can be stated with any certainty." [Richard S Courtney]	This bullet refers to dierct effect of solar radiation.
2-16	A	6:17	6:19	There is a need to check if the +0.55 Wm-2 RF of sulphate aerosols combined with soot particles (ref. Jacobson MZ, Nature, vol. 409, 695-697 (2000)) has been accounted as part of the stated value of -0.2 ± 0.2 Wm-2 for the RF of Direct Aerosols. If the RF of sulphate aerosols combined with soot particles has not been accounted, then the statements on lines 17 to 19 need to be amended in the light of the needed correction to the accounting. It is essential that this be done because it seems the authors of this chapter are ignorant of the warming effect of sulphate aerosols combined with soot particles. [Richard S Courtney]	Paper will be cited
2-17	A	6:22	6:22	Replace the words "and moisture surface budgets" with "budget" because it is factually incorrect to state that surface forcing "is a useful tool for understanding" the moisture budget (similarly, a screw driver is not a useful tool for hammering nails). [Richard S Courtney]	Do not agree
2-18	A	6:22	6:22	Replace the words, "However, unlike RF, it" with "It" because it is not correct to assert that surface forcing represents a measure of the global mean surface temperature response: the truth of this assertion in the draft cannot be known until, for example, cloud formation processes are completely understood. And it is very improbable that the assertion could be true because the magnitudes and spatial distributions of latent heat loss from the surface will adjust in response to RF changes. Mistakes of this kind cause doubt concerning the scientific competence of the authors of the draft. [Richard S Courtney]	We do not assert this. Text retained
2-19	A	6:26	6:29	There is a need to check if the +0.55 Wm-2 RF of sulphate aerosols combined with soot particles (ref. Jacobson MZ, Nature, vol. 409, 695-697 (2000)) has been accounted as part of the stated "global-mean surface forcing" and "RF". If the RF of sulphate aerosols combined with soot particles has not been accounted, then the statements on lines 26 to 29 need to be amended in the light of the needed correction to the accounting. It is essential	Paper will be cited

No.	Batch	Page:line From	Page:line To	Comment	Notes
				that this be done because it seems the authors of this chapter are ignorant of the warming effect of sulphate aerosols combined with soot particles. [Richard S Courtney]	
2-20	A	6:31	6:32	There is a need to check if the +0.55 Wm-2 RF of sulphate aerosols combined with soot particles (ref. Jacobson MZ, Nature, vol. 409, 695-697 (2000)) has been accounted as part of the stated "present day surface forcing". If the RF of sulphate aerosols combined with soot particles has not been accounted, then the statements on lines 31 and 32 need to be amended in the light of the needed correction to the accounting. It is essential that this be done because it seems the authors of this chapter are ignorant of the warming effect of sulphate aerosols combined with soot particles. [Richard S Courtney]	Paper will be cited
2-21	A	7:20	7:21	"The statements saying "These mechanisms … do not easily fit within the "radiative forcing" concept. However, as these mechanisms are not routinely or well represented in most current GCM simulations (Jacob et al., 2005) they will be discussed in this chapter in conjunction with the forcing agents." are extraordinary, and the statement saying "they will be discussed in this chapter in conjunction with the forcing agents." should be deleted and all parts of the draft report it alludes to should be deleted or severely amended. If the mechanisms "do not easily fit within the "radiative forcing" concept" then why discuss them "in conjunction with the forcing agents" except as a method to mislead? Mistakes of this kind cause doubt concerning the scientific competence of the authors of the draft. [Richard S Courtney]	Phrase changed for claity, but do not agree
2-22	A	7:22	7:25	The statements from, "This chapter will also …" to "… evaluated with other chapters." should be deleted and all parts of the draft report they allude to should be deleted or severely amended because they are nonsense. "The radiative energy budget changes" at the surface are not "diagnostics for understanding aspects of the climate response" because using them as such a diagnostic would ignore latent heat transfer. Mistakes of this kind cause doubt concerning the scientific competence of the authors of the draft (have they never heard of rain, snow, ice, hail, conduction, melting, freezing, evapouration, etc.?) [Richard S Courtney]	comment does not appear to be objective or scientific in nature
2-23	A	7:26	7:27	The statements from, "and present efficacies …" to "… compared to CO2)." should be deleted and all parts of the draft report they allude to should be deleted or severely amended because they are nonsense.	'comment does not appear to be objective or scientific in nature

No.	Batch	Page:line From	Page:line To	Comment	Notes
				As early as the FAR (1990) tables of RF of different gases relative to the RF of CO2 were introduced (see IPCC 1990, Tables 2.3 and 2.4), and if this is all that is meant by "efficacies" then why not say so? The following comments concern the nonsensical statements in lines 26 and 27. "Radiative forcing" (RF) is a mechanism for warming of the surface that was introduced as a concept in the IPCC's First Assessment Report. The RF of each greenhouse gas (ghg) is its contribution to the total RF. So, there cannot be "present efficacies for various mechanisms" because there is only one mechanism; i.e. RF. However, mechanisms creating the RF magnitude of each ghg (i.e. the radiative properties and atmospheric residence time of each ghg) have been discussed in previous IPCC reports, but these mechanisms do not have "efficacies" as defined in lines 26 and 27. And "the measure of the effectiveness of a given RF agent at changing surface temperature, compared to CO2" is complete twaddle because the surface does not know where radiation comes from, so the surface responds to receipt of 1 Wm-2 of radiative energy regardless of the origin of that energy. Therefore, the "the effectiveness of a given RF agent" is the same for all RF agents although each "RF agent" has an RF relative to the RF of CO2. Nonsense of the kind in lines 26 and 27 causes doubt concerning the scientific competence and the integrity of the authors of the draft. [Richard S Courtney]	
2-24	A	8:27	8:27	Replace "These forcings could be significant on local scales" with "These forcings could be significant on global and local scales" because the statement in the draft Report is incorrect in that it suggests changes to non-radiative effects (e.g. latent heat transfer) are not significant globally. Most of the Earth's surface is covered in water and any increase to global surface heating for any reason will induce additional evapouration from the bulk of the surface of the Earth. Indeed, tropical regions are near their maximum possible temperature of 305K because the warm pool responds to additional heating by additional evapouration that removes the additional heat from the surface. (ref. Ramanathan & Collins, Nature, vol. 351, 27-32 (1991)). Indeed, even the understandings in some GCMs indicate that the evapourative effects over Asia from irrigation provide more cooling than the warming from radiative forcing and this is stated in Page 2-23 Section 2.3.8.2 Lines 27 to 31. It seems the authors of the draft Chapter 2 are embarrassed that the importance of these non-radiative changes has been ignored by fanatical supporters of the radiative forcing concept: Page 2-8 Chapter 2 Section 2.2 Lines 26 and 27 admit that no attempt has yet been made to characterise non-radiative climate effects in terms of radiative forcing, but	We do not agree. Text retained

No.	Batch	Page:line From	Page:line To	Comment	Notes
				the draft report considers all climate change to be dependent on radiative forcing. It is an important and significant advance if the IPCC is – at last – starting to consider climate mechanisms that are more significant to climate change than radiative forcing. [Richard S Courtney]	
2-25	A	8:41	8:41	To avoid being completely misleading and to be factually correct, at the start of Section 2.3 it is essential to include a statement that says the following; "Water vapour is by far the most important radiative greenhouse gas because it provides radiative forcing that is greater than the total of the radiative forcings of all other greenhouse gases. However, the naturally and anthropogenically induced variations of its atmospheric concentration are not known and reasons for these variations are not understood. Therefore, this Report continues the practice of previous IPCC Reports of assuming that changes to the atmospheric concentration of water vapour are a feedback induced by changes to the RFs of other greenhouse gases. Hence, this Report assumes that water vapour differs from all other chemically and radiatively important gases in that it does not have an RF except in Section 2.3.8 of this Report. However, it is important to note that this assumption is adopted for convenience. The assumption cannot be justified as a scientific procedure in the absence of knowledge of the naturally and anthropogenically induced variations of atmospheric water vapour concentration, and this is why stratospheric water vapour is assigned an RF in Section 2.3.8.1, for example." The suggested insertion makes clear that the draft Report provides an inconsistent and illogical treatment of anthropogenic water vapour, but there is no valid reason to conceal this. [Richard S Courtney]	Agree to some rewording. Comment appears to have a confusion about water vapor – "forcing" or "feedback"?
2-26	A	8:50	8:50	"Between the phrases "the preindustrial value" and "was reached" it is very important to insert "as indicated by ice cores" because the statement in the draft wrongly suggests that there was a unique "preindustrial value". The assertion that there was a unique "preindustrial value" is supported by ice core studies (e.g. Neftel) but denied by stomata studies (e.g. Wagner). The ice core data cannot indicate high atmospheric CO2 concentration in preindustrial times because "high" values are assumed to be biogenic artefacts and are deleted from the data sets. And CO2 will diffuse from regions of high concentration through sealing firn during the 83 years that the FAR says the ice takes to seal so high concentrations that existed for less than 160 years cannot be recorded in the ice. Plants grow new leaves each year and adjust the sizes of their stomata with changing atmospheric CO2 concentration, and this permits the determination of high past atmospheric CO2 concentrations by analysis of leaves preserved, for example, in peat	Science does not justify this interpretation

No.	Batch	Page:line From	Page:line To	Comment	Notes
				bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenberg et al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)).) [Richard S Courtney]	
2-27	A	8:51	8:51	After the phrase "about 19 ppm" appropriate context requires addition of the following statement; "which compares to the Northern Hemisphere seasonal variation (decrease and increase) of about 14 ppm during each year." [Richard S Courtney]	Rejected
2-28	A	9:15	9:15	After the phrase "reached by 2010" it is very important to insert the following paragraphs if the Chapter is not to be completely misleading; "However, since the TAR, peer reviewed and published re-appraisal of the evidence indicates that the emissions of CO2 from human activities are not making a substantial or a significant contribution to the increasing CO2 in the atmosphere. The annual pulse of anthropogenic CO2 into the atmosphere should relate to the annual increase of CO2 in the atmosphere if one is causal of the other, but their variations greatly differ from year to year. (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)). Also, the annual increase to CO2 in the atmosphere is the residual of the seasonal changes to CO2 in the atmosphere, and the Northern Hemisphere seasonal changes (decrease and increase) each year are approximately an order of magnitude greater than both the total annual increase and the total annual anthropogenic emission. (Rorsch et al. (2005)). Rorsch et al. conclude; "This paper has considered the flows of CO2 in and out of the atmosphere. It used the disturbance of the natural cycle by current anthropogenic CO2 emission to investigate the cause(s) of alteration to atmospheric CO2 concentration. The considerations of this paper start from the suggestion that the relatively large increase of CO2 concentration in the atmosphere in the twentieth century (some 30%) is likely to have been caused by the increased mean temperature that preceded it. The main cause is possibly desorption from the oceans with an observed time lag of half a century. However, it cannot be excluded that the production rate from other sources, such as microbiological activity, among others, could have increased. " etc. [Richard S Courtney]	Do not agree. Rejected
2-29	A	9:53	54:1	Delete everything from "To a first order …" to "… to Chapter 7." because it is completely wrong. Wrong results are always obtained by using wrong assumptions to do the wrong thing,	Rejected. Do not agree with their interpretation

No.	Batch	Page:line From	Page:line To	Comment	Notes
				and these statements in the draft Chapter are an example of such incompetence. It should be noted that the climate system does not 'know' whether a CO2 molecule is part of the anthropogenic emission or not. So, for example, biota cannot discriminate to not absorb the anthropogenic emission and, therefore, biota absorbs part of the total emission (i.e. natural and anthropogenic). In other words, the assertion that the anthropogenic emission "is only removed over many thousands of years by erosion and sedimentation" cannot be true (whether or not it was asserted in IPCC 2001). However, it could be argued that the increase in atmospheric CO2 is equivalent to a percentage of the anthropogenic emission of CO2. From that argument one can then claim that the anthropogenic emission is responsible for the increase in atmospheric CO2. Hence, one can obtain the assumption that the anthropogenic emission is a significant "additional CO2 added to the active carbon reservoirs during the industrial era". But this assumption does not agree with the following observations. 1. Most importantly, the annual pulse of anthropogenic CO2 into the atmosphere should relate to the annual increase of CO2 in the atmosphere if one is causal of the other by being a significant addition. But their variations greatly differ from year to year. (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)). Indeed, on Page 2-11 Chapter 2 Section 2.2 Line 14 the draft Chapter admits that the airborne fraction of CO2 varies between 30% to 80% on 2 year time scales. 2. The seasonal variation in atmospheric CO2 concentration differs between localities. Typically, seasonal changes in atmospheric CO2 concentration in the Northern Hemisphere vary down then up by about 14 ppmv during each year, and that variation is approximately an order of magnitude greater than both the total increase and the total annual anthropogenic emission for the whole of each year. (Rorsch et al. (2005)). This does not suggest that the emission rate of anthropogenic "additional CO2" is significant when compared to the ability of the system to absorb it. 3. The annual increase in atmospheric CO2 concentration is the residual of the seasonal changes to atmospheric CO2 concentration. But the annual increase in atmospheric CO2 concentration is about 2 ppm which is about an order of magnitude less than the seasonal variation of atmospheric CO2 concentration. This does not suggest that the annual magnitude of "additional CO2" is significant when compared to the ability of the system to absorb it. However, the observations do agree with a different assumption; viz. "The relatively large increase of CO2 concentration in the atmosphere in the twentieth century (some 30%) is likely to have been caused by the increased mean temperature that preceded it. The main cause is possibly desorption from the oceans with an observed time lag of half a century. However, it cannot be excluded that the production rate from other sources, such as microbiological activity, among others, could have increased." (Rorsch et	

No.	Batch	Page:line From	Page:line To	Comment	Notes
				al. 2005) (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)). [Richard S Courtney]	
2-30	A	10:15	10:18	Delete everything from "the combustion of cement production …" to "… biomass burning (Andrea and Merlet, 2001)" because it is completely wrong. Wrong results are always obtained by using wrong assumptions to do the wrong thing, and these statements in the draft Chapter are an example of such incompetence. The assumption that anthropogenic emission is a "driving force for the increases in global atmospheric CO2 since the industrial revolution" does not agree with the following observations. 1. Most importantly, the annual pulse of anthropogenic CO2 into the atmosphere should relate to the annual increase of CO2 in the atmosphere if one is causal of the other. But their variations greatly differ from year to year. (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)). Indeed, on Page 2-11 Chapter 2 Section 2.2 Line 14 the draft Chapter admits that the airborne fraction of CO2 varies between 30% to 80% on 2 year time scales. 2. The seasonal variation in atmospheric CO2 concentration differs between localities. Typically, seasonal changes in atmospheric CO2 concentration in the Northern Hemisphere vary down then up by about 14 ppmv during each year, and that variation is approximately an order of magnitude greater than both the total increase and the total annual anthropogenic emission for the whole of each year. (Rorsch et al. (2005)). This does not suggest that the emission rate of anthropogenic "additional CO2" is significant when compared to the ability of the system to absorb it. 3. The annual increase in atmospheric CO2 concentration is the residual of the seasonal changes to atmospheric CO2 concentration. But the annual increase in atmospheric CO2 concentration is about 2 ppm which is about an order of magnitude less than the seasonal variation of atmospheric CO2 concentration. This does not suggest that the annual magnitude of "additional CO2" is significant when compared to the ability of the system to absorb it. However, the observations do agree with the cause of the observed increase to atmospheric CO2 concentration being emission of CO2 from the oceans. As Rorsch et al. conclude (2005); "The relatively large increase of CO2 concentration in the atmosphere in the twentieth century (some 30%) is likely to have been caused by the increased mean temperature that preceded it. The main cause is possibly desorption from the oceans with an observed time lag of half a century. However, it cannot be excluded that the production rate from other sources, such as microbiological activity, among others, could have increased." (Rorsch et al. 2005) (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)).	Rejected. Do not agree with their interpretation

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Richard S Courtney]	
2-31	A	10:24	10:24	Replace "A key question" with "A question of scientific interest" because the question is not "key" to anything. [Richard S Courtney]	Rejected
2-32	A	10:39	10:40	Delete from "showing a strong correlation …" to "… concentrations (Keeling et al., 2005)" because the correlation is not strong, and the cited graph (Figure 2.3.1) merely shows that atmospheric CO2 concentration and fossil fuel usage have both trended upwards with time (which is hardly news). Indeed, the draft of Chapter 2 admits the correlation is poor in Page 2-11 Chapter 2 Section 2.2 Lines 1 to 7. Anyway, if they did show a "strong correlation" (they don't) then that would prove nothing because correlation does not prove causation. The naked eye is sufficient to observe from the graph that the detrended curves show very poor correlation, and the text does not include the correlation coefficient for the detrended curves: this causes severe suspicion of why the text says the curves show "strong correlation" (are the authors of the Chapter desperate to show something – anything – that could be thought to support their assertion that use of fossil fuels is contributing to the rise in atmospheric CO2 concentration?). [Richard S Courtney]	Agree that "strong correlation" was over stressed - text reworded in draft.
2-33	A	11:9	11:29	Lines Page 2-11 Chapter 2 Section 2.2 Lines 9 to 29 should be deleted and replaced with a paragraph that makes some attempt to be logical and reasonable. Why does the paragraph state "2-year and shorter time scales" and "5 year means" but not annual values for the anthropogenic CO2 "airborne fraction"? If the anthropogenic CO2 is accumulating in the air because it overloads the natural system then a similar proportion of the annual emission should accumulate each year. But Page 2-11 Chapter 2 Section 2.2 Line 14 says the airborne fraction varies between 30% to 80% on 2 year time scales. This strongly indicates that natural variations in atmospheric CO2 are contributing most if not all of the increase to atmospheric CO2. And the fact that the "5 year means" of the airborne fraction show "no significant change over the last 30 years" despite "higher than average increases in global CO2 in several recent years (1998, 2002-2003)" demonstrates that either (a) the airborne fraction is a function of the assumptions and methods used to obtain it or (b) the airborne fraction is independent of the anthropogenic emission. The assertion on Page 2-11 Chapter 2 Section 2.2 Lines 17 to 19 says, "Thus long term trends in atmospheric CO2 growth rate over decades and longer, reflect the atmospheric	Rejected. Do not agree with their interpretation

No.	Batch	Page:line From	Page:line To	Comment	Notes
				CO_2 emission rates from fossil fuel burning whereas shorter term variations are due to other sources and sinks of atmospheric CO_2." But this assertion is fatuous. If the natural system of sinks copes with the emissions as they occur then the emissions cannot accumulate. [Richard S Courtney]	
2-34	A	11:54	11:54	A frustration of attempting to peer review this astonishingly poor draft report is the clear impression provided throughout the draft that the Report is intended to mislead. However, to avoid being extremely misleading and for completeness, it is very, very important to add the following paragraphs after "… are presented in Figure 2.3.3." "However, it is important to understand the limitations of the data obtained from ice cores. The ice core data cannot indicate high atmospheric CO_2 concentrations that had short duration because such "high" values are assumed to be biogenic artefacts and are deleted from the data sets. Atmospheric CO_2 concentrations of 455 ppm have been deleted from the ice core data for this reason (Neftel): the present atmospheric CO_2 concentration is about 376 ppm. This deletion of "high" values is a very reasonable procedure because high atmospheric CO_2 concentrations that existed for less than ~160 years cannot be recorded in the ice. This inability to record short-lived variations to atmospheric CO_2 concentration results from the time the ice takes to solidify and seal. The ice is formed from snow that becomes firn as it solidifies to ice, and the FAR (IPCC 1990) reported that the firn takes 83 years to seal. CO_2 and each other gas will diffuse from regions of high concentration through sealing firn during the 83 years that the firn is permeable. Therefore, high concentrations of CO_2 in the firn will reduce by diffusing to regions of lower concentration unless the high atmospheric CO_2 concentration existed for more than ~160 years (diffusion can occur both up and down through firn that takes 83 years to seal). The snow that fell in the 1930s has not yet sealed so the ice cores cannot indicate atmospheric CO_2 concentrations for the latter part of the twentieth century. Direct measurements of atmospheric CO_2 concentration did not begin until the 1950s when Keeling started to measure it at Hawaii. Hence, it is not possible to make direct comparison of ice core data with direct measurements of atmospheric CO_2 concentrations. Importantly, the short time series of the direct measurements cannot show if the present rise of atmospheric CO_2 concentrations is part of a natural cycle lasting less than 320 years (i.e. with peaks of less than 160 years). The data obtained by Keeling is consistent with an oscillation of atmospheric CO_2 concentration with a sinusoidal period of less than 250 years. Hence, the ice core data cannot indicate if the observed recent rise in atmospheric CO_2 concentration is atypical of past variations in atmospheric CO_2	Rejected. Do not agree with their interpretation

No.	Batch	Page:line From	Page:line To	Comment	Notes
				concentration. These difficulties may be resolved by use of proxy data obtained using other methods, and studies of plant stomata show especial promise for this. Plants absorb atmospheric CO2 through stomata in their leaves. They adjust the sizes of their stomata with changing atmospheric CO2 concentration, and some species grow new leaves each year. This permits the determination of high past atmospheric CO2 concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). Importantly, the stomata data are calibrated both using laboratory studies and against direct measurements of atmospheric CO2 concentration. And it is extremely important to note that the stomata data demonstrate the limitations of ice core data for determination of high atmospheric CO2 concentrations in the past. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO2 concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO2 concentration is indicated by the stomata data at a time when the ice core data indicate only 20 ppm rise." (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenberg et al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)). [Richard S Courtney]	
2-35	A	12:18	12:19	The statement that ice core measurements indicated atmospheric methane concentrations in year 1992 must be false because the firn from 1992 has yet to seal. A scientific report would explain that the reported data were obtained form different sources using different methods. [Richard S Courtney]	Rejected. Do not agree with their interpretation
2-36	A	12:25	12:26	The statement that a change of calibration has resulted in or revealed a systematic error of 90% requires explanation. This is an extraordinary degree of error that shows the limitations of the data concerning atmospheric methane concentration. A separate Section to discuss this is warranted, not a sentence hidden in a paragraph. (When the nature of the rest of the draft Report – see my General Comment – is considered, it seems likely that this important statement has been deliberately hidden). [Richard S Courtney]	Rejected. Comment not scientific in nature
2-37	A	12:50	12:50	The statement that, "Present atmospheric levels of methane are unprecedented in at least the last half million years." is a fabrication that must be deleted. The only source of this statement is the ice core data that is incapable of indicating whether this statement is true	Rejected. Do not beleive their interpretation

No.	Batch	Page:line From	Page:line To	Comment	Notes
				or not. The paragraph admits that the rate of change to atmospheric methane concentration has been very rapid during the recent years when it has been measured in the atmosphere. But ice cores cannot record high atmospheric methane concentrations that existed for less than ~160 years. This inability to record short-lived variations to atmospheric methane concentration results from the time the ice takes to solidify and seal. The ice is formed from snow that becomes firn as it solidifies to ice, and the FAR (IPCC 1990) reported that the firn takes 83 years to seal. Methane and each other gas will diffuse from regions of high concentration through sealing firn during the 83 years that the firn is permeable. Therefore, high concentrations of methane in the firn will reduce by diffusing to regions of lower concentration unless the high atmospheric methane concentration existed for more than ~160 years (diffusion can occur both up and down through firn that takes 83 years to seal). The present rapid variation of atmospheric methane concentration indicates that the ice cores are incapable of indicating whether the atmospheric methane was or was not higher in the past than now. This is especially important because several paragraphs in the Chapter say that reasons for the present variability are not known and are mostly – probably entirely – natural (i.e. not anthropogenic). The statement that, "Present atmospheric levels of methane are unprecedented in at least the last half million years." is a blatant lie. Delete it. [Richard S Courtney]	
2-38	A	14:9	14:17	The statements in the paragraph on Page 2-14 Chapter 2 Section 2.3.2 Lines 9 to 17 are such arrant nonsense that they are not worthy of mention and, therefore, the paragraph should be deleted. The Section admits that the sources and sinks of methane are not known, are not understood, and are varying for reasons that are completely not understood. Hence, the model of Wang et al. (2004) is – and can only be – pure conjecture. Its results are science fiction and not science. The work of Lassey et al. (2005) is pointless because it assumes "the methane sink remains stable" but the Section says these sinks are not known, are not understood, and are varying for reasons that are completely not understood. Hence, the work of Lassey et al. (2005) is not science and it is not even worthy of being described as science fantasy. It cannot be claimed that these publications are cited for completeness because the Chapter makes no mention of many other publications that are of great importance (my review comments provide references to several). The only purpose of this paragraph seems to be to enable the authors to cite publications of their cronies regardless of the nature and worth of those publications.	Comment not scientific in nature

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Richard S Courtney]	
2-39	A	14:21	14:21	To avoid being completely misleading, after "… and Antarctic ice cores." it is very important to add, "However, ice core data cannot indicate high (or low) concentrations of atmospheric methane that existed for less than ~160 years, and the present rapid variation in atmospheric methane concentration shows that it varies much more rapidly than that." This inability to record short-lived variations to atmospheric methane concentration results from the time the ice takes to solidify and seal. The ice is formed from snow that becomes firn as it solidifies to ice, and the FAR (IPCC 1990) reported that the firn takes 83 years to seal. Methane and each other gas will diffuse from regions of high concentration through sealing firn during the 83 years that the firn is permeable. Therefore, high concentrations of methane in the firn will reduce by diffusing to regions of lower concentration unless the high atmospheric methane concentration existed for more than ~160 years (diffusion can occur both up and down through firn that takes 83 years to seal). [Richard S Courtney]	Incorect intrepretation of science
2-40	A	14:26	14:26	To avoid telling a lie in the draft Report, after "… (Figure 2.33 and Table 2.3)" it is necessary to add, "ignoring water vapour that is the major greenhouse gas." (I assume this is an error and not a deliberate lie despite the inclusion of a clearly deliberate lie on Page 2-12 Chapter 2 Section 2.3.2 Line 50). [Richard S Courtney]	Rejected
2-41	A	15:3	15:3	To avoid misleading, the sentence beginning, "Since 1998 …" should be the start of a new paragraph and between "… atmospheric N2O levels" and "have steadily risen …" it is necessary to insert "indicated by measurement of the atmosphere". [Richard S Courtney]	Text clairifed but new paragrapg not necessary
2-42	A	16:56	17:1	Replace from, "Nevertheless the effect …" to "… of the halocarbon RF which" with "The growth of the halocarbon RF has substantially reduced. It" because this assertion is extremely contentious and it disagrees with the statements in Page 2-19 Chapter 2 Section 2.3.7.1 Lines 51 to 54 of the draft Report. The Montreal Protocol assumes that changes to the stratospheric ozone concentration are caused by changes to the atmospheric halocarbon concentration. And changes to the halocarbon RF are calculated from the atmospheric halocarbon concentration. Therefore, according to the assumption of the Montreal Protocol, if "the drivers of the changes" to the stratospheric ozone concentration are not clear then the drivers of the changes to the halocarbon RF cannot be clear. Hence, it is inconsistent and is not reasonable to attribute any of these changes to the "effect of the Montreal Protocol". The illogicality of this	Do not agrree with logic of arguement presented, text retianed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				attribution is emphasised by the sentence on Page 2-20 Chapter 2 Section 2.3.4 Lines 1 and 2 of the draft Report. (The repeated apparent attempts at self-justification that are in this draft make the process of peer reviewing the draft very unpleasant.) [Richard S Courtney]	
2-43	A	19:29	19:29	To avoid being misleading, after "… on past emissions." insert the additional sentence, "However, none of these approaches include consideration of anthropogenic changes to atmospheric water vapour (induced by, for example, land use changes) because they also utilise the assumption adopted throughout this Report (except in Section 2.3.8) that changes to atmospheric water vapour are a feedback." Page 2-23 Chapter 2 Section 2.3.8.2 Lines 34 to 37 suggest that changes to atmospheric water vapour (induced by, for example, land use changes) could be very significant. [Richard S Courtney]	Section and figure will be deleted so comment not required
2-44	A	19:37	19:37	Replace the phrase "the key" with "some of" because these results are not a key to anything. [Richard S Courtney]	Agree to some extent, text reworded
2-45	A	19:39	19:39	Between "from" and "is" insert the missing phrase "five different data sets". [Richard S Courtney]	Reworded as suggested
2-46	A	19:49	19:49	To correct the grammar, replace "decrease" with "decreased". [Richard S Courtney]	Agree, text changed
2-47	A	21:29	21:29	Delete "major" because it is not true. Developments have been made, but these cannot be thought to be "major" until the emulations provided by the models have been demonstrated to show significant improvement by comparison with empirical observations. [Richard S Courtney]	Do not agree, text retained
2-48	A	23:27	23:27	Replace the word "show" with "estimate" because a GCM can only "show" the behaviour of the model: it cannot "show" the behaviour of the real climate because the output of a climate model is merely virtual reality, not reality. Similarly, one can use a pocket calculator to make an estimate but not to "show" anything (the estimate depends on the understandings fed into the calculator or the construction of the GCM). [Richard S Courtney]	Agree with some rewording
2-49	A	23:31	23:31	Replace the word "found" with "estimated to exist" because a GCM can only "find" the behaviour of the model: it cannot "find" the behaviour of the real climate because the output of a climate model is merely virtual reality, not reality. Similarly, one can use a pocket calculator to make an estimate but not to "find" anything (the estimate depends on	Model result is refered to here, text reworded for claeity

No.	Batch	Page:line From	Page:line To	Comment	Notes
				the understandings fed into the calculator or the construction of the GCM). [Richard S Courtney]	
2-50	A	23:43	23:44	Insert "radiative" between "Earth's" and "greenhouse effect" because it is not known if other components of the greenhouse effect (e.g. the convective and evapourative components) have adjusted to compensate for the observed increase to the radiative component of the greenhouse effect. This statement in the draft is extremely (deliberately?) misleading because, for example, atmospheric CO2 concentration is known to have risen and, therefore, it would be surprising if its contribution to the radiative greenhouse effect had not risen, but that does not of itself indicate that the total greenhouse effect has increased. [Richard S Courtney]	Do not agree with argument, tet retained
2-51	A	24:15	24:16	After "… Tegen et al., 1996)" it is essential to insert an additional paragraph reporting the effect of sulphate aerosol combined with soot particles. A paragraph beginning with the following would be appropriate. "Very importantly, one aerosol provides strong positive radiative forcing (Jacobson MZ, 2000). Sulphate aerosols combine with soot particles in the air to create an aerosol with RF of 0.55 Wm-2 (that is greater than the RF of methane 0.47 Wm-2)." The additional paragraph is needed not only for completeness but also to avoid the draft Report being extremely misleading (although the bulk of the draft suggests that it is intended to mislead; see my 'General Comments' at the beginning of these review comments). [Richard S Courtney]	Do not agree with suggeted text. The science of absorbing aerosol species is recognized.
2-52	A	25:6	25:6	After "… cloud droplet broadening.)" it is essential to insert an additional paragraph reporting the effect of sulphate aerosol combined with soot particles. A paragraph beginning with the following would be appropriate. "Very importantly, it has been discovered that one aerosol provides strong positive radiative forcing (Jacobson MZ, 2000). Sulphate aerosols combine with soot particles in the air to create an aerosol with RF of 0.55 Wm-2 (that is greater than the RF of methane 0.47 Wm-2)." The additional paragraph is needed not only for completeness but also to avoid the draft Report being extremely misleading (although the bulk of the draft suggests that it is intended to mislead; see my 'General Comments' at the beginning of these review comments). [Richard S Courtney]	Reference cited

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-53	A	26:42	26:50	There is a need to check if the +0.55 Wm-2 RF of sulphate aerosols combined with soot particles (ref. Jacobson MZ, Nature, vol. 409, 695-697 (2000)) has been accounted as part of the stated values of "-5 Wm-2" for the clear sky DRE of aerosols, for the "-1.6 to -2.0 Wm-2" for the clear sky radiative effect of aerosols over oceans, for the "-1.4 Wm-2" for the clear sky direct RF of aerosols over oceans, and for the "-1.0 Wm-2" for the clear sky direct RF of aerosols over land and ocean. If the RF of sulphate aerosols combined with soot particles has not been accounted, then the statements in Page 2-25 Chapter 2 Section 2.4.2 Lines 42 to 50 and Tables 2.4.1 and 2.4.2 need to be amended in the light of the needed corrections to the accounting. It is essential that this be done because it seems the authors of this chapter are ignorant of the warming effect of sulphate aerosols combined with soot particles. [Richard S Courtney]	Reference cited
2-54	A	27:57	28:1	The assertion that "all the major aerosol species are now included in these global models needs to be checked because it seems the authors of this chapter are ignorant of the +0.55 Wm-2 RF of sulphate aerosols combined with soot particles (ref. Jacobson MZ, Nature, vol. 409, 695-697 (2000). [Richard S Courtney]	Reference cited
2-55	A	28:5	28:5	Replace "Major progress over the results" with "Substantial developments of the models used to provide the results" because the statement is incorrect. Additional complexity of a model does not necessarily mean "Major improvements in the results": it may result in the opposite. The draft Report provides no evidence that the additional model complexity has made any "progress" in the results they provide. [Richard S Courtney]	Reference cited
2-56	A	36:56	36:56	The Section on Combined aerosol species makes no mention of the +0.55 Wm-2 RF provided by sulphate aerosols combined with soot particles that has been reported (ref. Jacobson MZ, Nature, vol. 409, 695-697 (2000)) and at least a paragraph on this is required. This forcing is greater than that of methane. It seems the authors of this chapter are ignorant of the warming effect of sulphate aerosols combined with soot particles. [Richard S Courtney]	Reference cited
2-57	A	41:1	41:9	In line 5, replace the "measurements and models should be combined" with "model results have to be used and are compared to the available results from measurements." And if any of the works of Lohman and Lesins (2002), Anderson et al. (2003) and Quaas et al. (2005) did "combine measurements and models" to obtain their results then all reference to that/these works should be deleted from the draft.	Agree with rewording

No.	Batch	Page:line From	Page:line To	Comment	Notes
				This correction is needed because the statement in the draft Report is – yet another – 'schoolboy howler'. Measurements and models should never be combined (except that results of measurements must be used as input data to models). Such combination removes the possibility of validating the model results by comparison with the empirical data. And the output of a model is only valid to the degree that it has been validated by comparison with empirical data. When an empirical data series is too short, as is the situation in this case, then it may be shown alongside (e.g. on the same graph) as the predictions of a model that are for a longer time series. [Richard S Courtney]	
2-58	A	41:12	41:14	For accuracy, replace the sentence, "Modelling the cloud albedo …" to "… (Lohman and Feichter, 2005)." With "It is not possible to model the cloud albedo indirect and cloud lifetime effects from first principles (Lohman and Feichter, 2005) because the behaviours of clouds and of aerosol-cloud interactions are not understood so their representations in models is crude and differs between models." [Richard S Courtney]	Do not agree – text retained
2-59	A	41:21	41:22	Delete the sentence, "Uncertainties may be underestimates …" to "… from similar biases" because it is propaganda. The sentence would be equally true if the word "underestimates" were replaced by "overestimates". [Richard S Courtney]	Do not agree, uncertanities increase, not decrease
2-60	A	41:22	41:23	Delete the sentence, "Another uncertainty …" to "… RF estimates" because it is untrue. The word "uncertainty" in the draft Report is used to mean "source of uncertainty" and this usage clearly causes problems for the authors. In any subsequent draft it may be helpful if they use the phrase "source of uncertainty". The cloud albedo indirect and cloud lifetime effects are not known. Therefore, any assumptions concerning them are completely uncertain. The GCMs use such assumptions in the absence of understanding of the effects: the GCMs would use models of the effects if the effects were known. But any estimates derived by use of those assumptions are completely uncertain because the assumptions are completely uncertain. Ascribing statistical significance to those completely uncertain estimates would be a 'schoolboy howler' because the statistical significance is an indication of statistical error, and the total error cannot be known if the source data is completely uncertain. Therefore, absence of estimates of statistical significance cannot be "another uncertainty" or "another source	Source of uncertanity adopted, as suggested And setence reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
				of uncertainty". Simply, it is nonsense to assert that failure to conduct statistical analysis of errors to completely uncertain data is "another uncertainty" or "another source of uncertainty". All that can be said is that the estimates are completely uncertain. However, differences between computed statistical significances of the estimates may be used to assess the performance of the model that generated the estimates as the paragraph reports was done by Ming et al. (2005b). [Richard S Courtney]	
2-61	A	42:49	42:51	The sentence "The term "non-radiative forcing" has been proposed … which do not act directly on the radiation budget." deserves a Section of its own and much expansion. This sentence is the first acknowledgement in an IPCC document of non-radiative effects that are probably more important to climate changes than radiative forcing. (Hallelujah!) [Richard S Courtney]	Rejected. Sufficient weight given to these effects, and sticking to objective science.
2-62	A	42:57	42:57	After "… in terms of Wm-2" add "and it is likely to make significant contribution to the urban heat island effect that distorts surface temperature measurements. Several studies indicate that the urban heat island effect is a substantial contributor to the apparent global warming trend in these data sets. For example, Kalnay and Ming determine that land-use change and urbanisation account for a significant portion of the surface temperature increase of the last century. They determine an effect that is at least twice as great as has been previously estimated for the United States (Kalnay and Cai (2003)) (ref. Kalnay E, and M Cai, Nature, vol. 423, 528–531 (2003)). And Brandsma et al. have demonstrated that urban heat island biases in surface temperature data are not confined to cities but may spread to surrounding rural locations thus causing urban heat island effects much larger in magnitude than was previously thought (Brandsma et al. (2003)). (ref. Brandsma, T., G. P. Konnen, and H. R. A. Wessels, 2003. International Journal of Climatology, vol. 23, 829–845 (2003)) [Richard S Courtney]	Out of Chapter scope
2-63	A	45:1	45:2	Delete ", with a slight possibility of a positive RF, although very unlikely" because if the uncertainty bounds are ±0.2 Wm-2 then that is what they are (and the prejudices of the authors of the draft Report are irrelevant). [Richard S Courtney]	Text refers to Myhre et al. Study and retained
2-64	A	45:55	45:56	Replace the sentence, "Although HEP … Crutzen, 2004)" with "HEP may be very important for local climate change in cities (Betts and Best, 2004; Crutzen, 2004) and is likely to make significant contribution to the urban heat island effect that distorts surface temperature measurements. Several studies indicate that the	Not relevant to our chapter

No.	Batch	Page:line From	Page:line To	Comment	Notes
				urban heat island effect is a substantial contributor to the apparent warming trend in these data sets. For example, Kalnay and Ming determine that land-use change and urbanisation account for a significant portion of the gobal surface temperature increase of the last century. They determine an effect that is at least twice as great as has been previously estimated for the United States (Kalnay and Cai (2003)) (ref. Kalnay E, and M Cai, Nature, vol. 423, 528–531 (2003)). And Brandsma et al. have demonstrated that urban heat island biases in surface temperature data are not confined to cities but may spread to surrounding rural locations thus causing urban heat island effects much larger in magnitude than was previously thought (Brandsma et al. (2003)). (ref. Brandsma, T., G. P. Konnen, and H. R. A. Wessels, 2003. International Journal of Climatology, vol. 23, 829–845 (2003)) [Richard S Courtney]	
2-65	A	47:33	47:34	"Replace the sentence, "This unexpectedly large impact … Shine, 2005)" with "Two climate model studies have failed to emulate this unexpectedly large impact (Hansen et al., 2005; Ponater et al., 2005; Shine, 2005)." Models are emulations of reality that are constructed from understandings of real climate behaviours. Measurements indicate real climate. The models provide emulations that are virtual reality and the measurements are of reality. So, the measurements can confirm (or dispute) the outputs of the models, but use of the models cannot confirm the measurements. (The draft Chapter seems to show that its authors have extreme confusion concerning the difference between model emulation and empirical observation of reality and this is yet another example of their problem.) [Richard S Courtney]	Minnis' emperical mode is likely wrong. Models shows this, wording retained
2-66	A	47:46	47:46	Delete the phrase, "and a physical model" because it is a falsehood. Evidence says what it says, and construction of a physical model is irrelevant to that in any real science. The authors of this draft Report seem to have an extreme prejudice in favour of models (some parts of the Report seem to assert that climate obeys what the models say; e.g. Page 2-47 Chapter 2 Section 2.6.3 Lines 33 and 34), and this phrase that needs deletion is an example of the prejudice. Evidence is the result of empirical observation of reality. Hypotheses are ideas based on the evidence. Theories are hypotheses that have repeatedly been tested by comparison with evidence and have withstood all the tests. Models are representations of the hypotheses and theories. Outputs of the models can be used as evidence only when the output data is demonstrated to accurately represent reality. If a model output disagrees with the available evidence then this indicates fault in the model, and this indication remains true until the evidence is shown to be wrong. This draft Report repeatedly demonstrates that its authors do not understand these matters.	Agree, text reworded. Need to use all data, whether coming from observations or model or theories, wisely and objectively.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				So, I provide the following analogy to help them. If they can comprehend the analogy then they may achieve graduate standard in their science practice. A scientist discovers a new species. 1. He/she names it (e.g. he/she calls it a gazelle) and describes it (e.g. a gazelle has a leg in each corner). 2. He/she observes that gazelles leap. (n.b. the muscles, ligaments etc. that enable gazelles to leap are not known, do not need to be discovered, and do not need to be modelled to observe that gazelles leap. The observation is evidence.) 3. Gazelles are observed to always leap when a predator is near. (This observation is also evidence.) 4. From (3) it can be deduced that gazelles leap in response to the presence of a predator. 5. n.b. The gazelle's internal body structure and central nervous system do not need to be studied, known or modelled for the conclusion in (4) that "gazelles leap when a predator is near" to be valid. Indeed, study of a gazelle's internal body structure and central nervous system may never reveal that, and such a model may take decades to construct following achievement of the conclusion from the evidence. (Having read all 11 chapters of the draft Report, I had intended to provide review comments on them all. However, I became so angry at the need to point out the above elementary principles that I abandoned the review at this point: the draft should be withdrawn and replaced by another that displays an adequate level of scientific competence). [Richard S Courtney]	