Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8-1	8	0				In general very prudent and good descriptions of the knowledge base for solar radiative forcing changes. [Bo Andersen, Norway]	Noted
8-2	8	0				NO2, NOx, etc should always be typed with a subscript. [Pieter Aucamp, South Africa]	Accepted, this is taken into account
8-3	8	0				Put legend in box to make it clearer. [Pieter Aucamp, South Africa]	Rejected, legends treated in standard IPCC formats
8-4	8	0				The term well-mixed GHGs is used instaed of Long-lived. I woudl recommend to use Long-lived since even long-lived gases are not necessarly well mixed at all times (e.g. close to large point sources). [Terje Berntsen, Norway]	Rejected, WMGHG will be used and explained carefully
8-5	8	0				I welcome the evolution of the Radiative Forcing chapter since the TAR and AR4. It is nice to see new concepts such a AF and GTP being endorsed by this Chapter. It is also appreciated that the authors have tried to make the chapter relevant to policymakers by estimating RF by species and by sectors. On the minus side, I feel that the wording is not quite as rigorous as it should be and I will try to point weaknesses in the text in my comments. [Olivier Boucher, France]	Noted
8-6	8	0				It would be nice to refer to particular sections or subsections of chapter 7 rather than Chapter 7 as a whole when possible. [Olivier Boucher, France]	Accepted, taken into account
8-7	8	0				This is a well written section for this stage of the process. The text length in the various sections is appropriate and the text is well supported by figures. A good feature is the inclusion of numberical results from previous assessment reports. [David Fahey, USA]	Noted
8-8	8	0				The more extensive treatment of CO2 equivalence metrics is an important policy relevant feature of this chapter. Suggest that it be made clear if this topic is to be covered comprehensively in this chapter or if it will be parsed between sections in the assessments of the 3 working groups as it has in past assessments. If its assessment is to go beyond the physical science basis, suggest that this be made clear as it is a departure from past assessments. [Haroon Kheshgi, United States of America]	Taken into account, We agree that a more extensive treatment of CO2 equivalence is an important policy relevant feature. Re "parsed between sections in the assessments of the 3 WGs": WGIII will also treat the metric issue, but so far we don't know to which extent. Contact is established and our work will be coordinated. However, WGII will probably not write about metrics. That WGI goes beyond physical metrics is not a departure from previous assessments; economic metrics were to some extent discussed in AR4 WGI.
8-9	8	0				citations to Assessments are often too general (e.g., WMO, 2011); if made consistent with past reports they should be to the authors of the relevant chapters. [Stephen Montzka, USA]	Taken into account, some of the citations are modified but some references to earlier WMO reports are kept if they are more general.
8-10	8	0				There is a lot of material in chapter 8 that belongs in chapters 2 and 7, and in many cases has already been presented there. The authors of chapter 8 should revise their text to reduce this duplication. [JOHN OGREN, USA]	Taken into account, agreement with Ch 2, 6 and 7 on cross-chapter issue. More references to previous chapters have been implemented.
8-11	8	0				The chapter refers to "Radiative Forcing (RF)", but often just uses the word "forcing". Is "forcing" meant to be "RF". I suggest checking for consistency throughout [Glen Peters, Norway]	Taken into account - text revised and the following is described 'We use the term 'forcing' in general discussions, and the specific terms RF or AF in cases where the distinction is important'
8-12	8	0				This chapter is greatly improved from the ZOD and the authors are to be commended for that. I do, however, think that it is still closer to a review and not enough of an assessment. This is most apparent in the metrics section and the discussions on atmospheric chemistry effects on RF. The greatly expanded sections on chemical interactions are one of the largest changes from previous assessments. However, these sections suffer from being too general in tone and for not providing estimates for the magnitudes (on RF or metrics) of the effects discussed. Even if complicated processes have significant uncertainties, if the overall effect is likely to be small then this should be stressed instead of all of the uncertainties. Overall, I like the discussion of the new approaches to RF but I suggest some restructuring below and think you are overly harsh on the old RF	Taken into account, more assessment is included in the whole chapter and especially in the mentioned sections.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						approach (as mentioned in several comment below). In my mind the AF is a natural evolution of the old RF approach and for most gases the two give similar results. For other agents (e.g., AIE), the primary benefit is computation simplicity compared with the old approach, as opposed to it being a fundamentally new thing (as it is stated in many places). [Robert Portmann, United States of America]	
8-13	8	0				Chapter 8 is well laid out. The raionale for the observation scenarios is not always laid out. There needs to be much more cross-referencing to Chapter 2 and an explanation of differences in the observations used. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Taken into account, more cross-referencing to Ch2 have been implemented.
8-14	8	0				Dear colleagues, The chapter is starting to shape up well. My comments are mostly focused on the sections that I would have contributed to, if I had been able to continue as a Lead Author. Kind regards, Leon [Leon Rotstayn, Australia]	Noted
8-15	8	0				The aerosol forcing discussion in chapter 7 and 8 deserves a little more harmonization. It is a little confusing to find similar descriptions of eg the aerosol DRF again in chapter 8. [Michael Schulz, Norway]	Taken into account, aerosol forcing discussion in Ch8 is reduced.
8-16	8	0				Is it possible to judge on the models capacity to reproduce observed aerosol concentration, deposition, AOD and brightning trends at least for the last 30 years? This would add confidence in the models to reproduce the forcing history. [Michael Schulz, Norway]	Rejected, details of validation of aerosols belong to Chapter 7
8-17	8	0				Some general discussion related to the global dimming and brightening could be considered in this chapter as it is closely related to radiative forcing. If it is discussed elsewhere, it can be referenced. [Katsumasa Tanaka, Switzerland]	Rejected, see comment 8-16
8-18	8	0				I have an impression that the focus on GTP in the current manuscript is unusually strong in considering the state of discussion concerning the question as to what should be the alternative metric?. In the current draft, it may give a false impression that the report recommends GWP or GTP. I would suggest a paragraph at the beginning of this chapter that states clearly and explicitly why, unlike the previous IPCC reports, GTP is frequently used in this new report. If I may propose, it could be something like as follows: "GWP and GTP are simultaneously used in this report to illustrate the complexity of the issues surrounding metrics. Since AR4, GTP has been more widely used in scientific research. However, other alternative metrics have also been proposed and under dispute (Section 8.1.2.6)." In general, I appreciate the multi-metric approach, a drastic change from AR4. This new approach should be useful to advance the discussion surrounding the metrics and make metric users aware of issues. [Katsumasa Tanaka, Switzerland]	Taken into account, the text modified so it is more balance on the metric discussion in section 8.7 and ES.
8-19	8	0				The chapters is already in good shape. It reads very well. Well done. [Guus Velders, Netherlands]	Noted
8-20	8	0				It was a pleasure to read Chapter 8 "Anthropogenic and Natural Radiative Forcing." The authors present an authoritative and well-constructed overview of the subject matter. Obviously, a huge amount of work has gone into this effort and I applaud the result. I have one major comment and several minor comments. My major comment is that the balance of the discussion is too focused on short- and very short-term effects. While it is well understood that there is no single "correct" time horizon over which to assess radiative forcing impacts, I would argue that it is equally well understood that there are some "incorrect" time horizons (e.g., 1, 5, or 10 years). Climate is defined as average weather over time scales of decades or more. Recognizing this reality, the IPCC in previous reports has presented GWPs evaluated over 20, 100, and 500 year time horizons. It has been argued recently (Wallington et al., Environ. Sci. Technol., 45, 3169, 2011) that a choice of time horizons of a few years is inappropriate because it is inconsistent with the WMO and IPCC definitions of climate and because it discounts the long term impact of CO2 (the largest single actor in anthropogenic forcing of climate change). This argument has been challenged by Borken-Kleefeld et al. (Environ. Sci. Technol., 45, 3167, 2011) who suggest that for reasons of transparency, functionality, and the rate of climate change time horizons it is appropriate to consider time horizons of less than 20 years. Some discussion of the arguments put forward by Wallington et al. and Borken-Kleefeld et al. should be added to Chapter 8. For greater clarity in future discussions it would be helpful for the authors of Chapter 8 to provide an opinion on whether, or under what circumstances, it is useful to evaluate climate impacts of current emissions on time scales of less than 20 years and whether this is consistent with the precedant in previous IPCC reports to consider time horizons of at least 20 years. [Timothy Wallington, USA]	Rejected - one of the things we try to emphasize in AR5 is that choice of time horizon is arbitrary, and so we show time evolution when practical. Short time horizons are clearly relevant in some cases, notably the response to volcanic eruptions which is largest for 1-2 years following large eruptions and is clearly treated as a climate change by the IPCC and the wider community. Hence there is no compelling reason to impose a lower limit at a particular horizon such as 20 years.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8-21	8	0				This Chapter is generally well written. It depends very substantially on computer model simulations, some of which are not completed. More attention needs to be given to fully describing the models used. For example, the inclusion of GTP values is a major change from AR4 but I am not sure how these data were obtained. [Robert Waterland, United States of America]	Taken into account. We will include more documentation on the calculation of GTPs; i.e. give formula for GTP as well as describe the Impulse Response function that has been adopted for the temperature response. But this also applies to GWP, and we will also give more information about the impulse response function used for CO2.
8-22	8	0				Radiative forcing definition refers to chapter 1 - but a more detailed technical definitions (inc. adjusted radiative forcing) needs to be explicit in Chapter 8 (potentially a box). [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - a more detailed description is included in Figure 1, in section 8.1 and definitions given explicit in ES.
8-23	8	0				References often made to AR4 - it is not clear in these instance what chapter, and what year of the results is referred to for different forcing components. Needs to be explicit. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account -Explicit references have been implemented
8-24	8	0				Ramaswamy formula needs to be given in the chapter. We suggest that its basis be revisited, and the an updated discussion of this formula be provided here. See TAR Chapter 06. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected, the formulas are not given in the chapter. However the formula for CO2 is given in the supporting material and for CH4 and N2O it is stated that the same formulas as in TAR are used.
8-25	8	0				Discount rate, damage costs etc - such economic terms need to be explained for the GWP/GTP discussion. Make sure that thus material is being checked by appropriate LA or Contributing Author, preferably from WGIII. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account, we have asked for comments from WGIII. Brian O'Neill (CA who wrote the paragraphs on economic metrics in ch8 WGI) has strong competence in this field.
8-26	8	0				Page 23, l 7-10, please refer to Chapter 6 to ensure consistency regarding methane emissions. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account, text modified with reference to Chapter 6
8-27	8	0				Page 29 - Discussion on 'nuclear war' is beyond the IPCC/WGI scope, and should be removed. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected, after discussion with Co-chair WGI the paragraph will be kept in the chapter, but modified so no mentioning of conflicts nor countries will be made.
8-28	8	0				Useful to implement a consistent structuring across all sections, to the extent that this is sensible. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
8-29	8	0				Include confidence information from Table 8.8 within Fig 8.27. Uncertainty bar should also be added to Fig 8.27. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account, uncertainty ranges included in Fig 8.27
8-30	8	1	0	64	0	There are a large number of grammatical errors in this chapter; please edit. [Susan Solomon, USA]	Taken into account, many grammatical errors have been corrected
8-31	8	1	0			The discussion of metrics goes beyond the discussion of the radiative forcing concept, which is the subject of this chapter. Would it be better if the discussion related to GWP and alternative metrics is placed at the beginning of this chapter? [Katsumasa Tanaka, Switzerland]	Rejected, the metric section has been moved to the end of the chapter
8-32	8	1	1	1		Anthropogenic and Natural Radiative Forcing [Medani Bhandari, Nepal]	Noted
8-33	8	1	1	1		Chapter 8: Anthropogenic and Natural Radiative Forcing [Medani Bhandari, Nepal]	Noted
8-34	8	1	1	64	20	Overall, the chapter structure is quite good although there is quite a bit of repetition about individual trace gases. Firther work is on-going and will be included in the next draft. [Katharine Law, France]	Noted
8-35	8	1	1	119	6	The authors have done a good job getting the chapter to this point. Some sections are already well written, and all convey interesting and useful information. I enjoyed reading it. [John Daniel, USA]	Noted
8-36	8	1	1	119	6	I feel there are several sections in the text where literature results are listed and presented as they would be in a review article, with no assessment of our understanding. I hope that the assessment aspect can be added to all sections where it is currently absent before the 2nd order review. [John Daniel, USA]	Taken into account, more assessment aspect has been implemented by a summary paragraph or subsection in each section

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8-37	8	1	1	119	6	Some sections do a nice job comparing the current results to AR4. This allows the reader to quickly see how our understanding has changed. I hope this can be done in other sections as appropriate. [John Daniel, USA]	Taken into account, by including more AR4 estimates in the beginning of each section
8-38	8	1		119		I congratulate the authors on a excellent job with this First Order Draft. It builds on AR4 very well and brings in important clarity on some issues that were unclear in AR4 and also brings in important new information. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted
8-39	8	1		119		I welcome the discussion of evidence of other anthropogenic drivers of climate change other than radiative forcing, eg: biophysical effects of land cover change beyond albedo change, and the effecs of CO2 and O3 on plant physiology. It is important that this information is retained in future drafts. Indeed I think there is a need for greater prominance of some of this information, eg: in the Exec Summary and some key figures, because the chapter as a whole still gives the casual reader the impression that RF is all that matters. While RF has indeed been a useful concept for advice to inform aspects of mitigation policy (comparing different GHGs) I be, with the increasing prominence of land cover change (including Reducing Emissions from Deforestation and Degradation, REDD) I believe it is becoming increasingly necessary for policymakers to have clarity on the anthropogenic drivers of climate change beyond RF. The same applied to the rapidly-increasing prominence of Adaptation policy, with its focus on regional climate change, for which non-radiative forcings are particularly important. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Taken into account, non-radiative effects are discussed in the ES
8-40	8	1		119		May I suggest that a box or FAQ on "Do some gases in the atmosphere affect climate and the environment in others ways as well as the greenhouse effect?", and discuss the effects of CO2 and O3 on climate, ecosystems and hydrology via plant physiological processes. I think this is an issue that policymakers require clarity on. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Rejected, more discussion on this is issue is included but not included in a box or FAQ.
8-41	8	1				Good FOD. very comprehensive and up to date with literature, great effort. Well written with original ideas and nice figures. [Piers Forster, UK]	Noted
8-42	8	1				Quite long and detailed compared to AR4 chapter. You are trying to do a lot more: AF, spatial distribution, future, sector effects etc. I applaud you but I think you really need to conncetrate on getting the first order basics right. Use of AF is a huge step, you need to be very careful about introducing it, defining it and especially comparing RF and AF. Secondly you should be very explicit about following uncertainty through and each RF unceranity estimate should be clearly and consisitantly described. IYou seem to be really constraining the 2010 forcing to be large and positive, this is also a big change from AR4 and needs to be carefully thought about whether it's true! Good luck! [Piers Forster, UK]	Taken into account, a further discussion in section 8.1.1 on AF is included and the assessment of uncertainty have been done more consistently
8-43	8	1				Too many figures are not really discussed in text and could be dropped. generally figures concentrated in a few sections. Threse could be sprad across the chapter more [Piers Forster, UK]	Taken into account, the number of figures in FOD sections 8.2 and 8.5.2 have been reduced.
8-44	8	1				blank [Piers Forster, UK]	Noted
8-45	8	1				I am concerned that many of the key figures are "placeholders". this limits ability to review this chapter, but even more suggests that the forcings adduced in this chapter have little influence on the CMIP5 modeling of climate over the twentieth century, and that the modelers just go their own way instead of conforming to estimates of forcing summarized in this chapter. [Stephen E Schwartz, USA]	Taken into account, by including link between our section on future AF and Chapter 12. In SOD no there are no placeholders.
8-46	8	1				Some dialog between authors of chapters 5 and 8 on the volcanic section might be helpful to insure that they are saying similar things. I think that there are some minor inconsistencies at this point. [Larry Thomason, United States of America]	Taken into account, clarification between Ch5 and Ch8 have been done.
8-47	8	2	1	3	44	Generally ES is adequate but piece meal. Might consider making a few headlines, and putting RF in context, saying how other chapters use it? [Piers Forster, UK]	Taken into account, the structure of ES is modified. The relation to other chapters have not been included
8-48	8	2	1	3		State what the uncertainty range is 5-95%? For forcing numbers. Also helpful to have + signs for +ve forcings.. Oh, just found one page 3. Iwould move this into main text or at least a footnote [Piers Forster, UK]	Taken into account, the uncertainty is included as a footnote.
8-49	8	2	1			It is better not to use acronyms in the executive summaries. Rather use the full text to prevent people reading only the summary to get confused. [Pieter Aucamp, South Africa]	Rejected, the acronyms are still included in ES, but all acronyms are defined in ES

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8-50	8	2	1			The most essential information as regards to radiative forcing is contained in figure 8.27. Consequently, this figure might serve as a basis for the text in the executive summary. The text should also be reordered dealing with the most important forcing first. [Øyvind Christophersen, Norway]	Taken into account, by reordering the ES and dealing with the most important first.
8-51	8	2	3	2	3	Change "the global mean temperature response" to "the response of global mean temperature" [Larry Horowitz, USA]	Taken into account, text revised and this part of the sentence removed.
8-52	8	2	3	2	4	should this be "comparing... temperature response ACROSS…." or something else? doesn't make sense as is [Piers Forster, UK]	Taken into account, text revised and this part of the sentence removed.
8-53	8	2	3	2	6	This first bullet is not very informative. I would rather combine the RF and AF concepts in one bullet, and discuss the continuity (or lack of) from AR4 values in a separate bullet. [Olivier Boucher, France]	Taken into account, text revised the first two bulletts are merged.
8-54	8	2	3	2	6	The concept behind radiative forcing is relatively complicated and a good explanation here is vital for the understanding of the rest of the text. While keeping in mind that high RF represent high climate impact, the essence might be best summarized as in the caption to figure 8.27: "Values represent RF in 20XX due to emission changes since 1750". The current text might be confusing. We propose that the bullet point is rewritten on the basis of Chapter 8.1.1. giving the reader a better understanding of the term in relation to timeframe, molecular properties vs. quantity and natural- vs. anthropogenic forcing. An explanation of the relation between RF and GWP might also be useful in this context. [Øyvind Christophersen, Norway]	Taken into account, text revised and description of RF and AF have been improved.
8-55	8	2	3	2	11	There is a value judgement place on RF ("valuable"), but no such judgement placed on AF. Does this imply anything? Does this mean the AF is not valuable? And the first sentence is a little unusual, "RF is valuable for comparing ...temperature". RF to T is not a direct relationship (perhaps at equilibrium), the efficacy may vary with component, etc. Do you mean something like "RF is a useful concept to provide an indication of the global mean temperature response to..."? [Glen Peters, Norway]	Taken into account, text revised and 'valuable' is removed.
8-56	8	2	3	3	42	This entire Chapter is based on the "greenhouse" theory, which follows a real greenhouse in ignoring completely the real climate. Traditional meteorology has studied the climate for more than 200 years with scientific methods, observastions and techniques, and presents the current scientific view of the climate to all of us every night with the weather forecast. "Weather" is not just local it is global, and many media presentations emphasise its universality. The real climate is controlled by air pressure, air and ocean movements, directions and intensity, convection and evporation/precipitation of water in all its forms, ocean oscillations, changes in the sun and in the earth's orbit, and volcanism. They all play an intricate part. None of it could exist without the sun's radiation which warms the earrth by day, warmth which is psrtially lost by night..The replacement of this dynamic system, highly dependent on a knowledge of movement of fluids, has been replaced by an idealised static flat unchanging earth with permanent sunshine, day and night, entirely dependent on "balanced"radiation exchanges.. It is completely inrealistic and is unsurpringly far less successful at forecasting future climate than traditional meteorolopgy, with all of its limitations. [VINCENT GRAY, NEW ZEALAND]	Rejected, there is very high confidence that anthropogenic GHG are the main cause for climate changes over the industrial era. This chapter is about the different drivers causing the climate change.
8-57	8	2	3			The first sentence gets this chapter off to a wrong start: "The radiative forcing (RF) concept is valuable for comparing the global mean temperature response to most of the various components affecting Earth's radiation balance." Knowledge of the radiative forcing is essential to understanding human and natural influences on climate in the past and in the future. We wouldnt be concerned over climate change if it were not for radiative forcing. The sentence needs to speak much more strongly and directly to the importance of knowledge of rad forcing to consideration of climate change past and future. [Stephen E Schwartz, USA]	Taken into account, text revised and the suggested sentence starting with 'Knowledge of the radiative …' is included.
8-58	8	2	4	2	4	The word "components" is very unspecific. What about "atmospheric components"? [Dirk Olivié, Norway]	Taken into account, text revised so 'components' is replaced by 'mechanisms'
8-59	8	2	6	2	6	"time period" is redundant. "period" should suffice. This occurs elsewhere and I likely did not catch them all. Global search and replace should do the trick. [James Butler, United States of America]	Accepted, text revised as suggested
8-60	8	2	8	2	16	These bullets don't really define AF. The second one is quite technical but not really true - what about models with interactive vegitation (e.g.) I would try to devlop tight definitions for both and use these. Maybe bullet three isn't needed if glossary has good AF definition? Bullet 2could be reworked to clealry state the benefits	Taken into account, text revised to make the definition clearer

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						and drawbacks of AF? Quantifies processes not previously evalauted but leads to extra uncertainty and model dependence [Piers Forster, UK]	
8-61	8	2	8	2	16	The AF is difficult to explain in an Executive Summary, and I suspect I would have struggled to understand it if I wasn't already familiar with the concept. The paragraphs starting at lines 8 and 13 use different terminology "rapid feedbacks" and "rapid responses". By line 9, the reader might be wondering what AF is. I attempt to reorder and modify the sentences as follows: Adjusted forcing (AF) characterizes some of the more complex forcing agents that involve rapid feedbacks (called "rapid responses") with some components of the atmosphere that are assumed constant in the RF concept. Whereas in the RF concept all surface and tropospheric conditions are kept fixed, AF allows all variables except the ocean and sea-ice cover to respond to perturbations. (Next dot point...) Rapid responses in AF include, for example, aerosol-induced changes in cloud cover (or "lifetime"), which occur on a time scale much faster than responses of the ocean (even the upper layer) to forcings. The AF and RF values are significantly different for the anthropogenic aerosols, due to their influence on clouds and on snow cover. (I note the comment in 7.4.3.1 that "lifetime" is not very accurate, but it might still be useful, because many readers will recognise it from earlier IPCC reports.) [Leon Rotstayn, Australia]	Taken into account, text revised to a large extent in accordance to the suggestions.
8-62	8	2	8	2	16	The benefits of the adjusted forcing concept don't come across clearly in the executive summary bullet points (they do in the chapter). The advantages or reasons for using AF need to be included (briefly) in the third bullet point. [Oliver Wild, United Kingdom]	Taken into account, text revised with merging of bullets to highlight the difference in RF and AF and better explain the RF and AF.
8-63	8	2	8			"rapid feedbacks". These are not feedbacks as the term is conventionally used in climate research. better "rapid adjustments" or "rapid changes". This usage of the term "feedback" seems to be explicitly derogated at page 5, line 24. [Stephen E Schwartz, USA]	Accepted, text revised as suggested to 'rapid adjustement' which will be used througout this chapter as well as in Chapter 7.
8-64	8	2	9	2	9	Refer to fig. 8,1 [Michel Petit, France]	Accepted, the new structure includes reference to each section and relevant figures/tables.
8-65	8	2	9			Are we using "rapid feedbacks" or Radid adjustments - I thought the latter? [Piers Forster, UK]	Accepted, text revised as suggested to 'rapid adjustement' which will be used througout this chapter as well as in Chapter 7.
8-66	8	2	13	2	16	I think it is useful to give some ranges for what is considered a rapid or slow responses [Guus Velders, Netherlands]	Rejected, this information will be included in section 8.1, which is referred to in the end of the paragraph.
8-67	8	2	14	2	14	Actually this is not quite true. The LH and SH fluxes over the ocean can respond in the AF computation. [Olivier Boucher, France]	Taken into account - the definition in section 8.1 has been made clearer. LH and SH fluxes are to the air, so not part of the ocean response per se (as in a fixed-SST setup, they do not change the ocean conditions themselves even if more or less heat goes out).
8-68	8	2	18	2	24	It would also be useful to give an indication of the varability in RF within a solar cycle [Guus Velders, Netherlands]	Rejected, not suffiently important to mention in an ES, it is in the chapter.
8-69	8	2	19	2	19	You need to say upfront what the ranges refer to (I assume they're 2 sigmas). [Olivier Boucher, France]	Taken into account, the uncertainty is included as a footnote.
8-70	8	2	26	2	27	You should explicitly mention the potential impact of volcanoes on decadal changes in forcing/surface temperatures to expand on the "few years" of the first sentence. [John Daniel, USA]	Rejected, the decadal 2000-2010 is given in the paragraph.
8-71	8	2	26	2	29	You need to say if the RF is relative to 1750, 2000 or average stratospheric conditions. [Olivier Boucher, France]	Rejected, it is stated that it is for the 2000 to 2010 period.
8-72	8	2	26	2	29	Is it all volcanoes what about chinese coal, section may give me the answer but I haven't read it yet? [Piers Forster, UK]	Rejected, further information is given in the section.
8-73	8	2	26		29	I'd suggest 'can have a large impact on the climate for up to few years' as opposed to the current text since the impact is dependent on latitude and altitude of the injection and major is a fairly nebulous term (how big is	Accepted, text revised as suggested

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						major?). [Larry Thomason, United States of America]	
8-74	8	2	28	2	28	Remove comma after "eruptions" [Larry Horowitz, USA]	Accepted, text revised as suggested
8-75	8	2	28	2	28	Typo with data missing [James Wanliss, USA]	Taken into account, the placeholder for the uncertainty is removed.
8-76	8	2	28	2	28	Do we know what "XX" is? [Robert Waterland, United States of America]	Taken into account, the placeholder for the uncertainty is removed.
8-77	8	2	28	2	29	I do not believe that it has been proven that the volcanic eruptions are the full cause of the -0.1 W/m2 from 2000-2010. [John Daniel, USA]	Rejected, the episodic variability in the stratospheric AOD indicate that volcanic eruption is the major cause of the increase in stratospheric AOD
8-78	8	2	31	2	35	The relevant LLGHG should be listed by name. Preferable each of these (also N2O) should be dealt with in separate bullet points. [Øyvind Christophersen, Norway]	Rejected, all main WMGHG are given with chemical formulas. The structure of the ES is modified so that RF from WMGHGs is given in 2 paragraphs
8-79	8	2	31			In my judgment the uncertainty associated with LW ghg forcing is highly overly optimistic. To my thinking the best paper on the subject is Collins 06 JGR RTMFIP, which found about 10% concurrence with line by line models in _cloud free sky_ and did not even attempt to determine the forcing for cloudy sky because of the different treatment of clouds in the different gcms. If this dedicated study could not determine the forcing in the presence of clouds, what justification is there for extending the 10% number for cloud-free sky to the all sky-situation. Even the often quoted 3.71 W m-2 for CO2 doubling, which goes back to Myhre 1998 GRL (as ln 2 * 5.35) is overly precise, in my judgment (the ln 2 is well known; it is the 5.35 that is uncertain). The forcing is in the range of 3-4 W m-2. This sort of spread is shown in various model intercomparisons, such as Webb clim dyn 06, Fig 1a. See also Gregory and Webb J clim 08 on adjusted CO2 forcing. This leads to definitional issue associated with CO2 forcing; Gregory and Webb suggest 1 year as "rapid". So all of this must take its toll on the estimate of the accuracy with which the forcing of doubled CO2 is known. [Stephen E Schwartz, USA]	Taken into account - the text in section 8.3.1 has been revised and uncertainties for the LLGHG is discussed more in detail. Further, references have been included as the basis for the uncertainty.
8-80	8	2	32	2	32	I would prefer to see a year rather than "AR4" for the forcing reference. You could always add the year with either the year or 'AR4' in parenthesis. [John Daniel, USA]	Accepted, year is added in parenthesis after AR4
8-81	8	2	32	2	32	Give reference year for AR4 RF [Larry Horowitz, USA]	Accepted, year is added in parenthesis after AR4
8-82	8	2	37	2	39	N2O is said to become the "third largest LLGHG RF component" shortly. Currently, this position is held by tropospheric ozone. It would be helpful to mention this fact here. [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Rejected, tropospheric ozone is not a LLGHG (long-lived GHG).
8-83	8	2	37	2	44	I presume N2O thrid largest overtaking halocarbons? Halocarbon forcing not given. I would have said that N2O is currently theird largest forcing?? It depends if you bunch halocarbons doesn't it? Not well defined [Piers Forster, UK]	Taken into account, it is explicitly given that the current third largest LLGHG is CFC-12
8-84	8	2	38	2	38	Expected on what grounds ? Extrapolated ? [Michel Petit, France]	Taken into account, text revised to provide information on why it is expected
8-85	8	2	38	2	39	The sentence about N2O becoming the third largest is relevant, but does not follow well here, since no only two other compounds are mentioned before [Guus Velders, Netherlands]	Taken into account, text revised see response 8-83 and 8-84
8-86	8	2	38			From the NOAA observations, radiative efficiencies for halocarbons given in WMO-2010, and the methods for calculating RF for N2O from Ramaswamy et al (2001), N2O exceeded CFC-12 and became the 3rd largest single contributor to direct Radiative Forcing from LLGHGs in 2009… [Stephen Montzka, USA]	Rejected, with proper inclusion of overlap between CH4 and N2O in the TAR formula CFC-12 is currently the third largest LLGHG
8-87	8	2	39	2	39	Do the next 1-2 years refer to the year 2010? So in 2012, we can expect N2O to be the third largest LLGHG RF component. What is the third largest LLGHG RF at the moment? [Olivier Boucher, France]	Taken into account, text revised to state 2013
8-88	8	2	39	2	39	Mention the compound that it is expected to pass. [John Daniel, USA]	Taken into account, it is explicitly given that the current third largest LLGHG is CFC-12

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8-89	8	2	39	2	39	For clarity, please put the actual years (e.g. year 2015) instead of "the next 1-2 years". [Katsumasa Tanaka, Switzerland]	Taken into account, text revised to state 2013
8-90	8	2	41	2	41	Propose the following change: "The total RF from halocarbons…" [Øyvind Christophersen, Norway]	Taken into account, text revised with inclusion of 'all' before halocarbons
8-91	8	2	41	2	44	HFC-gases should also be dealt with under this bullet point. [Øyvind Christophersen, Norway]	Taken into account - the following sentence is included 'Since AR4 (2005) the RF from all HFCs has increased by 60% and has a RF of almost 0.02 Wm-2'
8-92	8	2	41	2	44	Give the RF value for halocarbons [Guus Velders, Netherlands]	Accepted, included as suggested.
8-93	8	2	43	2	43	Change "former" to "first" [Larry Horowitz, USA]	Accepted, see 8-94
8-94	8	2	43	2	43	Replace "former three" with "first three of these". [Robert Waterland, United States of America]	Accepted, modified as suggested
8-95	8	2	43		44	Text is misleading, increases in HCFC-22 have not "more than compensated" for the decrease in RF from the 3 major CFCs. From NOAA measurement records and radiative efficiencies for halocarbons given in WMO-2010, the summed RF from CFC-11, CFC-12, CFC-113 and HCFC-22 has been constant at 0.2935 since 2002. The value has varied from 0.2933 to 0.2937 during this period, and not with a consistent overall trend. The body of the chapter has this point accurately stated (section 8.4.2.4) because the text there mentions HFCs in addition to HCFC-22. [Stephen Montzka, USA]	Taken into, 'more than' is removed.
8-96	8	2	46	2	47	Why are the '80s chosen as the decade of reference? What about the 90s and the 00s. This seems like an arbitrary comparison. [John Daniel, USA]	Taken into account, 'and early part of 1990s ' is included
8-97	8	2	46	2	47	Why 1980s? What about the 90s, or other decades? And what about CO2 alone? Please craft a longer and more complete paragraph. [Susan Solomon, USA]	Taken into account, 'and early part of 1990s ' is included
8-98	8	2	49	2	49	It should be mentioned that tropospheric ozone may also exert a further indirect radiative forcing due to its impact on the ecosystem carbon sink (as alluded to very briefly on page 8-22 line 41 (citing Sitch et al, 2007b). [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Taken into account, text revised and following sentences are added 'There is strong evidence that tropospheric ozone also has a detrimental impact on vegetation physiology, and therefore on its CO2 uptake. This reduced uptake may lead to an indirect increase in the atmospheric CO2 concentration. Thus a fraction of the CO2 RF, the value of which is well known, should be attributed to ozone rather than direct emission, but there is a low confidence on the quantitative estimates.'
8-99	8	2	49	3	2	Compare ozone and aerosol forcings with AR4 estimates [Larry Horowitz, USA]	Accepted, sentence in the ozone and aerosol paragraphs are included with comparisons of AR5 estimates with AR4 estimates.
8-100	8	2	50	2	51	"stronger links between the changes tropospheric and stratospheric ozone"- rather vague, in what sense? [Ruth Doherty, UK]	Taken into account, text revised with inclusion of the following sentence 'Ozone is not emitted directly into the atmosphere; instead it is formed by photochemical reactions. Tropospheric ozone RF is largely attributed to increases inanthropogenic emissions of methane, carbon monoxide, volatile organics and nitrogen oxides, while stratospheric RF results primarily from ozone depletion by halocarbons.'
8-101	8	2	50	2	51	Please specify what causes the link between the changes in stratospheric and tropospheric ozone. [Katsumasa Tanaka, Switzerland]	Rejected, this information will be included in section 8.3.3. However, some further information is included see response to comment 8-101.
8-102	8	2	55	3	2	Providing total direct aerosol forcing without separating out the warming and cooling components confused	Rejected, Chapter 7 will provide this kind of

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						policy makers and the public in AR4. Please separate out the major warming particles (e.g., fossil-fuel soot) and cooling components of aerosol radiative forcing, just like this is done for gases. [Mark Z. Jacobson, U.S.A.]	information in the ES. Chapter 8 summarizes the findings in Chapter 7. Additional information on the components is given in the chapter and in one of the main figures.
8-103	8	2	55	3	2	Here the aerosol RF is described in terms of direct and indirect effect. If possible, please say something on the total (net) effect. [Shigeki KOBAYASHI, Japan]	Taken into account, a sentence on the total aerosol effect is included.
8-104	8	2	56	2	56	The first time BC is used in this chapter, remind the meaning [Michel Petit, France]	Taken into account, BC is defined
8-105	8	2	56			BC not defined. I don't think Chapter 7 wants to call it cloud albedo - should coordinate terminology [Piers Forster, UK]	Taken into account, BC is defined and cloud albedo changed to aerosol-cloud-interaction
8-106	8	3	1	3	2	It would be useful to also give bounds for total (direct+indirect) aerosol forcing [Larry Horowitz, USA]	Taken into account, a sentence on the total aerosol effect is included.
8-107	8	3	1			The executive summary needs to say something more about metrics, perhaps: "There are several methods to compare different greenhouse gases, e.g, physics based metrics such as GWP, GTP and SGTP/IGTP and economics based indicators such as the relative damage cost and the cost effective trade off. It is shown that GWP is rather similar (in interpretation and numerical values) to IGTP/SGTP and the relative damage cost, whereas GTP and the cost effective trade off are similar. When comparing a greenhouse gas with that of CO2, the choice of time horizon is very critical (more critical than whether to opt for say GWP or IGTP) as is the choice between a metric that is integrating the impact of the emission or one that is, like GTP, only focusing on the effect of a pulse emission at one point in time. " Perhaps the sentence on page 9, line 46, "choices of time frames and impact parameter are policy related and cannot be based on science alone" should be lifted to the executive summary. [Christian Azar, SWEDEN]	Accepted. Yes, we will say more about metrics in the ES.
8-108	8	3	4	3	5	Also mention CO2 fluxes resulting from landuse changes [Larry Horowitz, USA]	Rejected, there is not enough space in the ES to og into that kind of detail. This information is provided in section 8.3.5
8-109	8	3	4	3	5	What are the examples of "other modifications"? [Katsumasa Tanaka, Switzerland]	Rejected, there is not enough space in the ES to og into that kind of detail. This information is provided in section 8.3.5
8-110	8	3	4	3	7	I would suggest modulating this conclusion based on my comment for chap 8, p. 42, l. 25-35. "Current evidence suggest a cooling effect of land cover change (i.e. deforestation) at high latitudes and a warming effect at low ones. Large-scale land cover changes may also modify regional precipitation patterns" [Pierre Bernier, Canada]	Rejected - not sufficiently robust findings to be included in the ES.
8-111	8	3	4	3	7	An important paragraph, especially the part about non-radiative forcings. It is important to retain this paragraph in the Exec Summary in future drafts, and also I recommend that this point should be mentioned in the SPM and TS. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted
8-112	8	3	6	3	7	The sentence on temperature change is out of place here, since up to now only RF is discussed [Guus Velders, Netherlands]	Rejected - we agree this is beyond the radiative effect but no other chapter discuss this issus. Several other reviewer comments would like to include more about non-radiative effects on this issue.
8-113	8	3	9	3	9	It wasn't clear from this sentence whether the values for RF and AF are different measures of the same thing, or whether one includes effects that are excluded by the other. It gradually made sense as I read the chapter, but it might need some elaboration in the Executive Summary. [Leon Rotstayn, Australia]	Taken into account, the structure of the ES is changed and this main result is moved just after the description of RF and AF.
8-114	8	3	9			Worth saying something about this. It's very cleary positive compared to AR4? Can you really be that confident in your uncertainties? Why has it come down so much, is it all aerosol or larger LLGHG forcing etc. [Piers Forster, UK]	Taken, into account, the following sentende is added 'The total anthropogenic RF is 50% higher compared to AR4 (2005) due to primarily to reductions in estimated aerosol RF but also to continued growth in greenhouse gas RF. '

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8-115	8	3	9			which is much larger than the AR4 estimate! [Henning Rodhe, Sweden]	Taken into account, see 8-115
8-116	8	3	11	3	14	Is it really uncertain that forcing has increased since 1970, seems to contradict previous bullet where uncertanities were small [Piers Forster, UK]	Taken into account, text revised with a substantial new paragraph about the time evolution of RF.
8-117	8	3	13			Which results? [Henning Rodhe, Sweden]	Taken into account, see 8-117
8-118	8	3	16	3	17	I think you mean that "the net anthropogenic RF has increased by 0.7 Wm-2 +/- xx" [Olivier Boucher, France]	Accepted, text revised as suggested.
8-119	8	3	16	3	18	"The net RF from natural sources has been near-zero over the past three decades (since 1980). During this time, net anthropogenic forcing has been ~0.7 W m -2 ± XX". Do not forget to replace the XX value. [Rubén D Piacentini, Argentina]	Taken into account - the XX value has been replaced by a number.
8-120	8	3	16			Might be better to say athro forcing increased by X over this time? [Piers Forster, UK]	Taken into account, see 8-119
8-121	8	3	16			Quantify "near zero" [Henning Rodhe, Sweden]	Taken into account, a value has been included in a paranthesis
8-122	8	3	17	3	17	The uncertainty on anthropogenic forcing is still given as "±XX" [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Taken into account, see 8-120
8-123	8	3	17	3	17	Typo with missing data. [James Wanliss, USA]	Taken into account, see 8-120
8-124	8	3	21	3	21	An important paragraph. It should also be mentioned that while GTP is an improvement on GWP (as it may have the potential to include non-radiative forcings), it still does not capture the full impacts of some GHGs (eg: CO2 and O3) and hence may not allow comparison of different forcings agents in terms of their actual societal impact. Moreover, as mentioned on page 8-6 line 32, global metrics do not capture regional forcings, so again for the policymaker they only allow comparison of forcing agents from one particular policy perspective. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Rejected, this information is given in section 8.7.
8-125	8	3	21	3	21	The sentence «The choice of metric depends on the particular impact being investigated.» is rather vague, and gives the impression that one can choose the metric. Moreover, to my opinion, the impact is almost the metric. [Dirk Olivié, Norway]	Taken into account, text is rewritten with the following text included prior to the sentence: 'No single metric can accurately compare all consequences (i.e., responses in climate parameters over time) of different emissions, and ..'
8-126	8	3	21	3	24	Be careful here more widely used than what, and then only in research circles (CICERO/Reading?). GWP still does the heavy lifting at COP meetings. To be polcy neutral you could say two ones in common research use are GWP and GTP? [Piers Forster, UK]	Taken into account, text rewritten to 'Up to AR4, the most common metric has been the global warming potential (GWP), that builds upon the RF and integrates it out to a particular time horizon. There is now increasing focus on the Global Temperature change Potential (GTP), which is based on the change in global surface temperature at a chosen point in time. '
8-127	8	3	21	3	24	The dominant use of metrics is the use of GWP to weight emissions of different GHGs for consideration regarding mitigation. The chapter makes inroads assessing alternate approaches to this weighting, and applying weights to aerosols. Highlighting GTP as being widely used is inappropriate since the current use of GWP far exceeds that of GTP, and the socio-economic basis for choosing GTP over GWP is lacking. Given the importance of this topic, suggest that this bullet be replaced by three on 1) rationale for metric -- definition dependence on use, 2) applying such metrics to aerosols, and 3) inclusion of additional climate system mechanisms (e.g. effect of methane emissions on aerosols) in the evaluation of GWP and other metrics. [Haroon Kheshgi, United States of America]	Taken into account, see 8-127. The suggestion of changing this bullet into 3 is not taken into account.
8-128	8	3	21	3	24	There is no mention of the GWP, which may give the perception that you are favouring the GTP over the GWP (which may not be the intention) [Glen Peters, Norway]	Taken into account, see 8-127.

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8-129	8	3	21	3	24	This needs to say clearly that the 100-year GWP remains the metric most widely used in a variety of policy and decision-making contexts, but that the GTP concept (particularly in its time-dependent form) has gained some prominence as a possible alternative. As it currently stands, this para reads as if the GTP were the most widely used metric, which clearly it isn't (for better or worse). [Andy Reisinger, New Zealand]	Taken into account, see 8-127.
8-130	8	3	22	3	24	I would also introduce GWP (perhaps as a separate bullet). GWP is the index actually used in the Kyoto Protocol. [Katsumasa Tanaka, Switzerland]	Taken into account, see 8-127.
8-131	8	3	22	3	24	I am seeing a potential risk that might be brought about by overly emphasizing GWP and GTP and leaving other alternatives. In order to keep the options open and continue discussion, I think it is important to clarify in the Executive Summary that there are also other alternatives and that alternative metrics are still under discussion. For example, there are arguments against the idea of GTP from a climate policy point of view (e.g. Johansson, 2011, Climatic Change, 10.1007/s10584-011-0072-2). US climate policies are more dominated by the social cost of carbon. [Katsumasa Tanaka, Switzerland]	Taken into account, text modified with the following text 'One may imagine and define a large number of other metrics, down the driver-response-impact chain. No single metric can accurately compare all consequences (i.e., responses in climate parameters over time) of different emissions, and the choice of metric therefore depends strongly on the particular impact being investigated. '
8-132	8	3	24			It is now stated that GTP has become "much more widely used". Perhaps it should be added that it is more widely used in scientific papers, but less so in policy contexts. The reason is that it is very rarely used (to my knowledged) in public policy options, nor in life cycle assessments of different products, e.g., the CO 2 equivalent emissions of milk, or ethanol. [Christian Azar, SWEDEN]	Taken into account - text revised as follows 'Up to AR4, the most common metric has been the global warming potential (GWP) that builds upon the RF and integrates it out to a particular time horizon. There is now increasing focus on the Global Temperature change Potential (GTP), which is based on the change in global surface temperature at a chosen point in time. Both the GWP and the GTP use a time horizon, the choice of which is highly subjective. '
8-133	8	3	26	3	26	Net effect on what? surface temperature? [Guus Velders, Netherlands]	Taken into account, 'negative forcing' is included
8-134	8	3	26	3	28	I assume you mean for the GWP and GTP? [Glen Peters, Norway]	Taken into account, see 8-134.
8-135	8	3	26	3	28	Indicate the size of NOx effect on RF. [Robert Portmann, United States of America]	Rejected, not sufficiently important for ES, further details will be given in section 8.7
8-136	8	3	26	3	28	Briefly explain why NOx has a net cooling. This is confusing since it is a precursor of tropospheric ozone. Maybe something like: “NOx emitted from surface sources gives a net cooling effect due to interactions with methane and nitrate formation” [Helen Worden, USA]	Rejected, not sufficiently important for ES, further details will be given in section 8.7
8-137	8	3	26	3	39	these later bullets need some lead in about looking at forcing in different ways. It seems odd that the only source forcing dicussed is Nox - are CO2 and methane also source forcings, I guess so my not clear [Piers Forster, UK]	Taken into account, text is modified and 2 FOD bullets are merged together as a new paragraph.
8-138	8	3	30	3	34	This would be clearer if rewritten in terms of direct vs. indirect RF, as opposed to changes in concentration vs. emission. [Robert Portmann, United States of America]	Rejected, for mitigation purposes the perspective of forcing based on emissions is quite important and we want to highlight this.
8-139	8	3	30	3	34	Are the numbers for methane based on numbers in the chapter? They seem too large a difference to me. To get from 0.5 Wm2 to 0.9 Wm2 one would have to include the stratospheric water contribution (0.07) and ALL of the tropospheric ozone RF (0.34 W m2). But it is unreasonable to assume all of the tropospheric ozone changes are from methane. [Robert Portmann, United States of America]	Taken into account, this number is updated and made consistent with numbers in section 8.5
8-140	8	3	30	3	34	Very confusing [Henning Rodhe, Sweden]	Taken into account, text modified and 2 bulletts merged to make this clearer.
8-141	8	3	30	3	34	I am not sure why there is an 'in particular' statement here. Aerosols could have an even larger indirect effect. Why single out methane? [Susan Solomon, USA]	Taken into account - 'in particular' deleted

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8-142	8	3	31	3	34	Can one not formulate this as: «the strong emission increase in methane has been partially counteracted by a shorter life time»? [Dirk Olivié, Norway]	Rejected, no scientific evidence to support changes suggested by the reviewer
8-143	8	3	36	3	39	Consider to include a description of the differences in atmospheric lifetimes, and how it will influence climate change in the executive summary. [Øyvind Christophersen, Norway]	Rejected, not sufficiently useful to include in the ES.
8-144	8	3	36	3	39	What metric are you using here, seemed to vague to be useful. Does picture look different if different metric used? Does this include all possible aerosol cloud effects (AF?) [Piers Forster, UK]	Taken into account - text revised with inclusion of 'and the GTP metric'
8-145	8	3	36	3	39	This is forward looking perspective on sectors, but there is no mention of backward looking. E.g., next 25-100 years is mentioned, but what about mentioning what sectors are responsible for the currect temp (Fig 8.27 but for sectors)? [Glen Peters, Norway]	Rejected, no scientific publication available to support such an analysis
8-146	8	3	36			"activities", but you refer to "sectors" in the main text? [Glen Peters, Norway]	Taken into account - sectors used instead of activities
8-147	8	3	36			Clarification is needed for the conclusion "Livestock, household cooking and heating, on-road transportation, and agriculture are also large contributors to warming, especially over shorter time horizons." How short are the "shorter time horizons" and how large a contribution to the warming (a few mK?). [Timothy Wallington, USA]	Taken into account - text revised with inclusion of '(~20 years)' at the end of the paragraph and 'relatively' has been added before 'large contributors to warming'
8-148	8	3	37	3	37	"have" should be "has"; "contributions" should be "contribution" [James Butler, United States of America]	Accepted, text revised as suggested.
8-149	8	3	38	3	38	Based on the studies of Unger et al. (Cited in the report) household cooking is cited as an important contributer to AGW. This seems to be based on highly speculative forward modeling of rather poor data. They used a computer model at GISS to look at future at climate impacts if we continued emitting at today's rate. This is at very best a curious speculation, and not a helpful part of the science [James Wanliss, USA]	Rejected, results in chapter not based on single study, but on metric-based evaluation as for all other sectors. Emission data limited, but that is the case for many effects, and acknowledged in chapter discussions.
8-150	8	3	42			are 90% CIs given as well as 95% CIs? [Ruth Doherty, UK]	Rejected, only 90% CIs are given.
8-151	8	3	44	3	44	There should be a mention of the physiological forcing by CO2 in the Exec Summary. This is a separate issue from land use change so requires its own bullet point. When considering policy actions that require comparion of the effects of different GHGs, policymakers need to be aware that CO2 exerts additional effects over and above radiative forcing. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Rejected, not sufficient information to justify it being in the ES.
8-152	8	3				In my opinion this chapter is overly long, asserts accuracy and precision that cannot be justified, veers inappropriately off into response (it is a forcing chapter), presents much too much detail on geographical and sectoral issues that are rather new findings by a small number of groups (in some instances mainly involving chapter authors and not well established by the community). Moreover, the sectoral issues (e.g., transport) are pertinent perhaps to mitigation considerations but not to WG1. My concerns are spelled out in the comments. But I feel compelled to present this overall comment in the hope that the authors will carefully re-think what belongs in this chapter, what might be relegated to an appendix, what should be excised completely. [Stephen E Schwartz, USA]	Rejected - the overall response to this comment is rejected since we'll continue to to discuss response, provide geographical and sectoral information and have the chapter length in about the same length. However, we have considered this and some few other similar comments carefully and re-ordered the content of the chapter substantially. The geographical information is now based on a larger set of models. The sectoral information is more linked to RF values assessed in AR5. We have followed the suggestion to move some of the material to appendix and supplementary.
8-153	8	4	1	15	13	I would urge the authors to consider splitting this section into two discrete sections. The first deals with RF and AF as describing a state of the atmosphere or climate system; the second deals with short-cuts to compare the effect of emissions of specific substances on the state of the atmosphere or climate system. While related, they are rather discrete issues, and many problems that arise in the latter (related to the choice of metric, time horizon, links between economic and physical perspectives) do not arise in the former. There is a lot of confusion amongst non-experts around these issues, and many don't understand e.g. that a statement about CO2-eq concentration is scientifically robust and depends much less on subjective assumptions than a statement about CO2-eq emissions. Clearly separating sections 8.1.1 and 8.1.2 into two separate sections, and revising the introduction on page 4 to make this distinction clearer, would greatly help understanding by non-experts. [Andy Reisinger, New Zealand]	Accepted. Sections now separated, former 8.1.1 still 8.1.1, 8.1.2 now 8.7.1

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8-154	8	4	1	15	13	This section does seem to use any of the IPCC confidence/uncertainty language, even though it would really help clarify in many aspects what we know and how well we know it. [Andy Reisinger, New Zealand]	Noted. This section presents the background, so is generally not quantitative. We'll see if confidence language is appropriate in some cases though.
8-155	8	4	2	4	3	The current title is too long and needs to be shortened. [Katsumasa Tanaka, Switzerland]	Accepted. New title is shorter
8-156	8	4	2	4	3	What is the Greenhouse Gas Equivalent? [Katsumasa Tanaka, Switzerland]	Accepted. No longer in title.
8-157	8	4	2			Should you have an intro that puts chapter in context of other chapters and the report, and outlines your sections and why your covering them. (e.g. will you cover CMIP5 forcings) [Piers Forster, UK]	Rejected, no separate section but introduction part of section 8.1 is extended.
8-158	8	4	2			Some recognition that the RF concept also hits complications when applied to stratospheric ozone changes (see substantial publications of 2011) would be good here. [Olaf Morgenstern, New Zealand]	Accepted, but discussions of nuances associated with particular agents generally in sections on those agents, so in section 8.3.3 in this case.
8-159	8	4	2			What is a "greenhouse gas equivalent"? [Glen Peters, Norway]	Accepted. No longer mentioned here.
8-160	8	4	5	4	12	This section is a more appropriate introduction to the topic of 8.1.1 but not of 8.1.2. Suggest moving to 8.1.1 and shortening. [Haroon Kheshgi, United States of America]	Accepted. As 8.1.2 is now moved to a separate section (8.7) this introduction is now suitable here.
8-161	8	4	5	4	12	The word «factor» is rather vague. It is also hard to get a good interpretation of the second and third sentence in this paragraph. [Dirk Olivié, Norway]	Accepted. Factors changed to 'drivers' in first usage, though factors seems clear later. Following sentences revised slightly to clarify.
8-162	8	4	5			I don't think this is possible even "in principle", since we only have one Earth [Terje Berntsen, Norway]	Noted. Volcanic forcing is a case where the impact of a single driver can be well isolated in observations, so we believe that the principle that this can be done is valid. We agree, however, that typically this can't be done, as the text states.
8-163	8	4	6	4	6	"it's" should be "its" [Olivier Boucher, France]	Editorial. Done.
8-164	8	4	6	4	6	Replace "it's" with "its". [Robert Waterland, United States of America]	Editorial. Done.
8-165	8	4	6	8	9	No part of the earth is ever in energy equilibrium. The term "balanced" seems to be used to conceal this fact. There is overwhelming evidence from geology that energy entering and leaving the earth is never balanced, as there are many warming and cooling periods of differring requency and intensity. There is a fundamental difference between day and night which is never recognised and there is no rational method of averaging its state with daytime. Heat is removed from the surface during day by convection and the warmed air rises and emits radiation at various levels in the atmosphere, with half of it returned.Because of this, the temperature at the surface is not directly related to the raduative emission at the tropopause, which is much lower at night. All in all the concept of Radiative Forcing is unsatisfactory however it is defined [VINCENT GRAY, NEW ZEALAND]	Noted. Balance is not achieved at very short timescales, but for longer times as in the discussion here, the planet as a whole can indeed be in balance, though it is not always so. AR5 discusses the current energy imbalance at some length, and the concept of RF is in fact a measure of energy imbalance, so balance is not presumed.
8-166	8	4	6			"its" rather than "it's". [Timothy Wallington, USA]	Editorial. Done.
8-167	8	4	7	4	7	Replace "used" with "constrained". [Robert Waterland, United States of America]	Rejected. Climate models can be used to do these sorts of experiments, so this seems a proper explanation.
8-168	8	4	14	7	20	This section is very important and will hopefully influence the community for the years to come. I think the definition and discussion need to be more accurate. I will try to detail my specific comments below but would welcome a more profound rewrite if the LA are willing to do that. [Olivier Boucher, France]	Noted.
8-169	8	4	16	4	18	I don't know if «external» should be so much emphasized. What about «imposed» forcing? [Dirk Olivié, Norway]	Accepted. Changed 'external' to 'imposed'.
8-170	8	4	16	4	18	About the definition of Radiative Forcing: "RF is a measure of the net change in the energy balance of the	Accepted. Added that this is for a particular time

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						Earth system in response to some external perturbation. It is expressed in watts per square meter and quantifies the energy imbalance that occurs when the external change takes place". Radiative forcing is an irradiance (the difference of incoming and outgoing irradiances) with unit of Wm-2. Consequently, the following words must be added to the word "energy" (eventually between parentheses): per unit area and time interval. [Rubén D Piacentini, Argentina]	average. As it already says the unit is per m squared, not necessary to say per unit area again.
8-171	8	4	16	4	23	Should add some citations and give some indication as to why its used. Also some would argue that CO2 isn't a polllutant and land-use certainly isn't one. Introducing CAPs seems dangerous and unnecessary to me [Piers Forster, UK]	Accepted. As this acronym was only used twice, deleted.
8-172	8	4	16		23	This is a good definition of forcing. However I think "external" needs a bit of elaboration. I would suggest "external to the climate system". Thus "in response to a perturbation that is external to the climate system". The criterion of what is "external" is somewhat arbitrary. So it might be valuable to elaborate. The Sun is intrinsic to the climate system, yet it is convenient to consider a change in solar luminosity as a change that is external to the climate system. Similarly CO2, for which at long time scales weathering of rocks is part of the climate system, but on short time scales emissions of CO2 from fossil fuel combustion and land clearing are considered external. [Stephen E Schwartz, USA]	Accepted. Changed 'external' to 'imposed'.
8-173	8	4	16			Wording. The word "response" indicates that the RF concept allows for response in the climate system to the radiation perturbation. [Terje Berntsen, Norway]	Accepted. The use of the word 'response' could be confusing, so changed to 'due to'.
8-174	8	4	18			Add "calculated" before RF. [Terje Berntsen, Norway]	Accepted. Done.
8-175	8	4	21			CAPs - what is the definition? Calling CO2 a pollutant is controversial. Was it a pollutant when paleo levels were higher that present levels? How about 'climate altering atmospheric components'? [Stephen Gaalema, USA]	Accepted. As this acronym was only used twice, deleted.
8-176	8	4	21			Remove acronym "CAP" – only used once in rest of chapter. Try to reduce the number of acronyms as much as possible [Timothy Wallington, USA]	Accepted. As this acronym was only used twice, deleted.
8-177	8	4	23	4	23	Is it common to use forcing at a single time in any research field? [Katsumasa Tanaka, Switzerland]	Noted. It is common, for example the AR4 SPM shows a bar chart of RF at 2005 (this is relative to 1750, but all forcing is relative to a non-perturbed state by definition). Sentence clarified to highlight 'time-dependent' vs 'single time' is what's meant.
8-178	8	4	25	6	6	This section does a really important job for the whole report, but it still needs a lot of work. These basic concepts anre there but the definitions still are not that tight, the text is confused in places and citations are lacking. I think this needs completely rewriting. I would sugget you start with AR4 defnitions. Describe all work since on semi direct effect adjusted forcings etc. and talk about the different definitons used and how compatable they are. Talk about the benefits and limitations of each one. And then explicitly define the terms you will use. [Piers Forster, UK]	Noted. We will be extensively rewriting this section following the suggestions of many reviewers. As we have different definitions, we do not feel the material naturally flows as suggested here however. For example, we no longer use the semi-direct effect. We will endeavor to explicitly define terms, discuss benefits and limitations,etc.
8-179	8	4	25	6	8	There is no mention of the slope method (extrapolating to time zero in a coupled model run with sudden change of forcing agent) for computing RF in this section. I think this should be mentioned even if not used (much) in the chapter as it gives another perspective on isolating rapid adjustments. [Robert Portmann, United States of America]	Accepted. We talk about this method as well as fixed-SST.
8-180	8	4	25	6	8	The alternative method of Shine's of fixing both land and ocean surface temperatures for computing AF should be mentioned as well as Hanson's variation of his method. [Robert Portmann, United States of America]	Accepted. The method is introduced genearlly to include various portions of surface response (land and ocean or just ocean fit).
8-181	8	4	27	4	45	First there is an implicit assumption in many sentences that the RF is sustained infinitely ("steady-state climate response" or "eventual"), it should be said that RF can be transient, but the delta T = lambda F relationship only holds for sustained RF and equilibrium climate response. [Olivier Boucher, France]	Accepted. Revised to point this out.

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8-182	8	4	27	4	45	Please clarify that radiative forcing is calculated as the average of instantaneous radiative forcings over a period of time, and this is referred to as direct forcing, No one reports the instantaneous radiative forcing, except as an average, Further, the climate sensitivity is the ratio of the temperature change to the average radiative forcing. However, please clarify that some people report the climate sensitivity as the the temprature change per unit total forcing (e.g., adjusted forcing) and others, the temperature change per unit direct forcing. [Mark Z. Jacobson, U.S.A.]	Accepted. Stated that this is averaged over a period of time in 8.1.1 now.
8-183	8	4	27			What are these alternatives - only instantaeoud is mentioned? [Piers Forster, UK]	Noted. The sentence refers to the whole section.
8-184	8	4	29	4	29	It may useful to define the concept of "external agent" in this context. [Nicolas Bellouin, United Kingdom]	Accepted, but in response to other comments, we now refer to the forcers as imposed changes rather than external agents.
8-185	8	4	30			You mention "usually" and refer to two options "… or …". What is the "usual" choice, or are they both used equally, are they synonyms, etc? The phrasing makes it ambiguous to a non-RF expert. [Glen Peters, Norway]	Accepted. Clarified that tropopause better.
8-186	8	4	32	4	54	Please clarify that instantaneous radiative forcings are (or should be) calculated from one model simulation, but with two radiation calls each time step during the simulation.(to avoide any climate responses). On the other hand, adjusted radiative forcings are calculated from two simulations, to allow climate feedbacks to occur. [Mark Z. Jacobson, U.S.A.]	Accepted. Will add methodology to Figure 8.1.
8-187	8	4	32		39	The sentence: " The instantaneous RF provides a simple, quantitative basis for judging the effectiveness of different external forcing agents in producing a given climate response." Better " The instantaneous RF provides a simple, quantitative basis for quantifying the influence of different external forcing agents on the radiation flux of the planet that would result in climate response." I am surprised at the statement: "The assumed relation between the instantaneous RF forcing (F) and the equilibrium global mean surface temperature response (DeltaT) is DeltaT = lambda F where lambda is the climate sensitivity" First "RF forcing" is redundant; correct it to : The assumed relation between the instantaneous RF (F) and the equilibrium global mean surface temperature response (DeltaT) is DeltaT = lambda F where lambda is the climate sensitivity. But more substantively, the relation is between the equilibrium response to a _sustained_ forcing, not an instantaneous forcing. Finally I would recommend that it be explicitly stated, perhaps here, that the notion of an "equilibrium" response really denotes a "steady state" response, commonly denoted "equilibrium". (It is not a thermodynamic equilibrium). Last sentence: the feedbacks are not inherent in the _quantification_ of lambda. They are inherent in lambda. [Stephen E Schwartz, USA]	Accepted. As in reponse to prior comments, we now point out that this is response to sustained forcing.
8-188	8	4	33			The term "a given climate response" indicates that the RF is suited as a metric also as an indication of non-temperature responses. Consider change to "temperature response". [Terje Berntsen, Norway]	Accepted. Change to temperature.
8-189	8	4	35	4	37	This definition of "climate sensitivity" is different to that in chapter 9 (9-64 L10). The lambda symbol is associated with climate sensitivity parameter later in the chapter (8-12 L4) but as the climate feedback parameter in chapter 13 (13-26 L30-31) [Gareth S Jones, UK]	Accepted. We now use lambda only as climate sensitivity parameter.
8-190	8	4	35	4	37	The reference to chapter 7 is probably not correct here. [Twan Van Noije, Netherlands]	Accepted. Reference deleted.
8-191	8	4	35			Since "instantaneous RF" is used a few times, perhaps you could put "RF superscript instantaneous" [Glen Peters, Norway]	Taken into account. We no longer use 'instantaneous' so often.
8-192	8	4	36			Cross check terminology of AR5. Climate sensitivity usually refers to the warming following a doubling of CO2.	Accepted. We now use lambda only as climate

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						Lambda is the climate sensitivity parameter. [Terje Berntsen, Norway]	sensitivity parameter.
8-193	8	4	36			Why is F the instantaneous forcing in the equation? [Piers Forster, UK]	Taken into account. This has been changed in response to other reviewer comments.
8-194	8	4	39			Feedbacks are not that well defined at all - what exactly do you mean - how do you tie with other chapters [Piers Forster, UK]	Taken into account. We no longer refer to feedbacks at this point.
8-195	8	4	41	4	41	"Notional" is a strange choice of word here. [Robert Portmann, United States of America]	Editorial. Changed as in reply to next comment.
8-196	8	4	41	4	41	Replace "The notional idea of RF is that" with "Implicit in the concept of RF is the proposition that". [Robert Waterland, United States of America]	Accepted.
8-197	8	4	41	4	45	First sentence is confusing here. There isn't a fraction of the irradiance that affects delta T and a fraction of the irradiance that affects other things. You'd rather talk about "other parts of the system (feedbacks) that affect net irradiances". [Olivier Boucher, France]	Accepted. Revised text.
8-198	8	4	41	4	45	I find this paragraph confusing. It is couched in terms of climate response: are feedbacks not part of the climate responses? If you divide up climate response in this way, what does "steady state global mean climate response" actually mean? [Robert Waterland, United States of America]	Accepted. Revised text.
8-199	8	4	47	4	47	Add "also known as stratospherically-adjusted RF" after "RF" [Larry Horowitz, USA]	Accepted. Done.
8-200	8	4	48	4	48	"Comparatively" to what other uncertainty range? [Nicolas Bellouin, United Kingdom]	This comment does not appear to be correctly identified by page and line.
8-201	8	4	50	4	52	I am not sure there is an exception for the aerosol first indirect effect. Water vapour and cloud cover are also held fixed at the unperturbed values when calculating the RF associated with the first aerosol indirect effect. Only particle size can change. [Olivier Boucher, France]	Noted. However, the adjustment of cloud particle size is a response that is not part of the instantaneous energy flux changes associated with imposed aerosol, so by definition is not part of our instanteous RF as defined previously.
8-202	8	4	52	4	54	I do not like this sentence. RF is not a "part of the instantaneous RF", it is an instantaneous RF with stratospheric adjustment. I'd rather stick to the strict definitions. [Olivier Boucher, France]	Accepted. Removed the 'part of the inst RF' language.
8-203	8	4		15		Two sorts of metrics are here, RF as a diagnostic to understand models an response and RF as a way of evlauting emissions. This isn't really clear and there is some confusion as to the use of the term "metric" [Piers Forster, UK]	Taken into account. The new chapter 8 structure has these clearly separated.
8-204	8	5	1	5	17	I found this paragraph unnecessarily complicated. No need to discuss efficacy so quickly. The central idea is that only the flux changes after fast adjustments have occurred affect the long-term global mean temperature change. The AF just allows for more adjustments than the RF, which only allows for stratospheric temperature changes. Then discuss that some effects are much easier to compute in the AF framework than the RF (e.g., aerosol indirect effects). Finally discuss efficacy and mention that ideally AF allows for efficacies closer to one since more rapid responses are included (although this is likely still not true when considered across models). Now note that efficacies different than one can still be expected due to thermodynamic effects such as those mentioned for BC. The statement starting on line 4 makes the old RF definition sound much worse than it really is (in reality the RF and AF are similar and even aerosol indirect effects were estimated with the old definition, it is just that a model was needed to estimate the cloud changes). [Robert Portmann, United States of America]	Accepted. Revised discussion to make these points more clearly.
8-205	8	5	5	5	5	This phrase is vague: "especially for some of the aerosol cloud effects". A better phrase might be "such as absorbing aerosol", and this also links to later sentences in the paragraph. [Leon Rotstayn, Australia]	Accepted. Revised.
8-206	8	5	5	5	8	Effiacy discussion seemed rather short and a little ad hoc. It is not at all straightfoward how it relates to forcing. This needs much more careful discussion. Or maybe you don't need to bring it in? [Piers Forster, UK]	Accepted. We now have minimal material on efficacy as we no longer use that concept with AF.
8-207	8	5	5	5	10	Efficacy could maybe be explained simpler by saying that lambda depends on forcing mechanism. [Terje Berntsen, Norway]	Noted. We are revising this discussion of efficacy, and will consider if this is a useful explanation.

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8-208	8	5	5	8	6	I think it's good that this chapter is trying to deal with the RF and AF issue. However, I think more work will be needed to better illustrate what we know and don't know about this important issue. I think more of a review of Gregory's work, and Forster's, and showing how fluxes change with time since the RF is imposed, may help. I also think it would be helpful to make clear that the goal of RF or AF is a relative one - the key issue is always one of scaling to a reference (generally CO2) so the key thing to determine is to what extent a given adjustment is unique to the species considered versus occurring for other agents as well. [Susan Solomon, USA]	Noted. We discuss the various methods, including the 'slope' method of Gregory as we introduce a general AF before using fixed-SST AF here. We do not agree that the given goal of RF or AF is only to compare with CO2. Absolute values also provide an indication of the total impact on climate, e.g. using the climate sensitivity parameter, and both are used in this chapter and both are useful.
8-209	8	5	6	5	6	Hansen et al. (2005, http://www.agu.org/pubs/crossref/2005/2005JD005776.shtml) should be cited when the concept of "efficacy" is introduced. [Katsumasa Tanaka, Switzerland]	Accepted. Added.
8-210	8	5	6			Typical ranges for efficacy would be useful here, clarify whether "substantially differ from 1" means typically a smaller or larger value than 1. [Ruth Doherty, UK]	Rejected. We are not using efficacy much in this chapter, so do not want to go into more detail on this. We appreciate the point the reviewer was trying to make, however, and will note later when discussing forcing those cases where they are not good indicators and indicate if the real response is larger or smaller.
8-211	8	5	7			Move the reference to the end of the sentence for better readability [Pieter Aucamp, South Africa]	Accepted. Done.
8-212	8	5	9	5	10	Is this intended to imply that most deviations from efficacy=1 are related to fast feedbacks? Isn't there also an influence from the latitudinal distribution of forcing (and ocean heat uptake)? [Larry Horowitz, USA]	Accepted. Good point, and we'll add a sentence about this.
8-213	8	5	9	5	10	What are the "adjustments"? Are we talking about changes due to the feedbacks? [Gareth S Jones, UK]	Accepted. Clarified this section.
8-214	8	5	10	5	16	I find the first sentence and the rest of the paragraph here unclear (and not scientifically well phrased). All of the aerosol radiative perturbation goes to change the internal thermodynamics of the climate system. I think the point is that different radiative perturbations cause different rapid adjustments. In some cases, these rapid adjustments have been isolated (eg CO2 rapid adjustment on clouds, semi-direct effect, etc ...) because they were looking somewhat specific to a particular forcing. And most of the time they have gone unnoticed, but it does not mean they are not there. I think some cross-reference to Chapter 7 would be useful. [Olivier Boucher, France]	Accepted. Clarified this section.
8-215	8	5	11	8	11	"direct radiative forcing" is calculated in the exact same way for aerosols and gases so should be defined consistently for both, as described above (one simulation, two radiation calls). [Mark Z. Jacobson, U.S.A.]	Noted. Definitions of these two the same.
8-216	8	5	12	5	12	As “thermodynamics” is a science, maybe it is better to say «state», or “energy state», or «energy householding». [Dirk Olivié, Norway]	Accepted. Do not say thermodynamics.
8-217	8	5	13	5	14	About the sentence: "This has been called the semi-direct effect (Hansen et al., 2005) and Section 7.3.5.2", I consider that "Section 7.3.5.2" should be included in the parentheses, ie: "This has been called the semi-direct effect (Hansen et al., 2005 and Section 7.3.5.2)". [Rubén D Piacentini, Argentina]	Accepted. If this is maintained, will write as suggested.
8-218	8	5	13	5	14	… (Hanson et al., 2005) and Section 7.3.5.2. Parentheses should go to end of sentence. [Robert Portmann, United States of America]	Accepted. If this is maintained, will write as suggested.
8-219	8	5	14	5	16	The sentence explaining the CO2 impact on the thermodynamic was unclear to me [Terje Berntsen, Norway]	Accepted. Will clarify this section during rewriting.
8-220	8	5	14	8	14	The "semi-direct" effect is only one of many effect of aerosols that affects internal thermodynamics, so changes in "internal thermodyanics or the temperature distribution…" should not be referred to as "the semi-direct effect." The semi-direct effect is defined as the "change in cloudiness due to the decrease in near-cloud relative humidity (RH) and increase in atmospheric stability caused by absorbing aerosol particles below, within, or above a cloud [Hansen, J., M. Sato, and R. Ruedy (1997) Radiative forcing and climate response, J. Geophys. Res., 102, 6831-6864.7; Ackerman, A. S., O.B. Toon, D.E. Stevens, A.J. Heymsfield, V. Ramanathan, and E.J. Welton (2000) Reduction of tropical cloudiness by soot, Science, 288, 1042-1047;	Accepted. Clarified this section during rewriting.

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						Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, J. Geophys. Res., 107, (D19), 4410, doi:10.1029/ 2001JD001376, 2002). This definition is given in Section 3.7 of Jacobson (2002). Twelve effects of aerosol particles to climate are listed in that paper. In addition to these effects, other effects of absorbing aerosols are cloud absorption effects I and II (Jacobson, M.Z., Investigating cloud absorption effects: Global absorption properties of black carbon, tar balls, and soil dust in clouds and aerosols, J. Geophys. Res., doi:10.1029/2011JD017218, in press, 2012, http://www.agu.org/pubs/crossref/pip/2011JD017218.shtml) and the BC-snow effect. [Mark Z. Jacobson, U.S.A.]	
8-221	8	5	15			"Direct" forcing is explained as the forcing that is direct. Not a very useful explanation. [Terje Berntsen, Norway]	Accepted. Clarified.
8-222	8	5	16	5	17	"These are termed fast feedbacks by Gregory et al. (2004)…" The point that they were previously called fast feedbacks is repeated in the next paragraph, and this might be a good place to introduce the idea that we are now calling them "rapid response", to distinguish them from feedbacks that can be expressed as a function of surface temperature changes. In general, I feel that the explanation of "rapid response" needs more work, to make it clearer, and to avoid repetition. Some of the discussion in the introduction of Lohmann et al. (2010) might be helpful here. [Leon Rotstayn, Australia]	Accepted. Clarified this section during rewriting.
8-223	8	5	16			fast feedbacks generally refers to standard water vapour, cloud etc. I thought we were going to use rapid adjustment? Timescales can be seasonal for ice effects? [Piers Forster, UK]	Accepted. Clarified this section during rewriting.
8-224	8	5	17	5	17	I suggest giving a quantitative estimate of those time scales. [Nicolas Bellouin, United Kingdom]	Accepted. Done.
8-225	8	5	19	5	21	Other things also alter clouds. So it is not clear to me that a sharp distinction can be made regarding allowing adjustments for clouds for aerosols and not for e.g. CO2. While the effects of the latter are smaller, they are over every square meter of the globe. It has long been known that CO2 changes affect cloud heights and cloud distributions - so it's important to discuss timescales and what is unique to e.g. aerosols versus general. [Susan Solomon, USA]	Accepted. Clarified this section during rewriting.
8-226	8	5	19	5	27	This paragraph starts with a discussion about aerosols, and ends with a defintion of "rapid responses". It is not obvious that the "fast feedbacks" in Gregory et al (2004) (cf. Line 16) is included in the definition of rapid response. [Terje Berntsen, Norway]	Accepted. Clarified this section during rewriting.
8-227	8	5	20	5	20	Replace "Chapter" with "Section". [Olivier Boucher, France]	Accepted. Done.
8-228	8	5	23	5	23	delete "time" [James Butler, United States of America]	Accepted. Done.
8-229	8	5	24	5	27	I would encourage the authors to explain the issues behind the difference between fast-feedbacks and rapid-responses a little more carefully here. There are different views on what the concept of radiative forcing is useful for, so it is important to be as clear as possible about how the authors here want to use it. [Gareth S Jones, UK]	Accepted. Will clarify this section during rewriting.
8-230	8	5	25	5	25	Chapter 7 would suggest saying "adjustment" rather than "response", but this is somthing to be discussed in Marrakech. [Olivier Boucher, France]	Noted. Will consider in rewriting.
8-231	8	5	26			I think it is worth mentioning that fast/slow here has a very different timescale than fast/slow in this chapter. [Terje Berntsen, Norway]	Accepted. Done.
8-232	8	5	29	5	39	Chapter 8, Page 5, Lines 29-39:Please note that the separation of rapid and long term responses was also discussed by Lambert and Faull, (2007) and Andrews et al., (2009): Lambert, F. H. and N. E. Faull, Tropospheric adjustment: the response of two general circulation models to a change in insolation, Geophys. Res. Lett., Vol. 34, No. 3, L03802, 2007. Andrews, Timothy, Piers M. Forster, Jonathan M. Gregory, 2009: A Surface Energy Perspective on Climate Change. J. Climate, 22, 2557–2570. [Francis Hugo Lambert, United Kingdom of Great Britain & Northern	Rejected. This is an assessment, not a review. We do not cite all prior publications on a particular topic. The cited Andrews et al (2010) paper follows the 2 earlier papers given here and seems to us the most appropriate.

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						Ireland]	
8-233	8	5	29	5	39	This paragraph somewhat gives the impression that the idea wasn't considered prior to 2004. Hansen et al. (2002) considered the merits of the "fixed-SST" calculation of forcing (and in their Fig. 10, showed a cartoon similar to the one in this chapter). Also, Rotstayn and Penner (2001) went into some detail to show that it was a viable method for calculation of the second indirect effect on warm clouds, in the sense that it could be used as a predictor of the global-mean surface temperature response. The motivation for the Rotstayn and Penner paper was that Joyce, as CLA of the aerosol chapter in the TAR, was frustrated that the folks from the radiative forcing chapter didn't want to consider these cloud responses as part of the forcing. It has taken a decade, and help from various other scientists, but it seems that we have finally succeeded. References: Hansen, J., et al., Climate forcings in Goddard Institute for Space Studies SI2000 simulations, J. Geophys. Res., 107(D18), 4347, doi:10.1029/2001JD001143, 2002. Rotstayn, L. D., and J. E. Penner (2001), Indirect aerosol forcing, quasi forcing and climate response, J. Climate, 14, 2960– 2975. [Leon Rotstayn, Australia]	Accepted. Incorporated into revisions.
8-234	8	5	36	5	36	Consider replacing "eventual" with "ultimate". Again here there is an implicit assumption that you are talking about a sustained (rather than time-varying) RF. [Olivier Boucher, France]	Accepted. Done.
8-235	8	5	41	5	50	There are 3 different ways AF could be introduced: 1) define AF from the Hansen method (this is what is being done here), 2) define AF from the Gregory method, or 3) define AF in a slightly more general method, and say Hansen and/or Gregory methods can be used to diagnose it, and then discuss advantages and disadvantages of each method (eg Gregory has the advantage of having global-mean delta T unchanged, when Hansen does not, unless you make an ad-hoc correction for the land surface temperature change; Gregory has the disadvantage of not working well for small forcings because of noise unless an ensemble is done). I have a preference for option 3). If option 1) is used, then option 2) should be mentioned as well, and appropriate references should be provided. [Olivier Boucher, France]	Noted. This is discussed in later paragraphs.
8-236	8	5	41	6	3	This paragraph is pretty good, but it isn't clear if AF is only based on the Hansen method, or whether the Gregory (regression) method is also accepted. If so, then one difference (among others) is that the Hansen method allows land-surface temperature to adjust, whereas in principle the Gregory method imposes fixed global-mean surface temperature (e.g., line 44: "atmospheric and land temperatures"). Please also see next comment. [Leon Rotstayn, Australia]	Accepted. Clarified this section during rewriting.
8-237	8	5	41	6	6	AF - allowing for "land albedo to adjust". Does this include vegetation adjustment (longer growing season for example)? More examples would help. [Stephen Gaalema, USA]	Accepted. Clarified this section during rewriting, including new discussion on modeling realmn included..
8-238	8	5	48	6	3	Among the three places offering the definition or description of the "AF" concept, this 8.1.1 has the best writing and clearest description. My comment goes to the (3) here for stating that the AF is "readily calculated… with a comparatively small uncertainty range". This seems conflicting with several examples provided later in 8.1.1.2 and beyond, and also with ones in Chapter 7, and with the fact that the AF concept introduces arbitrary adjusting time and involves many model-dependent skills ot parameterizations that would lead to higher uncertainty range intra- or inter-model wise. An additional comment on the AF is that by fixing SST and sea ice property the adjustment is actually biased toward land, introducing arbitrary ocean-land energy and vapor flux perhaps to the worst. It would be good to describe these in 8.1.1.2. [Chien Wang, United States of America]	Accepted. Will clarify this section during rewriting.
8-239	8	5	48			On point 3). Is it really true that these effects can be calculated with a "comparatively small uncertainty range"? [Terje Berntsen, Norway]	Taken into account. Small compared to other methods, and small for a single model. Will clarify this.
8-240	8	5	49		50	I'm not sure about the statement "the AF would be identical if calculated at the tropopause instead of the TOA". Consider, e.g., the introduction of a factor whose only radiative effect is to absorb solar radiation in the stratosphere, positioned over a non-reflecting surface. At TOA the [instRF, RF, AF] would be [zero, negative due to enhanced OLR, same as RF]. At TPS it would be [negative, less negative than inst due downward LW from warmer stratosphere, less negative than RF due less upward LW from cooler troposphere]. AF is not the same at the two levels (?) [Joanna Haigh, UK]	Rejected. While we agree there are differences in instRF, the adjustment by definition has removed any flux imbalances at all levels between the TPS and TOA, so these match (consistent with the reviewer noting that they'd both be negative in the example given).
8-241	8	5	50	5	53	Not sure why you single out vegetation changes for the aerosol indirect effect. [Olivier Boucher, France]	Noted. It says this mention of vegetation effects is an example, but we will try to ensure that's clear in

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							revisions.
8-242	8	5	53	5	57	I don't think it is correct to say that only a part of the RF contributes to the steady-state climate response. Which part is that? [Olivier Boucher, France]	Noted. Will revise this section. The text refers to instantaneous RF, not RF, and the part that does not contribute so directly is the difference between these two, hence the preference for RF over instantaneous forcing.
8-243	8	5	53	6	4	The discussion on timescales is useful, but it is likely that there is a spectrum of timescales involved. For some RF mechanisms there may be a bimodal distribution of the timescales, and in this case, separating rapid adjustment from the rest might be feasible, but for other RF mechanisms there could be a continuum of timescale (I suspect it is the case eg for the BC in snow RF). But we don't really know, and i think the text should somehow reflect on this issue. [Olivier Boucher, France]	Accepted. Clarified this section during rewriting and point out that there is a spectrum of timescales.
8-244	8	5	54	5	54	Make sure you define all terms in Figure 1 somewhere. For example, dT0 and dTs could be defined in words. dTs could also just be placed in parenthesis after the description of figure 8.1.e on page 8-7, line 16. [John Daniel, USA]	Accepted. Will clarify this caption during rewriting.
8-245	8	6	1	6	13	Given that AF and RF are virtually identical for increased CO2, how can CO2 rapid responses be considered "main adjustments"? [Twan Van Noije, Netherlands]	Accepted. Deleted word 'main'.
8-246	8	6	2	6	13	The CO2 rapid response is mentioned several times indicating that it is important, but then on line 13 it refered to Hansen et al. (2005) that it is very minor. It would be good to have the authors' assessment of its importance. [Terje Berntsen, Norway]	Accepted. Will clarify in revision that it is important in some models, but best estimate small with large uncertainty range.
8-247	8	6	2			It's now called rapid responses and this repeats some of the ealier paragraph. There aremore papers than those chosen here to discuss. [Piers Forster, UK]	Noted. Will use consistent language.
8-248	8	6	5	6	6	Need to justify mixing AF and RF up better than this. E.g. if you combine AF from aerosol with RF from LLGHGs, won't you be missing things? [Piers Forster, UK]	Noted. Will discuss in revisions the limitations of various choices.
8-249	8	6	8	6	8	RF does not have only limitations over AF. The definition of RF also gives a key advantage: holding the atmospheric state constant removes not only feedbacks, but also natural variability. The RF can then be used to quantify changes in radiative fluxes that are much smaller than year-to-year changes due to natural variability. A small AF is lost among the natural variability noise, but a small RF remains quantifiable. [Nicolas Bellouin, United Kingdom]	Accepted. Good point,I added in revisions.
8-250	8	6	10	6	10	Here, the chapter launches into a discussion of the fixed-SST method and the regression method, but neither has been explained. A separate paragraph is needed, to clearly explain the two methods. [Leon Rotstayn, Australia]	Accepted. As in several comments by reviewers on this subject, we will briefly discuss the various methods here and in defining Figure 8.1
8-251	8	6	10	6	11	This is arguable. Hadley Centre models have previously calculated radiative forcings (which you call RF I think) by using double radiation calls (Tett et al. JGR 2002). This has a significant computational impact, and is technically difficult for some factors especially in models with interactive chemistry. A model simulation with fixed SSTs is not as computationally complex in comparison. Perhaps somewhere it should be explained how the different types of forcings are actually calculated (Fig 8.1 does not tell us how the experiments are done). [Gareth S Jones, UK]	Accepted. Adding this to Figure 8.1
8-252	8	6	10	6	23	which varient of AF was used here, does it matter? [Piers Forster, UK]	Noted. We give the type of calcualation for most of the studies in the lines indicated in this comment. No evidence that it matters from limited available studies.
8-253	8	6	11	6	11	"However, in many cases the AF and RF are nearly equal" has this really been tested - only in few gcms for a few forcings? [Piers Forster, UK]	Accepted. Will point out which data is available on this.
8-254	8	6	14	6	18	I think you're referring here to the Gregory method as an alternative to estimate AF and rapid adjustment, but this has not been introduced before. [Olivier Boucher, France]	Accepted, now introduced previously first.
8-255	8	6	17	6	18	It would be helpful to explain the differences in regression analyses and fixed-SST expts [Gareth S Jones, UK]	Accepted, now introduced previously first.

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8-256	8	6	18	6	20	This sentence states that Lohmann found no substantial difference between RF and AF for "aerosol indirect effects on cloud lifetimes". But at the end of the paragraph it states "aerosol indirect effects on clouds for which RF due to change in cloud lifetime is not defined". These statements contradict each other. As I state above, people did compute aerosol indirect effects using the old RF definition (the indirect effects were estimated with a model and used in RT code to compute the RF). There is no doubt that the AF is an advancement but there is no need to overstate things. (See also comment 46 below that refers to page 36) [Robert Portmann, United States of America]	Accepted. See two comments below, this was a mistake and we meand cloud albedo, not lifetime.
8-257	8	6	20	6	20	I'm confused because RF cannot be defined for the cloud lifetime effect, so what do you mean? [Olivier Boucher, France]	Accepted. See comment below, this was a mistake and we meand cloud albedo, not lifetime.
8-258	8	6	20	6	20	Lohmann et al. 2010 evaluated the cloud albedo effect, not the cloud lifetime effect [Ulrike Lohmann, Switzerland]	Accepted. Will revise.
8-259	8	6	24	6	25	Can you summarize the error magnitude? For example, what is the approximate error on the 300% number. If no error is given, some people will just use this value. [John Daniel, USA]	Noted. Will see if other values are available, but likely not enough data to characterize range.
8-260	8	6	26	6	27	I'd suggest "computationally more demanding", rather than "more demanding". The fixed-SST calculations are conceptually simpler than, e.g., the stratospheric adjustment, though they do need quite a few years of simulation. [Leon Rotstayn, Australia]	Accepted. Will do.
8-261	8	6	26	6	29	or the other way around. RF is a good estimate of AF, which has the advantage of being defined for a larger set of RF mechanisms. Not sure what the best way of putting this. [Olivier Boucher, France]	Noted.
8-262	8	6	27	6	27	AF calculation is more computationally-demanding but easier to do than a double radiation call with stratospheric adjustment included. [Olivier Boucher, France]	Accepted. Will describe this.
8-263	8	6	28	6	28	"exceptions of the BC direct and snow albedo forcings…" Please include cloud absorption effects I and II in this list (Jacobson, M.Z., Investigating cloud absorption effects: Global absorption properties of black carbon, tar balls, and soil dust in clouds and aerosols, J. Geophys. Res., doi:10.1029/2011JD017218, in press, 2012, http://www.agu.org/pubs/crossref/pip/2011JD017218.shtml; Jacobson, M.Z., Effects of absorption by soot inclusions within clouds and precipitation on global climate, J. Phys. Chem., 110, 6860-6873, 2006). [Mark Z. Jacobson, U.S.A.]	Accepted. Will add.
8-264	8	6	33			Define OC (first use in chapter) [Pieter Aucamp, South Africa]	Accepted. Done.
8-265	8	6	36	6	36	The list can be extended to include Kang, S., L. M. Polvani, J. C. Fyfe and M. Sigmond, Impact of polar ozone depletion on subtropical precipitation, Science, 332, 951-954, 2011, and a few other papers that deal with the climate impacts of Antarctic ozone depletion. [Olaf Morgenstern, New Zealand]	Rejected. While stratospheric ozone loss does indeed cause regional circulation responses, that's not the topic here, and that subject is covered elsewhere.
8-266	8	6	37	6	37	replace "a useful metric" by "useful metrics" as you've just defined two. [Olivier Boucher, France]	Accepted. Done.
8-267	8	6	42	6	51	SLCF are defined as species with lifetimes less than one year. Except for inter-hemispheric mixing this is not "short compared to atmospheric mixing times" and thus I think it is not correct to state that teh distribution of these species will be (in general) highly inhomogeneous. [Terje Berntsen, Norway]	Noted. These terms are being changed in the SOD, see new box in ch 8.
8-268	8	6	42	6	51	I believe that the mitigation community would use a different definition of short-lived vs. long-lived, taking those compounds that contribute to a peak warming mainly through their emission rates at the time of the peak as short-lived, while long-lived are those which contribute through their accumulated emissions. With thsi approach short-lived components would include e.g. methane. The WG I should make sure that its definition is consistent with teh one used by WG III. [Terje Berntsen, Norway]	Noted. These terms are being changed in the SOD, see new box in ch 8.
8-269	8	6	48			The citation to the relevant paper of Boucher 09 is missing (there is another, different Boucher 09 paper in the ref list). The pertinent paper appears to be Implications of delayed actions in addressing carbon dioxide emission reduction in the context of geo-engineering O. Boucher · J. A. Lowe · C. D. Jones Climatic Change Volume 92, Numbers 3-4, 261-273, DOI: 10.1007/s10584-008-9489-7 [Stephen E Schwartz, USA]	Noted. Will check to ensure correct citation.

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8-270	8	6	50	6	50	«methane is short-lived compared with temperature response» A large part of the global mean temperature response to a forcing (estimated as 60% in Boucher and Reddy (2008)), has a response time of around 8 year. In that perspective is methane not really shortlived. [Dirk Olivié, Norway]	Noted. These terms are being changed in the SOD, see new box in ch 8.
8-271	8	6	51	6	51	Not clear at the end of the paragraph if CH4 is considered a SLFC for this report. [Helen Worden, USA]	Noted. These terms are being changed in the SOD, see new box in ch 8.
8-272	8	6	53	6	55	As written the sentence is not quite correct. The immediate RF cause by a CO2 emission pulse does depend on location and time of the emissions. It is only because we're usually interested in the RF from cumulative past emissions, or cumulative RF from an emission pulse that we do not have to take location into account. Note that the time of emissions (10 years ago, now, or in 10 years) does matter for atmospheric concentrations on similar timescales. What does not matter is the time of emission in the year for the same reason as explicited above. [Olivier Boucher, France]	Noted. Will clarify that these are for broader impacts that we care about, and the time matters of course in the sense of now vs a decade later, but not in the sense of time of day or which month within a year.
8-273	8	6	53			Use long-lived instead of well-mixed. [Terje Berntsen, Norway]	Rejected. Our new terminology uses well-mixed precisely because it dconveys more physically relevant information. Will define clearly and use consistently.
8-274	8	6	53			LLGHGs, not well mixed? Check whole document? [Piers Forster, UK]	Rejected. Our new terminology uses well-mixed precisely because it dconveys more physically relevant information. Will define clearly and use consistently.
8-275	8	6	54			reads, can be related to the total change in emissions. Perhaps better to write related to the change in concentration [Christian Azar, SWEDEN]	Rejected. The discussion here is about relating forcing to emissions and how that differs for short-lived vs long-lived species.
8-276	8	7	3	7	3	The discussion of "limitations of radiative forcing" needs to include the point that RF does not allow comparison of non-radiative forcings, such as effects of land cover change on evapotranspiration or physiological impacts of CO2 and O3. These drivers of change are directly relevant to societal impacts and hence of interest to policymakers. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Accepted. Will add.
8-277	8	7	4	7	10	I don't think RF yields the tepmerature response, but I like this paragraph of caveats [Piers Forster, UK]	Noted. Using the simple formula of forcing times climate sensitivity parameter, forcing does yield dT. Or of course one could use and energy balance model, etc.
8-278	8	7	4			Reads, most widely used definitions of RF and most forcing based metrics are intended to yield the eventual temperature response. Formulation is problematic: metric as such will not give temperature response, co2 equivalent emissions of say 1 kg of methane will not at all give the same temperature response, efficacies are not included. [Christian Azar, SWEDEN]	Noted. The reviewer points out a limitation of a metric that lacks accounting for efficacy. However, it is still typically used as an indication of eventual temperature response.
8-279	8	7	13	7	16	Which AF varient will you show in the cartoon? Does AF matter if it is global mean T from regression of fixed SST, Andrews et al. 2010 I think compared these [Piers Forster, UK]	Taken into account. The caption already defines the AF in part d as the fixed-SST type.
8-280	8	7	20	7	20	In this chapter both "preindustrial" and "pre-industrial" are used. [Gareth S Jones, UK]	Editorial.
8-281	8	7	20	7	51	Seemed overly long for essentially an introduction. Not sure of point being made, has some overlap to previous section [Piers Forster, UK]	Noted. This is a section of it's own that we feel is an important topic to include. It has been substantially revised, however, and overlap reduced.
8-282	8	7	28		30	The sentence: " One way to evaluate this is to examine how much each forcing is contributing to the Earth's current energy imbalance with space (see Section 8.5)." is incorrect. The contribution to current imbalance would be accurate only if there had been no climate system response to that forcing. The objective is to determine the contribution of each forcing agent to the externally imposed change on flux since a given initial time. [Stephen E Schwartz, USA]	Noted. This sentence referred to the latter half of the previous sentence, the future temperature increase already in the system. Will clarify this in revision.

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8-283	8	7	29	7	29	Would it not be preferable to drop "with space"? [John Daniel, USA]	Accepted. Done.
8-284	8	7	32		51	This para is pretty simplistic. I am not sure why the phrase "energy imbalance" twice in lines 39-41. why not "forcing"? because of course when the system adjusts there is no longer any imbalance. Calculating for a sustained concentration or sustained emissions or for a pulse: none of these implies anything about future emissions; these are just hypothetical constructs. All of them. Line 38, again not "equilibrium." [Stephen E Schwartz, USA]	Noted. This section was heavily revised, and this is no longer relevant.
8-285	8	7	32			I'd add a fourth way to characterize, which is current forcing due to historical emissions: subtly different from current forcing due to current concentrations: Table 2.13 in AR4 is a good example of this, where it is clear that historical CH4 emissions contributed a lot more to current day forcing than would be evident by just looking at elevated CH4 concentrations. [Marcus Sarofim, USA]	Noted. This section was heavily revised, and this is no longer relevant.
8-286	8	7	33			Point 1). It seems like a misprint, shoudl "forcing" be replaced by "impact". [Terje Berntsen, Norway]	Noted. Forcing is correct, but we will see if text can be clarified.
8-287	8	7	36			Perhaps worth mentioning that the "pulse" is usually taken as instantaneous (Dirac delta function) and not constant emissions over one year (step function). [Glen Peters, Norway]	Noted. Will mention is space allows, but a minor point so we feel not a high priority (though correct).
8-288	8	7	36			Perhaps this can be made more explicit by stating that the "constant concentrations" is really an emission scenario that keeps the concentration constant. [Glen Peters, Norway]	Noted. Will try to use this in revision.
8-289	8	7	37			It seems that the AR5 will not use the term "committed" as used in the AR4. I will strongly support this as I believe it is misleading. [Terje Berntsen, Norway]	Accepted. We agree.
8-290	8	7	42	7	43	I do not believe I agree with the greater generality of a pulse. Constant emissions metrics can be mathematically added to achieve any actual temporal emission evolution as easily as a pulse metric. In fact, subtracting a constant emissions metric from itelf after shifting it one year would lead to the one-year pulse metric. I would suggest rewording this statement unless I am missing the point here. [John Daniel, USA]	Accepted. Both are general. We'll revise to reflect this.
8-291	8	7	42	7	43	"because pulse emissions possess greater generality". Sustained emissions are also quite general and have similar utility (http://www.agu.org/pubs/crossref/2011/2010GL045208.shtml). In a linear system, the equilibrium response to a sustained emission is the same as the integrated response to a pulse, so the two can be connected. The ODP was based on sustained emissions, as was some early GWP variants, and the GTP has a sustained emission counterpart. I am not aware why the GWP was decided to be based on a pulse and not sustained emissions? Give these points, I think it is quite bold to state that "pulse emissions possess greater generality" (without a reference) and gives the impression that a pulse is preferable for metrics (a value judgement). Perhaps you mean something weaker like "pulse emissions are particularly useful as they are the building block of arbitrary emissions via convolutions" or similar. [Glen Peters, Norway]	Accepted. Both are general. We'll revise to reflect this.
8-292	8	7	43			"…a choice of sustained emissions…" A pulse is also a type of scenario, or really, no scenario (assuming nothing about the future). The wording implies that a pulse is assumption free, but a pulse is an assumption just as sustained emissions are. [Glen Peters, Norway]	Accepted. Both are general. We'll revise to reflect this.
8-293	8	7	55	7	20	Shindell et al. 2011 worth citing. Seems out of place though why not discuss this when introducing patterns of RF? [Piers Forster, UK]	Accepted. Will add citation, and try to unify discussion in one place (or minimize here and refer to later).
8-294	8	8	2	8	2	"Ozone absorbs both…" change to "Tropospheric ozone absorbs both…" [Robert Portmann, United States of America]	Rejected. True for both tropospheric and stratospheric ozone.
8-295	8	8	5	8	5	Add the Lacis et al 1990 reference. [Robert Portmann, United States of America]	Rejected. This is an assessment, not a review, and we cannot cite all prior work. We are emphasizing newer works, and especially here those based on observations.
8-296	8	8	6	8	9	I think this chapter would benefit from a statement here regarding the fact that inhomogenous forcings do not necessarily give inhomogeneous responses, and that climate feedbacks may very well dominate the pattern of response, not the forcing. See chapter 10. [Susan Solomon, USA]	Noted. This is complex, as it depends on the relative magnitude of the various quasi-homogeneous vs inhomogeneous forcings. Feedbacks may dominate in

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							some cases, forcings in others. We will revise to try and reflect this more clearly.
8-297	8	8	15	8	47	text on GWP and GTP and box seem similar and overlapping. Genrally emission metric section seemed too long [Piers Forster, UK]	Taken into account. The chapter team decided to keep box. Metric section moved to later in the chapter and the text in box revised and reduced.
8-298	8	8	22	15	13	My comments will only focus on metrics, i..e., section 8.1.2. Overall I think the number of pages focused on metrics seem to be about right. I further think the general outline of the metric section is good, the length spent on general metric issues, GWP, GTP and "new metric concepts" is fine. I think it is important that new metric concepts and metrics taken into account economics is given space and the authors have succeded in doing so. Still I have a range of minor comments and suggestions. [Daniel Johansson, Sweden]	Noted. Thank you.
8-299	8	8	22	15	14	Suggest that the term impacts only be used for those impacts where adaptation has been accounted, and that the new term “climate impacts” be avoided or clearly defined. Climate impacts seem to be used in this section as any metric of climate and is dismissive of the relation between climate and actual impacts on society and ecosystems. [Haroon Kheshgi, United States of America]	Rejected. We do not agree. We think impact is OK to use as a synomyn to effect.
8-300	8	8	22	15	14	A key observation about metrics it that their definition should be appropriate for their use. The dominant use of metrics like GWP in the world today is to measure and manage emissions of GHG emissions. Suggest that the focus of this section be towards that use and the appropriate definition of metrics. This would argue for the reversal of the order of sections starting with the definition of the metric appropriate to its use (8.1.2.6-7) and then to specific metrics such as GWP and their application to different forcing agents. [Haroon Kheshgi, United States of America]	Noted. The chapter has been restructured. Section 8.1.2.7 is meant as a synthesis and it would not fit the structure to move this in front.
8-301	8	8	22			8.1.2: This is interesting, but are these metrics better described in the WGIII(?) where they have more relvence rather than here? [Gareth S Jones, UK]	Noted. According to scoping we need to have them here in WGI, but they may also to some degree be described In WGIII - with main focus on economics. But we don't know yet to which extent. We will coordinate with WGIII.
8-302	8	8	22			Section 8.1.2: I found this section to be much more of a review and not an assessment. I would suggest moving the discussion at the end of the section (page 15) that a choice of metric depends on the "particular use to which it will be put" to the very front and use this as a central theme in discussing the various metrics. [Robert Portmann, United States of America]	Taken into account as we will try to do more assessment. And to emphasize the point on "use to which it will be put" earlier in the (and probably also in ES.)
8-303	8	8	25	8	54	You could be more explicit in mentioning that there are absolute metrics which have a given application (this is mentioned later), and there are normalized metrics used to weight GHG relative to the reference. [Glen Peters, Norway]	Accepted. We will mention absolute and relative metrics.
8-304	8	8	25	9	49	Section 8.1.2.1 and Box 8.1 are fairly generic and do not convey much useful information [Larry Horowitz, USA]	Rejected. This has been discussed in the chapter team and it was decided to keep the section and the box. But the text is revised and shortened.
8-305	8	8	25	9	49	The introduciton and the box cover many related issues, but one needs to make a choice of what is in the introduction and what is in the box. Thus, both become incomplete. I would suggest combining the box and the introduciton and make the introduction more thorough on what a metric is, used for, etc. Another idea is to replace the box with a table similar to used in Tanaka et al (2010) Carbon Management (in reference list). [Glen Peters, Norway]	Noted. The tables from Tanaka et al. are too big and will require more explanation than we have space for here. Text in the box is revised.
8-306	8	8	26	15	14	I feel like this section is closer to a review than an assessment. If a policy maker reads this, I do not believe he/she will have an idea of the best path forward given the recent research. For example, should GWPs continue to be used as the trading metric in future climate talks? I realize that you cannot provide policy suggestions and that you are only assessing the science, but I feel that you could go a bit further in summarizing what is good and bad with the current metrics we have and maybe even what future science advancement is needed or what specific guidance is needed from the policy makers to make progress on this issue of metric choice. [John Daniel, USA]	Taken into account. We will do more assessment.

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8-307	8	8	27	8	27	«place their impacts on a common scale»: I do not fully agree with this word "on a common scale", I think. For me formulating a metric has different important aspects. First, there is work in describing as good as possible the impact of certain emissions. For this, indeed, results of different models are combined and analyzed, see e.g. Fuglestvedt et al. (2010). E.g., for NOx one tries to describe all its impacts, i.e. on ozone, methane, CO2 (via O3 damaging the plants), ... This can also be done per sector. Secondly, these impacts are expressed per unit mass of the emitted species. Thirdly, one has to choose a function which expresses what one wants to compare as the consequence of a unit emission: the RF forcing over a certain time period, or the temperature at a certain time after the emission, or the precipitation change, ... One can even imagine a metric function which is based on multiple parameters, and maybe not only on global variables. [Dirk Olivié, Norway]	Rejected. We agree with much of this but still it is valid to say that the metrics place the impacts on a common scale; e.g. Kelvin, W/m2, CO2-eq.
8-308	8	8	27	8	29	Could emphasise more that emissions metrics by and large are short-cuts aimed to aide decision-making. In principle, one could use complex climate models to fully evaluate the effects of any emission in all respects that are tractable with currently available models - metrics aim to avoid the need having to model each and every emissions consequence in detail. By making this clear up-front, it should become clearer to readers that short-cuts inevitably entail assumptions and omissions, which is where the value-ladenness of metrics comes to the fore. [Andy Reisinger, New Zealand]	Taken into account. (We tried to make this clear; but will try improve the text. This is also the point of fig 8.2. see page 8 line 40-43.)
8-309	8	8	27			This introduction needs a broader introduction as to why comparisons between emissions of different gases are needed, who needs to make them and what for. E.g. list emissions trading in international agreements but also for domestic policy purposes, life cycle analysis involving emissions of more than one substance, evaluating benefit of mitigation R&D measures, etc etc. Set the scene to say that ultimately, the pull for metrics comes from policy demands (which is fine), and hence naturally the policy purpose will have an influence on decisions about which metrics are appropriate, science cannot determine good or bad metrics without this interaction with the ultimate purpose of why and by whom emissions are to be compared. [Andy Reisinger, New Zealand]	Rejected. These are good points, but due to space limitations it is unfortunately not possible to expand on these issues.
8-310	8	8	28	8	28	«One has to choose a climate impact parameter.» I would express it more as: «One compares/focusses on specific impacts.» [Dirk Olivié, Norway]	Rejected. We think one has to choose an impact parameter. If not it would be impossible to compare.
8-311	8	8	28	8	29	"Various types of models are needed ...": In principle one could imagine that one model is able to give all the information. An AOGCM containing chemistry could in principle be able to allow to quantify the whole chain from emissions until climate impacts ... [Dirk Olivié, Norway]	Accepted. We now write "Various types of (sub)models are needed …"
8-312	8	8	28	15	13	"parameter": a parameter is in general not something that you observe or measure, or which is the solution of a problem (end point). A parameter is more a free variable which plays internally a role in the dynamics of a system, and can possibly have different values to describe different possible dynamics. [Dirk Olivié, Norway]	Taken into account. The text is rewritten.
8-313	8	8	32	8	32	It is not clear to me what "application" means in the figure. [John Daniel, USA]	"Application" has been removed to avoid confusion. Boxes on right and left contain explaining text.
8-314	8	8	36	8	37	Parameterization rather than linearization would be better. Nonlinear approaches have also been used (e.g. Hooss et al., 2001, Climate Dynamics). [Katsumasa Tanaka, Switzerland]	Taken into account. This part of the sentence is deleted now.
8-315	8	8	36			I think also use of metrics in legal agreements, with a reference to the Kyoto Protocol should be mentioned here. [Terje Berntsen, Norway]	Rejected. We dont think a reference to KP is needed here. We write that they can be used as exchange rates in multi-component policies. We think this should be sufficient.
8-316	8	8	36			Not clear how these metrics differ from RF ones introduced ealier. It seems like you don't need to introduce the concept of metrics again here if done ealier effectively. Just say GWP and GTP belong to a suite of forward looknig metrics? Infact defintion here seems different as they are tools for multi gas policy, wheras they were previously defined differently [Piers Forster, UK]	Taken into account, The structure of the chapter will be changed. But will try to make it more clear how these metrics differ from the RF ones introduced earlier
8-317	8	8	40	8	43	The most useful and important application of metrics is multi-gas climate policies, which I would state at the beginning of the bullet. Some cautionary statements could be added here. For example, metric users need to be aware of the limitation when metrics are used in a climate impact assessment because metrics do not	Rejected. We start with saying that metrics quantify contributions, which is the basis for their applications. Then we mention applications in policies.

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						necessarily carry sufficient information for decision makers. [Katsumasa Tanaka, Switzerland]	
8-318	8	8	40			Metrics do not only communicate the relative contribution. The AGTP itself gives the contribution to the warming [Terje Berntsen, Norway]	Taken into account. The point will be made in the text.
8-319	8	8	41			It would be useful I think to link to "Life Cycle Assessment" here as they use the GWP religiously. Peters et al (2011) ES&T which suggests using GTP in LCA instead of GWP. This will provide a link to WGIII where LCA is used to analyze energy systems, etc. This is important, I think, as the current text can be seen to indirectly critique the use of GWP. WGIII should know that there are other metrics. [Glen Peters, Norway]	Taken into account.
8-320	8	8	43			I suggest to be more concrete by changing to: Furthermore, metrics provide the exchange rates needed in multi-component ……….. [Terje Berntsen, Norway]	Rejected. One may have multi-component polcies without metrics (e.g. "gas-by-gas")
8-321	8	8	45	8	46	You could discuss the problems with using CO2 as a reference gas. For instance, the decay of atmospheric CO2 differs quite a bit from other species, making a comparison more difficult. While most other gases/particles/effects are totally removed from the atmosphere after some time, about 20 % of the CO2 stays for thousands of years. [Borgar Aamaas, Norway]	Taken into account. (This will be discussed but not necessarily here).
8-322	8	8	45			Just to open a can of worms, "It is common to use CO2 as a reference", but is this the "best" reference gas? Could link to comparing SLCF with LLGHG (e.g., Peters et al (2011) ERL, Multi-basket approaches, etc) [Glen Peters, Norway]	Taken into account. (This will be discussed but not necessarily here).
8-323	8	8	46	8	49	"To transform the effects of different emissions ... and i is the component." The word «horizon» comes here a bit abrupt, maybe it could be explained before. Also the way CO2 equivalent is introduced is maybe not so clear. I would rather write: "This dimonsionless number expresses the relative difference in strength between the impacts. It can also be used to express the CO2-equivalent of a certain species, i.e., the amount of CO2 emission that would cause the same impact (or give the same value for that specific metric).» [Dirk Olivié, Norway]	Taken into account; i.e. horizon not mentioned here.
8-324	8	8	47	8	48	Define E = Emissions [David Stevenson, UK]	Taken into account. E is defined.
8-325	8	8	48	8	48	Define E as standing for "emissions". [Nicolas Bellouin, United Kingdom]	Taken into account. E is defined.
8-326	8	8	48	8	48	Note that not every metric has a time horizon (see Boucher, ESDD, 3, 1-29, 2012 and older papers), so it would be better to start with general statements before going into the specifics. [Olivier Boucher, France]	Accepted as suggested.
8-327	8	8	48			I suggest to skip time-horizon here since it has not been introduced. M_i * E_i = CO2-eq [Terje Berntsen, Norway]	Accepted as suggested.
8-328	8	8	48			E is not defined, I dont think [Glen Peters, Norway]	Taken into account. E is defined.
8-329	8	8	49	8	50	"Ideally, the climate effects should be the same regardless of composition of the equivalent CO2 emissions, but in practice this is not possible." This is a strange way of expressing it. I would rather say something like: "equal values for 2 species in a certain metric do not imply equal values in another metric". [Dirk Olivié, Norway]	Rejected. While your suggestion may be somewhat more precise, the general wording we used is closer to how users think about this and more relevant.
8-330	8	8	50	8	50	It would be helpful to provide some reasons here for why it is not possible. [John Daniel, USA]	Accepted. But due to space restrictions we have only inserted one sentence.
8-331	8	8	50	8	50	"in practice this is not possible" - give more detail as to why this is not possible. The key reason is that different GHGs have very different physical properties, and a metric that established equivalence with regard to one property cannot guarantee equivalence with regard to other proprties or effects. This is implicit in this para but would benefit from being out more clearly, so that at the end of this para the trade-off is clear: whenever you want to use any single metric, you will only ever end up with equivalence in a particular respect for which the metric is designed, and only an approximate (and in some cases, not-so-approximate) equivalence in most other potentially relevant respects. [Andy Reisinger, New Zealand]	Accepted. But due to space restrictions we have only inserted one sentence.
8-332	8	8	50	8	52	There are other factors that should be considered in designing a metric (e.g. policy relevance). It is still an open question as to wether a transparent metric can really be policy relevant. [Katsumasa Tanaka,	Noted. The sentence referred to is now deleted.

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						Switzerland]	
8-333	8	8	50			I suggest to add a ref. to O'Neill (2000) as to why this is not possible [Terje Berntsen, Norway]	Accepted as suggested.
8-334	8	8	51	8	51	In the end a metric is a number, so it is easy to apply. I don't really buy the argument that the definitionof a metric has to be simple because it is to be used by non-specialists. A non-specialist probably does not understand a GWP calculation, that's already too complicated anyway. [Olivier Boucher, France]	Taken into account. (Sentence deleted).
8-335	8	8	51	8	52	It is not clear what “for these purposes” is referring to. Also, “easy to apply” should maybe be precedented by a discussion whether they are “appropriate to apply” I think. [Dirk Olivié, Norway]	Taken into account. (Sentence deleted).
8-336	8	8	56	8	56	As with comment 12, some discussion of the reasons why this is true should appear somewhere early in the section. [John Daniel, USA]	Accepted. But due to space restrictions we have only inserted one sentence.
8-337	8	8	57	9	1	“the most appropriate metric will depend on which aspects of climate change are most important to a particular application”: The parts “aspects of climate change are most important to a particular application” is rather unspecific. It again gives the impression that you may choose the metric that best suites you. [Dirk Olivié, Norway]	Rejected. We believe that the current text is sufficeently clear wrt how the metric depends on the goal.
8-338	8	8		9		GWP concept. I urge again, now, as I have done for several IPCC reports, that the CO2 denominator in GWP calculations be abandoned and rather that AGWP's be presented. I recognize there is long history and political advantage to use of CO2 based GWP. But it cannot be scientifically defended, on the grounds of uncertainty in CO2 forcing and impulse profile. I call attention in the latter to major differences in CO2 profile among recent papers listed in next row of this spreadsheet (Note overlapping authorship). For a comparison of the decay profiles of these several papers following a hypothetical cessation of CO2 emissions see Schwartz, S. E. Well Known . . . to a Few People: Attribution of Excess Atmospheric CO2 and Resulting Global Temperature Change to Fossil Fuel and Land Use Change Emissions. American Geophysical Union Fall Meeting, San Francisco CA, December, 2010. Poster A21A-0018. http://www.ecd.bnl.gov/steve/pres/WellKnownAGU10vgphs.pdf viewgraph 7. Sooner or later GWP will be abandoned in favor of AGWP's. I see you cite Reisinger later to this effect. I urge GWP's be abandoned now. Ort at least AGWP's be presented in parallel in all instances. One more point. Recall in AR4, table 2.14 presented new GWPs for many substances, not because of any change in understanding of the substance but because of change in understanding of CO2. [Stephen E Schwartz, USA]	Taken into account. We will present both relative and absolute metrics. And more attention to why these metrics change due to updates (RF, IRF) and with time will be given. The role of CO2 as reference gas is also given more attention.
8-339	8	8		9		REFERENCES TO ARTICLES EXHIBITING MODEL STUDIES SHOWING HIGHLY DIFFERING DECAY PROFILES OF CO2 FOLLOWING CESSATION OF EMISSIONS Solomon S, Plattner GK, Knutti R, Friedlingstein P. Proc Natl Acad Sci U S A. 2009 Feb 10;106(6):1704-9 J A Lowe, C Huntingford, S C B Raper, C D Jones, S K Liddicoat and L K Gohar Environ. Res. Lett. 4 (2009) 014012 (9pp) doi:10.1088/1748-9326/4/1/014012 How difficult is it to recover from dangerous levels of global warming? BILL HARE and MALTE MEINSHAUSEN HOW MUCH WARMING ARE WE COMMITTED TO AND HOW MUCH CAN BE AVOIDED? Climatic Change (2006) 75: 111–149 DOI: 10.1007/s10584-005-9027-9 Matthews, H. D., and K. Caldeira (2008), Stabilizing climate requires near-zero emissions, Geophys. Res. Lett., 35, L04705, doi:10.1029/2007GL032388. M. EBY, K. ZICKFELD, AND A. MONTENEGRO D. ARCHER K. J. MEISSNER AND A. J. WEAVER Lifetime of Anthropogenic Climate Change: Millennial Time Scales of Potential CO2 and Surface Temperature Perturbations VOLUME 22 JOURNAL OF CLIMATE 15 MAY 2009	Noted.

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						Thomas L. Fro¨licher • Fortunat Joos Reversible and irreversible impacts of greenhouse gas emissions in multi-century projections with the NCAR global coupled carbon cycle-climate model Clim Dyn DOI 10.1007/s00382-009-0727-0 Myles R. Allen, David J. Frame, Chris Huntingford, Chris D. Jones, Jason A. Lowe, Malte Meinshausen & Nicolai Meinshausen Warming caused by cumulative carbon emissions towards the trillionth tonne Nature Vol 45830 April 2009 doi:10.1038/nature0801 [Stephen E Schwartz, USA]	
8-340	8	9	1	9	3	“different climate policy goals may lead to different conclusions about what is the most suitable metric with which to implement that metric”: this gives the impression that the metric is identical to the “cost”, “tax” or whatever «insentive» to arrive at a certain aim – which is not true I think. [Dirk Olivié, Norway]	Noted. The paragraph has been revised, but not this sentence.
8-341	8	9	1	9	3	Berntsen et al. (2010, Climatic Change Letters) explicitly terms it as" the combined target and metric approach", which can be introduced here. [Katsumasa Tanaka, Switzerland]	Noted. But due to space restrictions not possible to discuss or refer to that here.
8-342	8	9	1	9	4	What you are really saying, is that it is not so smart to have one metric for all applications. Perhaps mention that explicitly. [Glen Peters, Norway]	Noted. But to keep the text short we could not add this.
8-343	8	9	3	9	3	What is an example of the tools? [Katsumasa Tanaka, Switzerland]	Rejected. We think the text is sufficiently clear.
8-344	8	9	4	9	4	Policies can be more than "multi-gas", i.e. they can include emissions of non-gaseous species such as primary carbonaceous aerosols. [Nicolas Bellouin, United Kingdom]	Taken into acccount. Gas is replaced by component.
8-345	8	9	4			As mentioned on page 8, metrisc are also used to evaluate (not only implement) different multi-gas (change to multi-component) policies. [Terje Berntsen, Norway]	Accepted. Has added "evaluation"
8-346	8	9	4			To be more explicit, "It is important to note that…goals and policy, BUT THE OTHER WAY AROUND as tools that…" [Glen Peters, Norway]	Noted. But due to space restrictions not possible to discuss or refer to that here.
8-347	8	9	6	9	8	The use of GWP was used here before it is defined. [Fiona O'Connor, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
8-348	8	9	6	9	8	I suggest to cut this text as you take it up later in more detail. [Glen Peters, Norway]	Accepted as suggested.
8-349	8	9	6	9	8	Might be worth mentioning explicitly that metrics are also used increasingly in life cycle analysis, and could heavily skew the conclusions reached by such analyses, with significant implications for economics and trade. [Andy Reisinger, New Zealand]	Partly taken into account by adding LCA as one of the various applications.
8-350	8	9	6			Please, add after the words "Cherubini et al. (2011) " the words "and Pingoud et al. (2011)"… Reference: Pingoud, K., Ekholm, T., Savolainen, I. Global Warming Potential (GWP) factors and warming payback time as climate indicators of forest biomass use. Mitigation and Adaptation of Strategies for Global Change (3 November 2011), pp. 1-18. DOI 10.1007/s11027-011-9331-9 [Ilkka Savolainen, Finland]	Taken into account (will be added later in the text)
8-351	8	9	8			At the end of the paragraph, please, add: "Kirkinen et al. (2008) developed a metric which can directly estimate the greenhouse gas and albedo impacts thru energy balance change due to activity considered." Reference: Kirkinen, J., Palosuo, T., Holmgren, K., Savolainen, I.: Greenhouse impact due to the use of combustible fuels – Life cycle viewpoint and Relative Radiative Forcing Commitment. Environmental Management (2008) 42:458-469. [Ilkka Savolainen, Finland]	Rejected. We could not find this in the paper.
8-352	8	9	11	9	49	Box 8.1 is a great idea and I fully support its inclusion. It should however be entitled "How can human influences on climate be compared" as anthropogenic influence extends beyond just emissions, and it should also include a brief discussion of non-radiative forcings (eg: impacts of land cover change on evapotranspiration and the physiological impacts of CO2 and O3), the importance of these for societal impacts (hence policy relevance) and the difficulty of comparing these in terms of current RF-based metrics. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Rejected. Good comment, but rejected as non-radiative effects will be covered in other parts of the chapter. We have also added a short para in the section on new concepts.
8-353	8	9	11	9	49	Box 8.1. The authors should consider replacing this box with a FAQ. The issue of emission metrics has gained a lot in policy relevance and interest, but the confusion about the issues is large. Producing a more extensive	Rejected. Good point, but it is not possible to introduce more FAQs at this stage.

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						FAQ than this box would really help clarify key issues for policymakers. As it stands, the box seems to be a half-way house between a FAQ and a scientific perspective, but adds little value to either in my view in its current form. [Andy Reisinger, New Zealand]	
8-354	8	9	15	9	47	This box would be a good place to talk about metrics at a more fundamental level. Right now, it seems you are expecting the reader to understand a lot of background. Just as an example, I'm not sure that most readers will understand what you mean by 'discounting of future effects' and the implications of this without some additional discussion. Also, a discussion of the wide variety of GHG lifetimes and the implication of this variety on the ability of metrics to 'equate' impacts over various times would be useful. I realize this has been done in past assessments, but it seems worth repeating. Consider introducing global damage potential and cost potential in more detail somewhere, too, perhaps here. [John Daniel, USA]	Rejected. Good point, but due to space limitations we cannot expand the box and give a more a fundamental presentation of metrics. We have however inserted some words about discounting and damage based metrics. This is also a potential topic for WGIII.
8-355	8	9	15			It may be worth giving a very entry level example of a metric. E.g., if x kg of CO2 and ykg of CH4 is emitted, then the CO2-eq (GWP) = x+25*y (but use some real numbers) [Glen Peters, Norway]	Rejected. Good suggestion, but not possible due to space restrictions
8-356	8	9	17	9	17	I do not see the distinction being drawn between "models" and "metrics". Metrics such as RF are produced from models albeit less complex ones than full-blown AOGCMs. Are you trying to say that metrics are a compact summary of model results? [Robert Waterland, United States of America]	Taken into account. Clarification added.
8-357	8	9	27	9	30	One could cite Tanaka et al. (2009, Climatic Change) as an example that uses both foreward and backward-looking perspectives in computing metric values. [Katsumasa Tanaka, Switzerland]	Rejected since we do not give references for the various perspectives discussed here.
8-358	8	9	32	9	38	the concepts of "level" and "rate" of change are introduced but not further explained. A brief discussion in paraentheses would help the reader. [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Brief clarifications added.
8-359	8	9	33	9	33	I suggest replacing "measured as" by "measured as a function of". [Olivier Boucher, France]	Noted. The para has been reworded.
8-360	8	9	36	9	36	discounting of future effects' - I find this unclear and ambiguous - discounting can mean several things. [David Stevenson, UK]	Taken into account. Clarification added.
8-361	8	9	42	9	42	Define CAP [David Stevenson, UK]	Taken into account. We will not use "CAP" in the next draft.
8-362	8	9	47			"…and PROBABLY cannot be based on science alone" [Glen Peters, Norway]	Noted. Changes in line with this are implemented.
8-363	8	9	47			Please, add after the paragraph: ", but scientific studies can be used to analyse different approaches and policy choices." [Ilkka Savolainen, Finland]	Accepted. Suggested text has been inserted.
8-364	8	9	52			One key aspect of the GWP which should be stressed more is that it is calculated based on constant background concentrations: given the likely increase in CO2 concentrations over time, this will bias the GWP compared to a hypothetical "optimal" metric that could be calculated based on perfect foresight... [Marcus Sarofim, USA]	Accepted. This issue has now been given more attention.
8-365	8	9	54	10	3	The quotation shows how unsatisfacory the GWP concept is amd how unfair is its current application [VINCENT GRAY, NEW ZEALAND]	Noted.
8-366	8	9	54	10	24	Repeats text and citations from AR4, is all this necessary? [Piers Forster, UK]	Rejected. And this text was not given in AR4. We think it is important to make readers aware how tentative this was.
8-367	8	9	54			I would suggest to change to : The GWP was presented in the First ….. [Terje Berntsen, Norway]	Accepted.
8-368	8	9	54			Excellent that these quotes are here. If someone suggests to remove them, I suggest to keep them! [Glen Peters, Norway]	Noted.
8-369	8	9	57	9	57	make clear that the 100-year GWP was adopted, not just the GWP as metric per se, as this highlights the rather bold leap of faith made by the policy process in responses to the careful and nuanced discussion by the IPCC [Andy Reisinger, New Zealand]	Accepted

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8-370	8	10	1	10	1	This story appears in Shine (2009, Climatic Change), which can be cited here. [Katsumasa Tanaka, Switzerland]	Accepted. Will add reference.
8-371	8	10	2	10	2	“to a common scale”: see earlier comment on common scale. [Dirk Olivié, Norway]	Rejected. See reponse to similar comment.
8-372	8	10	3	10	3	could make clear here that even though the term "CO2 equivalent" is used stand-alone and universally now whenever 100-year GWPs are used, the only thing that really is CO2-equivalent is the integrated radiative forcing over 100-years, but the equivalence is weaker or poor with regard to many other relevant climatic criteria [Andy Reisinger, New Zealand]	Accepted.
8-373	8	10	3			Perhaps add something like "The GWP has not been recommended as a robust metric for policy applications in its reports". Which is true, emphasises that input is needed from several disciplines (c.f., Shine 2009 Climatic Change), and that policy makers should ask before they apply something! [Glen Peters, Norway]	Rejected. Due to space limitations we cannot og further into these issues beyond what is already included.
8-374	8	10	5			A fuller definition of a pulse emission (pulse-based line 38) including timescale would be useful here. [Ruth Doherty, UK]	Rejected due to space limitations, but figure 8.3 (original numbering) should convey the most important aspects. We have also some more documentation and discussion included now in Supplementary Material.
8-375	8	10	5			I suggest including the quote on the 20, 100, 500 years in FAR: "...as candidates for discussion [that] should not be considered as having any special significance" [Glen Peters, Norway]	Accepted as suggested.
8-376	8	10	6	10	8	This looks like a simplification because the shape of the CO2 response also matters in addition to the CO2 adjustment time (if this can be defined). [Katsumasa Tanaka, Switzerland]	Taken into account. The text on this issue has been rewritten now.
8-377	8	10	7	10	7	You are using "adjustment time" rather than lifetime or half-life. I think you need to add some explanatory material. [Robert Waterland, United States of America]	Taken into account to the extent possible given space restrictions. (We have also added Supplementary Material.)
8-378	8	10	7	10	8	The authors write "gases with adjustment times shorter than the adjustment time for CO2, the GWP values will decrease with increasing time horizon, since GWP is defined with the integrated RF of CO2 in the denominator." This statement is unclear the since the adjustment time for CO2 cannot be reflected in one time constant. Please clarify 8 increasing time horizon, since GWP is defined with the integrated RF of CO2 in the denominator." [Daniel Johansson, Sweden]	Taken into account. The text on this issue has been rewritten.
8-379	8	10	7		8	More on GWP. The absurdity of GWP as normalized to CO2 becomes apparent in the statement "For gases with adjustment times shorter than the adjustment time for CO2, the GWP values will decrease with increasing time horizon, since GWP is defined with the integrated RF of CO2 in the denominator. " This problem becomes exacerbated the shorter the residence time of the substance of interest; in the limit, say, an aerosol. I recommen a switch to AGWP, now, and with explanation why the switch is made, rather than try to explain absurdities such as that forevermore. The advantages of AGWP in this respect have been apparent at least since 1993 (Schwartz, 1993). It's not anything having to do with sulfate that its GWP continues to decrease year after year; its AGWP is constant once it is removed from the atmosphere. Its the AGWP of CO2 that keeps building up year after year. Bite the bullet now and deal with explaining the reason for the switch to AGWP once; stay with GWP and regret forever, or at least until you make the change to AGWP at some later date. Schwartz, S. E., Energy Internatl. J. 18, 1229-1248 (1993).Does fossil fuel combustion lead to global warming? (http://www.ecd.bnl.gov/steve/pubs/Fossil.pdf). [Stephen E Schwartz, USA]	Taken into account. We will present both relative and absolute metrics. And more attention to why these metrics change due to updates (RF, IRF) and with time will be given. The role of CO2 as reference gas is also given more attention.
8-380	8	10	10	10	14	Yes, the choice of time horizon is value-based and there is no single time horizon that is appropriate for all purposes. Can any guidance be given on whether there are any inappropriate time horizons? For example, are time horizons less than 20 years appropriate? I would argue that consistent with previous IPCC reports time horizons less than 20 years are not meaningful. [Timothy Wallington, USA]	Rejected. Good point, but difficult due to space limitations. However, we have discussed this briefly in the Supplementary Material. This issue also involves impact considerations (WGII), but this is as far as we know, not included in the scoping of WGII.

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8-381	8	10	12	10	14	I like that you are coming right out and saying this directly. [John Daniel, USA]	Noted. Thank you.
8-382	8	10	13	10	13	Change "no conclusive scientific argument" to "no scientific argument". [Robert Portmann, United States of America]	Rejected since there may be scientific arguments, but not conclusive,
8-383	8	10	13	10	15	I certainly agree that the choice of a time horizon (or equivalently a discount rate - they're essentially the same things) is value-laden. However I wouldn't say that the choice is completely open, even from a physical climate point of view. There are good reasons for a time horizon of 100 years or so as it is the typical timescale over which climate change is a problem (eg carbon cycle and ocean heat timescales). [Olivier Boucher, France]	Rejected. These are good points but it is difficult to go into this rather huge and broad issue due to space limitations. This issue also involves impact considerations (WGII), but this is as far as we know, not included in the scoping of WGII.
8-384	8	10	14	10	14	Please say why the choice is value-based (i.e., it depends on the value assigned to damages in the longer versus shorter term) [Susan Solomon, USA]	Accepted.
8-385	8	10	14	10	14	As a paper which stresses the value-laden aspects of metrics, Tanaka et al. (2010, Carbon Management) could be cited. But there may be more papers, which I am not aware of. [Katsumasa Tanaka, Switzerland]	Noted. But we do not need references for this here.
8-386	8	10	17	10	17	"into any specific climate response parameter" - insert "other" before 'specific' since the RF is a climate response parameter [Andy Reisinger, New Zealand]	Rejected since we do not consider RF a response parameter
8-387	8	10	18	10	19	Tanaka et al. (2009, Climatic Change) can be cited here because the paper evaluated GWP against the historical data. [Katsumasa Tanaka, Switzerland]	Accepted. Will add reference.
8-388	8	10	18			Perhaps add Peters et al 2011, ERL [Glen Peters, Norway]	Rejected, due to enough references already
8-389	8	10	20	10	21	“that emissions that are equal in terms of CO2 equivalents will not result in the same climate response over time”. It is strange that 8 references are used to support some conclusion that has been mentioned already twice earlier in the text, line 49—50 page 8-8, and line 56 page 8-8. [Dirk Olivié, Norway]	Rejected. Yes, this has been mentioned in general for metrics earlier in the text, but need to show strong basis in the literature in the case of GWP.
8-390	8	10	21	10	21	replace "over time" with "at all times", since the response may well be the same or very similar over some times [Andy Reisinger, New Zealand]	Rejected since paragraphs has been revised.
8-391	8	10	21	10	24	The similarity between pulse GWP and sustained GTP has mathematically been shown by Peters et al. 2011a. You could reference that. [Borgar Aamaas, Norway]	Accepted.
8-392	8	10	22	10	22	“integrated forcing”: maybe express that it is integrated until infinity. [Dirk Olivié, Norway]	Noted. The paragraphs has been rewritten
8-393	8	10	22	10	24	I suspect this is only true for time horizons long enough. [Olivier Boucher, France]	Noted. The paragraphs has been rewritten
8-394	8	10	23	10	23	Is one not allowed to say “equal” instead of “similar”? [Dirk Olivié, Norway]	Noted. The paragraphs has been rewritten
8-395	8	10	23			Change from similar to equal. This assumes that the efficacy is equal to 1.0. [Terje Berntsen, Norway]	Noted. The paragraphs has been rewritten
8-396	8	10	23			With reference to Peters et al 2011, ERL, the temp response to a sustained emission is the same as the integrated temp response to a pulse. That is, the references and Peters et al show that GWP is approx iGTP [Glen Peters, Norway]	Noted. The paragraphs has been rewritten
8-397	8	10	23			On reflection of some of the early documentation (Derwent R G 1990 Trace Gases and their Relative Contribution to the Greenhouse Effect (Harwell: Atomic Energy Research Establishment) Document: AERE R 13716; Wuebbles D J 1989 Beyond CO2—the other greenhouse gases Lawrence Livermore National Laboratory Report UCRL-99883; Air and Waste Management Association Paper 89-119.4) it seems apparent that Derwent and Wuebbles had integrated temp in mind before the FAR was written. The FAR dropped the link to temp, and I cant find a reference for why. Later studies have shown a link between GWP and iGTP, when in fact, the intention may have been that GWP is an approx of iGTP! I think this is perhaps an important point to mention as, I think, it helps put the GWP in a better context. The founders of the GWP did not arbitrarily integrate RF, but integrated temp. [Glen Peters, Norway]	Taken into account.
8-398	8	10	24	10	24	“emission changes”: maybe just “emissions”. [Dirk Olivié, Norway]	Noted. The paragraphs has been rewritten

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8-399	8	10	24	10	24	One can also say that “the ratio of integrated forcing from pulse emissions of two gases is equal to the ratio of the integrated temperature response for pulse emissions of these two gases”. (integrated must be interpreted here as integrated until infinity) [Dirk Olivié, Norway]	Noted. The paragraphs has been rewritten
8-400	8	10	24	10	24	I would say that it gives an interpretation of AGWP, rather than of GWP. [Dirk Olivié, Norway]	Noted. The paragraphs has been rewritten
8-401	8	10	26			Section 8.1.2.3: The non-expert will find some of this section confusing. There is no attempt to define GTP in terms of RF so: (i) it might have been calculated from a climate model run, (ii) what is the reader to make of the statements on p.11 l.11-12 that "GTP requires additional assumptions about the climate sensitivty" and on p.12 l.4 "[lambda] appears in both the numerator and denominator". Also the last sentence on p.10, when "efficacies" haven't been defined, will confuse. [incidentally I'm delighted to see a focus on GTP instead of efficacies which I thought were not helpful!] [Joanna Haigh, UK]	Taken into account. We have added equations and more information in the supplementary material.
8-402	8	10	28	10	32	Since an average temperature of the earth's surface cannot be made, the GWP concept is dependent entirely on unsatisfactory measurements [VINCENT GRAY, NEW ZEALAND]	Noted.
8-403	8	10	28	10	33	I suggest that you should clearly state here what emission profile the GTP is based upon, i.e.,a pulse, a step or a general emission profile. Nothing is written about that as it is now. [Daniel Johansson, Sweden]	Accepted. We have addedd information on this. (the illustrating figure 8.3 also inidcates pulses).
8-404	8	10	28	10	55	Please clarify that the GTP concept is similar to and developed after the STRE (Surface temperature response per unit emissions), which was first calculated in FIGURE 14 of Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, J. Geophys. Res., 107, (D19), 4410, doi:10.1029/ 2001JD001376, 2002 and defined more formally in SECTION 4.5 and TABLE 4 of Jacobson, M.Z., Short-term effects of controlling fossil-fuel soot, biofuel soot and gases, and methane on climate, Arctic ice, and air pollution health, J. Geophys. Res., 115, D14209, doi:10.1029/2009JD013795, 2010 [Mark Z. Jacobson, U.S.A.]	Taken into account. STRE is presented in a later section.
8-405	8	10	29			Change in global mean surface temperature ... [Terje Berntsen, Norway]	Accepted as suggested.
8-406	8	10	31	10	33	Mention that "i" is for the "ith" component. [Robert Portmann, United States of America]	Accepted as suggested.
8-407	8	10	32	10	33	What is "the absolute GTP"? [Henning Rodhe, Sweden]	Taken into account. We have rewritten the text.
8-408	8	10	32			Since you put the equation in for GTP, perhaps worth including for GWP? [Glen Peters, Norway]	Taken into account. The equation given here is related to applications of GTP. The equations for definitions of GWP and GTP are given in the figure. And more equations for GTP are given in the Supplementary Material.
8-409	8	10	37			You have the equation for T in terms of emissions and AGTP, but also worth putting in the AGTP in terms of E and IRF (in the appendix of Boucher and Reddy 2008). This shows more clearly how the GTP is related to the GWP (that is, by the inclusion of a physical discount term, the IRF). [Glen Peters, Norway]	Taken into account. We have added equations and more information in the supplementary material.
8-410	8	10	38	10	38	Define AGTPs. In line 32, page 10, lower case letters refers to which gas the AGTP is calculated for. In line 38, I think the lower case S stands for step but I am not sure. Please clarify. [Daniel Johansson, Sweden]	Taken into account.
8-411	8	10	38	10	38	I don't know if this paragraph is really necessar – it feels like inverting things. The fact that responses to general emission scenarios can be expressed as convolutions is something that exists on its own, and is not specifically related to the metric problematics. I would also write on line 38 “as the pulse AGTP” instead of “as pulse based AGTPs”. Why not write “as the convolution of the emission scenario em with t he absolute temperature potential (AGTP)”? [Dirk Olivié, Norway]	1) Rejected. We think this application is a useful aspect of AGTPs. 2) Accepted. 3) Rejected since we believe that the current wording is easier to understand for a broader audience.
8-412	8	10	38	10	43	Add a statement about this equation assuming linearity and about the accuracy of this assumption. [John Daniel, USA]	Accepted (i.e. we add "assuming linearity")
8-413	8	10	43			Sugges to add Peters et al 2011 ES&T [Glen Peters, Norway]	Taken into account. Will add reference
8-414	8	10	43			The AGTP values AND EMISSIONS are needed for all times up to th, just to be clear. [Glen Peters, Norway]	Accepted as suggested.

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8-415	8	10	45	10	45	I would suggest "by accounting for physical climate processes" instead of "by accounting for the climate sensitivity and the exchange…". [Katsumasa Tanaka, Switzerland]	Rejected since this may be misunderstood as many other processes not accounted for.
8-416	8	10	46	10	49	A counter argument here would be that the value of MGTP is very similar to GWP (e.g. Gillett and Matthews, 2010, Environmental Research Letters). Tanaka et al. (2009, Climatic Change) shows this in the context of TEMP. Thus, the implication to the actual metric values may be small. [Katsumasa Tanaka, Switzerland]	Rejected. Even if GWP is similar to MGTP it does not include the physical processes that GTP includes.
8-417	8	10	49	10	50	The authors write "Shine et al. (2005b) presented the GTP for both pulse and sustained emissions, and used a simple model to account for the uptake of heat by the ocean. This has later been developed by accounting for the longer time scales of the ocean (Berntsen and Fuglestvedt, 2008; Boucher and Reddy, 2008; Collins et al., 2010; Fuglestvedt et al., 2010)". I do not think that the sentence fully reflects what was done in the Shine et al (2005b) paper. In that paper GTP values was also estimated using an Upwelling Diffusion Energy Balance Model (UEBM), which captures the long time scales of the ocean. [Daniel Johansson, Sweden]	Accepted. We will add that an energy balance model as well as analytical equations were used.
8-418	8	10	54	10	55	Both RF and AF suffer from non-zero efficacies so the "(rather than AF)" distinction is incorrect. Efficacies would be a good idea in either case. [Robert Portmann, United States of America]	Taken into account - This sentence is deleted.
8-419	8	10	54	10	55	It does not sound right to say that the climate sensitivity is built into GTP (although I understand what this means). This sentence needs to be revised. [Katsumasa Tanaka, Switzerland]	Taken into account - This sentence is deleted.
8-420	8	10	54			Last sentence. First, I think the meassge is a bit unclear. Is the recommendadtion to use efficacies AND RF, rather than AF? Secondly, this claim needs a reference to a paper. I don't understand why efficacies and RF is better than using AF. [Terje Berntsen, Norway]	Taken into account - This sentence is deleted.
8-421	8	10	55			Efffiacay not properly defined yet. If using adjusted forcing do you need to account for effiacy? [Piers Forster, UK]	Taken into account - This sentence is deleted.
8-422	8	10	55			Has anyone used AF instead of RF? Are you suggesting they should? If so, perhaps mention that [Glen Peters, Norway]	Taken into account - This sentence is deleted.
8-423	8	10		11		Strongly suggest do not introduce or discuss or treat GTP in this (forcing) chapter. GTP is not a forcing quantity. It is a response quantity. It requires assumptions about magnitude of climate response to forcing and timing of response. On page 8-47 it is stated that the whole concept rests on the Boucher Reddy temperature impulse profile. Reliance on any such profile makes the whole enterprise doubtful. Get rid of it. If the concept is to have any justification it must be in a response chapter, not a forcing chapter. [Stephen E Schwartz, USA]	Rejected. Inclusion of GTP is given by the scoping of AR5.
8-424	8	11	9			No climate response is explicitly included in the GWP concept and it is based on the RF concept. [Pieter Aucamp, South Africa]	Noted
8-425	8	11	10	11	10	This is not the case any longer because in AR5 climate-carbon cycle feedbacks will be taken into account in the CO2 IRF. [Katsumasa Tanaka, Switzerland]	Accepted. This will be discussed together with presentation of updated AGWP-CO2
8-426	8	11	10			Incomplete sentence. [Terje Berntsen, Norway]	Accepted and corrected.
8-427	8	11	13	11	13	The uncertainty ranges are larger for GTP than for GWP only in relative terms, not necessarily in absolute terms. [Olivier Boucher, France]	Noted. But we prefer to focus on relative values.
8-428	8	11	14	11	14	Please compare with Reisinger et al. (2009, GRL) and cite if this fits here. [Katsumasa Tanaka, Switzerland]	Taken into account.
8-429	8	11	18			iGTP has less uncertainty then GTP, but is further down the "chain", Peters et al 2011 ERL [Glen Peters, Norway]	Rejected. The integration of the chosen parameter will not move it down the cause effect chain.
8-430	8	11	18			If GWP is a proxy for iGTP, then arguably GWP should appear after T in the cause effect chain (chain would be RF, T, int RF and int temp, …) [Glen Peters, Norway]	Rejected. But integrated RF as such is before delta T in the cause effect chain
8-431	8	11	20	12	23	I would not call the issues related to the choice of metric "uncertainties". [Dirk Olivié, Norway]	Rejected. After considerations and discussions among the authors we decided to use the language from the literature.

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8-432	8	11	20	12	23	Section 8.1.2.4: it would really help to have a table that shows the different values for GWPs and GTPs for CH4 for different time horizons, with their estimated uncertainties, plus the effect of potentially including more feedbacks, as it helps highlight the different scales of scientific and structural uncertainties. Alternatively, such a table would make a lot of sense in an FAQ on metrics if an FAQ were to be developed. [Andy Reisinger, New Zealand]	Rejected. Good suggestion, but not possible due to space restrictions
8-433	8	11	20	12	23	Another type of uncertainty that is somewhat buried in this discussion is what processes are included in the definition of a metric. E.g. Especially for methane, whether one includes some of the feedbacks identified by Shindell et al (2009), and also whether climate-carbon cycle feedbacks discussed by Gillet and Matthews (2010) are included. This is partly a definitional issue and hence structural, but this is not clear from the current discussion. Another issue is whether metrics are defined relative to constant background concentrations, or for projected future changes, which then links with scientific uncertainties about the effect of the spread of plausible futures. [Andy Reisinger, New Zealand]	Accepted (will add discussion on consistency)
8-434	8	11	20			Somewhere, it may be worth elaborating a little on Peters et al 2011, ERL. They interpret the AGWP, AGTP, iAGTP in terms of an energy balance. The AGWP is the total energy added to the system, c.AGTP energy currently in the atmopshere, ocean mixed later, and iAGTP/lambda is the energy lost. Since the AGWP is the total energy added it remembers the effects of the RF regardless of what happens afterwards (e.g., energy lost back to space). In other words, AGWP has perfect memory (also seen by putting IRF=1 in the AGTP definition). [Glen Peters, Norway]	Taken into account to some extent.
8-435	8	11	22	11	22	"parametric" might be better than "scientific". [Olivier Boucher, France]	Rejected. After considerations and discussions among the authors we decided to use the language from the literature.
8-436	8	11	22	11	26	From a perspective of climate modelling, many (including myself) would call an uncertainty in a model equation or a model choice as a structural uncertainty (as opposed to a parameter uncertainty), which is not compatible with the definition here. Isn't the typology for uncertainty harmonized throughout the report? However, I am aware of the difficulty in arriving at single clear definitions of uncertainties. Here is an alternative uncertainty typology: see Figure 1 of Tanaka et al. (2010, Carbon Management). Factors shown on the left hand side would be policy uncertainties and those on the right hand side would be be scientific uncertainties. [Katsumasa Tanaka, Switzerland]	Rejected. After considerations and discussions among the authors we decided to use the language from the literature.
8-437	8	11	22	12	23	Please consider adding a citation to Sarofim 2011 (Environ. Model Assess, DOI 10.1007/s10666-011-9287-x) on CH4 GTP calculations: there are a couple of interesting contributions of this paper, regarding both the use of more complex models and more in-depth comparisons of the GWP to the sustained GTP concept. In particular, it could be added as a citation on pg. 11, line 13, and in the discussion on pg. 12, line 4-12, where Sarofim looked at dependence on climate sensitivity, baseline concentration assumptions, and the rate of ocean uptake of heat and CO2... [Marcus Sarofim, USA]	Taken into account; these issues are discussed (but without reference to the mentioned paper)
8-438	8	11	31	11	31	The concept of impulse response function needs to be discussed. [Robert Waterland, United States of America]	Taken into account by the presentation of this concept in the Supplementray Matreial.
8-439	8	11	31			definition of impulse response function is rather vague, perhaps an example here would be useful [Ruth Doherty, UK]	Taken into account by the presentation of this concept in teh Supplementray Matreial.
8-440	8	11	34	11	34	typo: "cycle, , and because…"; remove one comma [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Editorial.
8-441	8	11	34	11	34	What does it mean by "prior values"? [Katsumasa Tanaka, Switzerland]	The text has been rewritten.
8-442	8	11	41	11	41	This sentence needs to be reworded as it starts too suddenly from the previous paragraph. Also, it is not clear what is meant by "constant background atmosphere". [Robert Portmann, United States of America]	The text has been rewritten.
8-443	8	11	41	11	41	I think that this statement needs to take into account the fact that constant background concentrations have been assumed in AR4 and TAR (and will probably be in AR5). [Katsumasa Tanaka, Switzerland]	Taken into account, This has been rewrittem and feedbacks and background levels are given more attention.

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8-444	8	11	41	11	45	You could also mention the concentration-forcing relationship for CO2, since increasing concentrations will decrease the radiative efficiency. This point is important since CO2 is the denominator in emissions metrics (GWP/GTP). [Borgar Aamaas, Norway]	Accepted. This will be discussed together with presentation of updated AGWP-CO2
8-445	8	11	41			I guess there is only one strict defintion of teh GWP, and that is that for the Kyoto P. GWP as defined in the SAR should be used, i.e. with a constant background. [Terje Berntsen, Norway]	The text has been rewritten.
8-446	8	11	41			Drop "strickly" I think [Glen Peters, Norway]	The text has been rewritten.
8-447	8	11	42	11	42	This includes feedbacks from the climate system (e.g. climate-carbon cycle feedbacks). [Katsumasa Tanaka, Switzerland]	Taken into account. The text will be re-written here
8-448	8	11	42			Why use "turnover rates" instead of adjustment time that is elswhere in the chapter? [Terje Berntsen, Norway]	Accepted. Will be rewritten.
8-449	8	11	44	11	44	If you use RCP, you must refer to it as Representative Concentration Pathway (RCP) and you nust define it. [Robert Waterland, United States of America]	Accepted as suggested.
8-450	8	11	45			What if a scenario is used instead of constant background. See for example, Caldiera and Kasting 1993 Nature, Wuebbles et al 1995 Climatic Change, Enting et al 1994 "Koala Report", Reisinger et al (2011) ERL, etc. In other words, constant background is a choice. [Glen Peters, Norway]	Accepted. The text is changed.
8-451	8	11	47	2	2	Gillett and Matthews (2010, Environmental Research Letters) can be included in the discussion here because they use a range of models for metric calculations (from a simple energy balance model to an earth system model). [Katsumasa Tanaka, Switzerland]	Accepted. Yes, I will use more of their results in discussions.
8-452	8	11	49	11	49	The authors write “Shine et al. (2005b) used one time-constant for the climate response”. I do not think that the sentence fully reflects what was done in the paper. In the paper GTP values was also estimated using an Upwelling Diffusion Energy Balance Model (UEBM), in addition to a one layer energy balance model. One cannot, in general, capture the response time in an UDEBM in a single time-constant. [Daniel Johansson, Sweden]	Accepted. Here we write "analytical expression". And earlier in the text we added that an EBM was also used.
8-453	8	11	53			It shoudl be made clearer that "Use of a more raelistic function …" refers to the "two time-constants" described in the previous sentence. [Terje Berntsen, Norway]	Accepted as suggested.
8-454	8	11	55	11	55	"…as compared to the one time-constant climate response approach". [Olivier Boucher, France]	Accepted as suggested.
8-455	8	12	1	12	2	I cannot see why an impulse response approach should be preferred over approaches based on UDEBMs (or similar) models given that they are calibrated to emulate GCM models, or observational data. Please clarify, or make the sentence more neutral concerning which approach that is preferred. [Daniel Johansson, Sweden]	Taken into acccount by additing "if analytical expressions are preferred".
8-456	8	12	4	12	4	A GTP expression with a climate sensitivity parameter is not given in the chapter. One should be given or this sentence should be re-worded. [Robert Portmann, United States of America]	Accepted. We will add equations and more information in the supplementary material.
8-457	8	12	4	12	4	Ramda is more often an inverse of the equilirium climate sensitivity. In Chapter 10 of AR5 FOD, it is defined so and ramda is called climate feedback parameter. This is potentially a source of confusion. Is it possible to reconcile the difference in the definitions across the chapters? [Katsumasa Tanaka, Switzerland]	taken into account
8-458	8	12	4	12	4	“The climate sensitivity parameter appears in both the numerator and denominator of the GTP expression” - lambda is not explicit in the definitions for GTP on p. 8-10 or in Fig. 8.3. [Helen Worden, USA]	Accepted. We will add equations and discussion; as well as more information in the supplementary material.
8-459	8	12	4			It may also be stated that GTP values for contrails are senstive to changes in the climate sensitvity. For instance, In Azar Johansson (2011), already cited in the report, we write that "the 100-year GTP value for contrails roughly doubles if the climate sensitivity increases by 50%. The climate inertia increases with increasing climate sensitivity; as a consequence, the temperature response from contrails lingers for a longer period of time. However, since the contrail contribution to a 100-year GTP-based emissions weighting factor is only 3% of the contribution from CO2, the impact of a change in the climate sensitivity will nevertheless be relatively small. " This sentence explains why there is more uncertainty for GTP (despite the fact that CS is both in the nominator and the denominator)	Rejected. Good points, but we do not have space for such considerations on a specific effect from one specific sector.

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						but is also shows that it matters very little for the overall analysis. [Christian Azar, SWEDEN]	
8-460	8	12	4			Although it is correct that both the numerator and the denominator of eth GTP expression is a function of lambda, it does not "appear" in the expressions explicitly. Lambda incfluences teh time constants in teh impulse response function for the climate response. I suggest to replace "appear" with "influences" or something similar. [Terje Berntsen, Norway]	Taken into account in the discussion of these issues.
8-461	8	12	5			It would be helpful to give the equation for climate sensitivity here. [Ruth Doherty, UK]	Taken into account. We have already given earlier in the chapter and will add a reference to that.
8-462	8	12	6	12	6	Does this sensitivity occur because of the relationship between climate sensitivity parameter and deep ocean mixing (thus, climate response time scales)? Whatever the reason, it would be good to state it. [John Daniel, USA]	Taken into account by adding more discussion on this issue.
8-463	8	12	14	12	23	There need to be a discusson of how to chose time horizons for the GWP and the GTP, stating that a given time horizon means different focus in a GWP context than in a GTP. [Terje Berntsen, Norway]	Rejected. This is a huge and broad issue and a proper treatment of this would require more space and is also an issue for the other WGs.
8-464	8	12	14	12	23	It may be worth comparing pulse and constant emissions to highligh how the SLCF have a much larger contribution in the constant emission case [Glen Peters, Norway]	Taken into account in illustrations and applicationsin later sections.
8-465	8	12	18			Since there is no reason to choose the same time horizon for the GWP as for the GTP, the statement in this sentence (that the impact of aviation is larger using GWP) is not valid on a general basis. [Terje Berntsen, Norway]	This is rewritten now.
8-466	8	12	19	12	19	Please add reference concerning the aviation related claim. For example, refer to Azar & Johansson (2011): Valuing the non-CO2 climate impacts of aviation, available online in Climatic Change. [Daniel Johansson, Sweden]	Accepted. Will add reference.
8-467	8	12	21	12	23	You should perhaps mention the secondary effects of contrails, in addition to BC and CH4. The lifetime of contrails is even shorter than BC; thus, acting even more different than CO2. For aviation, the impact due to contrails is a large part of the total. [Borgar Aamaas, Norway]	Rejected, due to space limitation.
8-468	8	12	22	12	22	Perhaps "…timescales DRAMATICALLY different" since all are different. [John Daniel, USA]	This is rewritten now.
8-469	8	12	23	12	23	«emissions of CH4»: shouldn't it also include emissions that affect the concentration of CH4? [Dirk Olivié, Norway]	Rejected, since we only need simple and clear examples here.
8-470	8	12	25	12	25	Presumably some discussion will be added about the recent UNEP report on ozone and black carbon as well as the related Shindell et al paper? [Katharine Law, France]	Noted. This section will be moved and revised.
8-471	8	12	25	13	4	There are many very general statements in this section but no indication of how large these effects can be. Indicate how important these effects are. [Robert Portmann, United States of America]	Taken into account. But this section will be moved and revised.
8-472	8	12	27	12	32	This paragraph seems somewhat unfocused. It could be rewritten to be more coherent. [John Daniel, USA]	Taken into account. But this section will be moved and revised.
8-473	8	12	27			Define SLCF (first use in chapter) [Pieter Aucamp, South Africa]	Accepted (SLCF will be NTFC; near term climate forcers)
8-474	8	12	27			Remove "can", keep it if you mean "large and rapid". [Terje Berntsen, Norway]	Taken into account. This section will be moved and revised.
8-475	8	12	28	12	28	I don't understand the purpose of "but these effects quickly equilibrate". Please clarify. It seems that the first part of this sentence is more important. [John Daniel, USA]	Taken into account. This section will be moved and revised.
8-476	8	12	28	12	28	“but these effects quickly equilibrate”: what is meant is that the SLCF evolves fast to a new equilibrium burden I presume, but this is not so clear from the sentence as it is written. [Dirk Olivié, Norway]	Taken into account. This section will be moved and revised.
8-477	8	12	28	12	29	The issue with the halocarbons is not their current forcing but their potential future forcing. Because of	Taken into account. This section will be moved and

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						projected growth in demand for refrigeration, air conditioning, etc., particularly in developing countries, there is the possiblity that forcing will become quite large in the coming decades. The current statement seems too dismissive of this fact. [John Daniel, USA]	revised. Need to chech the likely future RF from short-lived halocarbons
8-478	8	12	34	12	39	Can the "primary mode" (Prather, 2007, Philosophical Transactions of the Royal Society A) be discussed here? Metrics have been calculated by considering the primary mode (e.g. Berntsen et al., 2005, Tellus B). [Katsumasa Tanaka, Switzerland]	This section is moved and rewritten
8-479	8	12	37			Define VOC (first use in chapter) [Pieter Aucamp, South Africa]	Taken into account. This section will be moved and revised.
8-480	8	12	42			It is unclear to me what is meant by "specific forcing pulse" [Terje Berntsen, Norway]	Section moved and rewritten.
8-481	8	12	45			I do not see that an RTP would be a problem. You would just have a "matrix" of radiative efficiency and lifetimes that are regionally depenent? [Glen Peters, Norway]	Taken into account. We will clarify this.
8-482	8	12	47	12	56	This para should give a sense of the magnitude of the various feedbacks, to help policymakers understand how important it may (or may not) be to better quantify those feedbacks and decide whether they should be include in calculations of the GWP and GTP metrics or not. This links with the discussion of uncertainties in the preceding section. [Andy Reisinger, New Zealand]	Accepted. This issue will be given some more attention; we will add a sense of the magnitude of the various feedbacks.
8-483	8	12	47	12	56	It might be worth noting here that the indirect effects can be both system-dependent and location-dependent, even when the direct effects aren't. There is clearly much greater uncertainty in these indirect effects, too. [Oliver Wild, United Kingdom]	we find this comment unclear. However, much of the text has been rewritten.
8-484	8	12	54	12	54	Maybe put “response time” in plural? [Dirk Olivié, Norway]	Noted. This refers to page 10, line 54.
8-485	8	13	1	13	4	This para should be expanded by considering two additional studies. One is Cox PM, Jeffery HA (2010) Methane radiative forcing controls the allowable CO2 emissions for climate stabilization. Current Opinion in Environmental Sustainability 2(5-6): 404-408, which discusses (without explicitly referring to metrics) the fact that methane impacts on ozone influences the carbon cycle and hence influences the CO2 emissions consistent with a long-term target. The other, more important in my view, is Gillett NP, Matthews HD (2010) Accounting for carbon cycle feedbacks in a comparison of the global warming effects of greenhouse gases. Environmental Research Letters 5(3): 034011. They discuss the fact that the emission of CH4 results in warming, which in turn prolings the atmospheric pool of CO2 emissions. This feedback effect is not included in the current definition of GWPs but could add another 20%, and plausibly could be included. This is an important issue for policymakers, they need to decide whether they are satisfied that the way the GWP is defined captures the relevant feedbacks. [Andy Reisinger, New Zealand]	Taken into account (but only to some extend due to space restrictions). This section will expanded to include some more discussion of these carbon cycle indirect effects.
8-486	8	13	6	14	25	I don't find this extended discussion of economic metrics to be a useful addition to this chapter. Instead, they can be mentioned briefly in 8.1.2.1 or 8.1.2.7 [Larry Horowitz, USA]	Rejected. We need to bulid a bridge to the work in WGIII on this and to show that there are more aspects to metrics than purely natural sciences. We have kept this discussion short and do not agree that this is an extended discussion.
8-487	8	13	6	15	13	Include discussion of work topwards metrics that consider non-radiative forcings, eg: Huntingford et al (2011). Highly contrasting effects of different climate forcing agents on terrestrial ecosystem services. Philos T R Soc A, 369(1943), 2026-2037. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Rejected. Due to space restrictions we could not go much into these issues.
8-488	8	13	6	15	13	These are nice sections on new metrics and synthesis [Piers Forster, UK]	Noted. Thanks!
8-489	8	13	6	15	13	Much of the literature on metrics that are assessed are not “new” but rather have been omitted in past assessments of WG1. What is new is that a more comprehensive treatment of metrics is now being included. Suggest that the chapter be clearer about it choosing to provide a comprehensive treatment in this assessment in one place in the AR5; that is what is new and not the metrics, many of which have existed for some time. An excellent example is Hammitt et al., 1996 which is referenced in the IPCC metrics report but omitted in this chapter. [Haroon Kheshgi, United States of America]	Taken into account. We try to show the "big picture" but main focus is given to physical metrics. Many issues are addressed just briefly due to space limitations.

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8-490	8	13	10	13	10	I would invert the order of this sentence and write: “The GWP is based on the RF concept and no climate response is explicitly included.” [Dirk Olivié, Norway]	Taken into account. The text has been rewritten.
8-491	8	13	10			Manne and Richels could be added? [Glen Peters, Norway]	Accepted. Refernce addedd.
8-492	8	13	12	13	13	Maybe replace “requires additional assumptions about the climate sensitivity” by “depends on the climate sensitivity which is not well known”. [Dirk Olivié, Norway]	Taken into account. The sentences will be rephrased. (Reference to page is not correct)
8-493	8	13	16	13	16	I would skip “closer to responses”. [Dirk Olivié, Norway]	Taken into account. The text has been rewritten.
8-494	8	13	16	13	25	In this paragraph, it seems as if the global damage potential is determined by the relative damage cost whereas the the global cost potential is determined by the abatement cost. This dichotomy gives an erroneous description of how it all works. The "Kandlinkar approach" (global damage potential) is correctly described, but it should also be noted that since the marginal abatement cost should be equal to the marginal damage cost, the ratio between the marginal abatement costs for the different gases and the ratio between the marginal damage costs for the two gases are the same. The same thing applies for the cost-effective approach. Basically, the temperature target implies a constraint in the optimisation model which translates into a shadow price on gas X and CO2. It is the ratio between these two shadow prices that defines the metric. (All this implies that the abatement cost curves will have very little impact on the shadow price ratios, the impact is very indirect in that changes in the abatement costs will affect when the temperature target is met and this in turn will affect the distance in time from the emissions occur and the target is met, and thus the relative valuation between say methane and carbon dioxide change). MY SUGGESTION THUS, IF YOU WANT TO WRITE THIS AS SIMPLY AS POSSIBLE, IS TO REPLACE MARGINAL ABATEMENT COST ON LINE 19 AND 20 WITH SHADOW PRICE RATIO (THIS HAS TO BE DONE TWICE). [Christian Azar, SWEDEN]	Taken into account. The description will be clarified. This will involve introducing and defining the concept of shadow price. We will not go into great detail, but will give references.
8-495	8	13	16	13	25	In this paragraph only the "Global Cost Potential" and the "Global Damage Potential" are discussed. But what about the "marginal cost/marginal damage" potentials. Also, the explaination of GCP and GDP uses the concept of "marginal abatement costs" which are not further discussed. It would be helpful to introduce these terms here or at least refer to other sections and not only to the literature. [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Accepted. We can clarify the text that uses these different concepts. Marginal abatement cost is no longer used.
8-496	8	13	19	13	20	The authors write “The price ratio, also called the Global Cost Potential (GCP; Tol et al., 2009), is defined as the ratio of the marginal abatement cost of a gas to the marginal abatement cost of CO2 within a scenario that meets the target at least cost”. The price ratio that Manne & Richels suggested is not necessarily equal to the ratio of marginal cost, it is only under some special conditions that this hold. Please write something like “defined as the ratio of the shadow price of emissions of a gas to the shadow price of emissions of CO2. These shadow prices can be interpreted as the optimal tax on the emissions of each gas that are required to meet the climate target at the lowest possible cost.” [Daniel Johansson, Sweden]	Accepted; we can make this change, see comment 494 above which addresses the same point.
8-497	8	13	19			Reads: the price ratio, also called the global cost potential, … Please consider the following rewrite, The price ratio, also called the global cost potential (GCP, Tol et al, 2009), or the cost effective trade off ratio (CETO, Azar & Johansson, 2011), The reason for this is that we felt, when we wrote the paper, that the acronym by Tol et al does not convey the meaning of the ratio, whereas our suggestion (hopefully) does. [Christian Azar, SWEDEN]	Accepted; can rewrite along these lines.
8-498	8	13	22	13	23	Reads:known as the global damage potential, (Kandlikar, 1995), Please consider to rewrite as follows... known as the global damage potential, (Kandlikar, 1995), or the relative damage cost (Azar Johansson 2011). The reason for this is that we felt, when we wrote the paper, that the acronym by Kandlikar does not convey the meaning of the ratio, whereas our suggestion (hopefully) does. [Christian Azar, SWEDEN]	Accepted; can rewrite along these lines.
8-499	8	13	23	13	25	It might be worth saying that along the optimal trajectory GCP equals GDP. However GDP can be defined in a cost-effectiveness approach as well (i.e. along a decided climate trajectory which does not have to coorespond a cost-benefit optimal). [Olivier Boucher, France]	Rejected. It is true that marginal abatement costs equal marginal damages along an optimal trajectory, however the GCP is not always equal to the marginal abatement cost (text had been revised to reflect this fact). Therefore GCP does not always equal GDP.
8-500	8	13	25	13	25	Maybe explain “discounting”. [Dirk Olivié, Norway]	Taken into account

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8-501	8	13	26	14	25	It is critical that WGI liaise with WGIII about a shared discussion of metrics across the two Working Groups. I'm happy with this discussion to occur in WGI, but really it belongs to and should be picked up by WGIII. If WGIII fails to deal with metrics, then the discussion here should be expanded further; but if WGIII does give adequate treatment of metrics from a mitigation/cost perspective, then this discussion can probably be curtailed a little. [Andy Reisinger, New Zealand]	Taken into account. If discussion of metrics is included in WGIII we still need to have some discussion of aspects beyond natural sciences also in WGI. This will be coordinated with WGIII.
8-502	8	13	27	8	31	This para either needs to be expanded significantly (if WGIII fails to adequately consider metrics) or deleted. If it is kept, it should be expanded with the following issues: (a) quantify the cost-inefficiency at global level arising from GWPs compared to cost-minimising metrics, as otherwise policymakers can be misled into thinking that GWPs are completely wrong to achieve mitigation goals, whereas they are simply not perfect for the job (as nothing is!) Line 29 needs to make clear that GWPs are not economically optimal only if the sole goal of climate policy is to limit long-term warming; if it also wants to limit the rate of warming they are not half bad, as Manne and Richels have shown. Add Reisinger et al (submitted to Climatic Change) to the studies that compare the global-level cost-inefficiency of GWPs with other metrics. [Andy Reisinger, New Zealand]	Accepted. (With minor modifications we can be more explicit about cost implications and about the benefits of GWPs even if non-optimal. So, major expansion is not necessary (and probably not possible given space constraints). Deleting the text does not seem ncessary given that it can be clarified.)
8-503	8	13	27	8	31	With regard to country/region level impacts, Shine (2009) doesn't give concrete figures but only conjectures. Revise text and cite Godal and Fuglestvedt (2002) and Reisinger (2012, report to NZ Ministry of Agriculture) to show that very few studies have actually explored costs at country/regional level, and those that have indicate that the picture is quite complicated, and even for a country with large CH4 emissions such as New Zealand the implications are highly dependent on other policy settings especially those linked to international trade and changes in carbon prices. [Andy Reisinger, New Zealand]	Accepted.
8-504	8	13	27	13	29	Can you provide more information about this? There is not enough information to understand how general this statement is. Is it for attaining a particular maximum T increase, or always when using GWPs? The cost increase being relatively small must depend on the application. [John Daniel, USA]	Accepted; this can be clarified. By definition a GWP will always lead to higher costs relative to an optimal (least cost) metric, because the GWP is non-optimal. The text is unclear at the moment however because the comparison is between physical and economic metrics, rather than physical and optimal metrics.
8-505	8	13	27	13	29	This statement certainly cannot be true in general (i.e., without specifying a what is meant by short-lived and specifying a time-horizon). This whole paragraph strikes me as an attempt to review a bunch of papers in as few lines as possible. [Robert Portmann, United States of America]	Accepted. This will be clarified; see also response to comment 8-504
8-506	8	13	31	13	31	Recent work by Reisinger et al should be relevant here. This is not publicized in a peer review journal yet but results could be find in the per reviewed report" Implications of alternative metrics to account for non-CO2 GHG emissions" (2012) by Andy Reisinger and Adolf Stroombergen and prepared for the Ministry of Agriculture and Forestry, New Zealand. [Daniel Johansson, Sweden]	Taken into account. If this work is available as paper we may refer to that.
8-507	8	13	33	13	42	See Boucher (ESDD, 3, 1-29, 2012) . [Olivier Boucher, France]	Accepted
8-508	8	13	35	13	35	The sentence starting with "GTPs, for example.." makes little sense without looking up the papers. [Robert Portmann, United States of America]	Rejected. We can't think of any clearer way to state this, so have not changed the text.
8-509	8	13	37	13	42	This is not clear. It also may not be appropriate to include in a WG1 report, where issues like price ratios and cost-benefit issues are not discussed in enough detail to make this readable for the non expert. Isn't this better left to WG3? [Susan Solomon, USA]	Taken into account. Will clarify this.
8-510	8	13	41			Add one sentence (with a reference) explaining why (or how) the GWP is an interpretation of the GDP. [Terje Berntsen, Norway]	taken into account.
8-511	8	13	41			I would suggest to add something like "This could suggest that it is not the economics that causes the price ratios to change, but rather, a metric that converges to a target" [Glen Peters, Norway]	Rejected, given the space for this section.
8-512	8	13	44	13	55	Another metric, the CETP, is introduced without further details. This poses a fundamental question: Should the potential AR5 reader be required to go and read the literature or should the AR5 summarize these metrics to present an integrated overview. This question can be asked more generally. [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Noted. But we think this section is aimed at giving a sense of what types of new metrics are being developed and used, rather than to fully explicate all of them.

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8-513	8	13	48			This is a partially erroneous description of GTP. Using GTP there is essentially infinite discounting before t=H and after t=H. Thus, it cannot be stated that for GTP there is no discounting through the time horizon H. [Christian Azar, SWEDEN]	Taken into account.
8-514	8	13	55	13	55	Reference should be (Azar and Johansson, 2011), not (Johansson and Azar, 2011) [Daniel Johansson, Sweden]	Accepted. Will be corrected.
8-515	8	13	55			It may be added that "For a given time horizon H, GWP, IGTP, SGTP and the relative damage cost metric - given that the discount rate corresponds to the time horizon H, are similar in values.On the other hand GTP and the cost effective trade off (CETO) are similar. " [Christian Azar, SWEDEN]	Rejected. We tried this out, but it introduces too many indexes that are not discussed in the preceding paragraphs, and will be too hard to follow to insert here.
8-516	8	13				The discussion of metrics especially economics seems out of place in WG1. To my thinking it occupies a lot of space; could be omitted or shortened. [Stephen E Schwartz, USA]	Rejected. We don't agree that this occupies a lot of space, and we find that this is needed to establish a link to WGIII. We also find it important to show that there are aspects related to metrics that go beyond natural sciences.
8-517	8	14	1	14	3	Can you add the CETP values or range of values to this figure? [John Daniel, USA]	Rejected. We considered this but found that it would be too complicated. Many readers find this too complicated already as it is.
8-518	8	14	1			This figure requires a better explanation. It is difficult to understand. [Henning Rodhe, Sweden]	Taken into account. A better explanantion will be given to the extent possible within the space limits.
8-519	8	14	5	14	5	It is not clear what it means by "take into account temperature effects over a broader time horizon." [Katsumasa Tanaka, Switzerland]	Taken into account; the text is rewritten.
8-520	8	14	7	14	7	Shine (2009, Climatic Change) characterizes TEMP as a metric to ensure a climatic equivalence. This term can be introduced here because it captures what TEMP means concisely. [Katsumasa Tanaka, Switzerland]	Rejected. We considered this but it did not work very well.
8-521	8	14	8	14	8	Here one could consider adding an important aspect of TEMP, which is missing in the discussion here. While both GWP and GTP are more transparent in terms of their analytical definitions than TEMP, their policy implications are less clear when they are applied in a context of a mitigation scenario. TEMP is derived explicitly from a chosen scenario and clearly reflects a climatic goal. From this perspective (tranparency vs. policy relevance), TEMP is somewhere between GCP and GWP (Shine, 2009, Climatic Change). [Katsumasa Tanaka, Switzerland]	Rejected. Due to space limitations, we cannot discuss this in further detail.
8-522	8	14	12	14	13	There is an alternative explanation (p.453 and 454 of Tanaka et al. (2009, Climatic Change)). FEI and TEMP are computed differently. FEI is computed every year to reproduce the forcing (i.e. an optimization for each single year). A TEMP value for present is obtained by a single optimization using an emission scenario from the present to a target year throughout. Then a TEMP value for the next year is based on an emission scenario from the next year to the target year. In other words, while FEI does not carry information of a policy time horizon, TEMP reflects a policy time horizon. Such fundamentally different computational methods explain the difference between the behaviors of TEMP and FEI. [Katsumasa Tanaka, Switzerland]	Rejected. Due to space limitations, we cannot discuss this in further detail.
8-523	8	14	15			Again with reference to Peters et al 2011 ERL, it is likely that the original intention of the GWP was to be the iGTP. [Glen Peters, Norway]	Rejected due to space limitations.
8-524	8	14	18	14	19	Change "sustained pulse emission" to "sustained emission". [Robert Portmann, United States of America]	Accepted as suggested.
8-525	8	14	20	14	23	See also: Azar, Christian; Johansson, Daniel J.A (2012).:On the relationship between metrics to compare greenhouse gases – the case of IGTP, GWP and SGTP. Earth System Dynamics Discussion, 3 (1) pp. 113-141, and the relatively old article: Rotmans, J. and den Elzen, M. G. J.: A model-based approach to the calculation of global warming potentials (GWP), Int. J. Climatol., 12, 865–874, 199. [Daniel Johansson, Sweden]	Accepted. Will add reference to the ESDD paper.
8-526	8	14	20			Reads: "If the time horizon is 100 years". Comment: the similarity between GWP and SGTP is much deeper	Taken into account.

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						than that and numerical similarities holds for almost all time periods (they are, under linearised conditions) even asymptotically equal when the time horizon approaches infinity. " [Christian Azar, SWEDEN]	
8-527	8	14	21	14	22	O'Neill and Peters et al have shown this, but it may also be noted that similar results are produced in Rotmans and den Elzen, and Azar & Johansson, 2012. Full references: Rotmans, J. and den Elzen, M. G. J.: A model-based approach to the calculation of global warming potentials (GWP), Int. J. Climatol., 12, 865–874, 1992.Azar, C & Johansson, D.J.A., 2012. On the relationship between metrics to compare greenhouse gases – the case of IGTP, GWP and SGTP Earth Syst. Dynam. Discuss., 3, 113–141. [Christian Azar, SWEDEN]	Taken into account. Reference to Azar and Johansson is given.
8-528	8	14	26			This is a better place to mention the GWPbio (Cherubini et al 2011), but mention that it has not been estimated for GTP, but since the IRF goes negative, then it is likely that GTP goes negative too. [Glen Peters, Norway]	Taken into account.
8-529	8	14	26			This is a better place to mention albedo in metrics. Articles such as Betts 2000 Nature, Rotenberg, E., Yakir, D., 2010. Contribution of Semi-Arid Forests to the Climate System. Science 327, 451-454, Bright et al (2011), etc. Many other references [Glen Peters, Norway]	Taken into account.
8-530	8	14	27	15	13	The summary needs to step up one level I think and provide clearer guidance and conclusions for policymakers. The AR4 stated that "GWPs are a useful metric for comparing the potential climate impact of the emissions of different LLGHGs". This assessment provides a more nuanced picture that needs to be summarised more clearly. Something in the sense that GWPs remain a potentially useful metric to inform climate policy given their transparency and good understanding of relevant factors and their uncertainties. However, recent literature has shown more clearly their limitations and implications for the cost-effectiveness of mitigation measures, and highlighted that alternative metrics could remedy some of those shortcomings. Alternative metrics bring their own issues though, such as greater scientific uncertainties and lesser transparency or greater dependence on specific assumptions". If the discussion of economic issues is retained, it is important that this summary clearly states that at the global level, different metrics tend to result in only small cost differences (order of less than 10%), and that implications at national, regional or sectoral level remain underexplored and will be heavily dependent on other policy assumptions. [Andy Reisinger, New Zealand]	Taken into account. We will add clearer conclusions. But the second point about impacts on costs is outside the scope of this chapter and should be covered by WGIII.
8-531	8	14	28	15	14	This is an important section that should be less of a summary and more of an assessment. I suggest that it is structured so that it goes through the metrics introduced in the previous sections, and clearly lists the pros and cons. That should start with a description of the policy framework that the metric is suited for, and include factors like uncertainties and transperancy. [Terje Berntsen, Norway]	Taken into account to some extent. The section has been rewritten.
8-532	8	14	31	14	33	The time variant GTP offers a more dynamical view of the problem but there is a transition issue past the target year, or one has to accept that the target year and climate objective changes over time. I think it is worth generalising these statements a bit. The time variant GTP is not the only metric that offer a more dynamical view. Most economically-based metrics (such as the GDP) do that as well. [Olivier Boucher, France]	Taken into account. The section has been rewritten.
8-533	8	14	32			Suggest to add "…various species over time and shows some properties of economic-based approaches" [Glen Peters, Norway]	Rejected. This paragraph has been rewritten
8-534	8	14	35	14	42	As a paper which stresses the value-laden aspects of metrics, Tanaka et al. (2010, Carbon Management) could be cited. But there may be more papers, which I am not aware of. [Katsumasa Tanaka, Switzerland]	Rejected. We do not give references here.
8-535	8	14	36	14	36	“more degrees of freedom”: I would say “larger uncertainty”. [Dirk Olivié, Norway]	Taken into account. Text rewritten
8-536	8	14	41			Drop "economic", as this applies for all metrics [Glen Peters, Norway]	Accepted as suggested.
8-537	8	14	44	14	44	SFP was designed for short-lived species only, rather than as a metric aimed to compare short-lived and long-lived species so I'm not sure how appropriate it is to discuss it here. [Olivier Boucher, France]	Accepted. SFP is not mentioned here now.
8-538	8	14	44	14	48	I think that the spatial variability of metrics is an important issue but still a detailed technical one, which does not fit well into the summary of status. [Katsumasa Tanaka, Switzerland]	Rejected since we think this is important and much more than just a technical one. The regional variations hidden behind global mean values is in our view an issue of fundamental importance.

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8-539	8	15	2	15	13	I think we can say a bit more than just there is no single best metric and the metric depends on the climate policy. There are some properties a good metric should have. For instance, it is important for a metric to be relatively robust (or policy-makers will argue forever on what is the best value) and to have a temporal behaviour that is continuous and makes sense for climate mitigation. GTP(t) is not the only metric that captures a temporal behaviour. [Olivier Boucher, France]	Taken into account to some extend. Will consider rewording.
8-540	8	15	5	15	9	The arguments against GWP and in favor of GTP are perhaps too policy prescriptive in this context. Also, the argument “the time invariant GWP is not well suited for a policy context with a global concentration, forcing or temperature target.” is in the text backed up with references, while the statement “GTP(t) is generally more suitable, especially in that it captures temporal behavior” is not backed up with reference. Please add references that back up the latter statement. [Daniel Johansson, Sweden]	Taken into account. Will reword.
8-541	8	15	7			It is stated that the time invariant GWP is not well suited… and then it is stated that GTP(t) is generally more suited. Please reconsider the wording here, the terminology is very close to being policy prescriptive. I would be more cautious, inparticular since it has been shown that even the thereotically perfect approach (when meeting a temperature target) only yields a rather small economic benefit over GWP (see Johansson et al 2006, and many others). In the real world, it would be difficult to calculate the correct theoretical trade off, so the benefit is probably even smaller. [Christian Azar, SWEDEN]	Taken into account. Will reword.
8-542	8	15	8	15	9	I think that AR5 should not promote a use of GTP in climate policies, given the status of discussion surrounding alternative metrics. Problems for GTP have also been raised -- for example, its negligence of the post-target temperature change (Johansson, 2011, Climatic Change) and its high sensitivity for SLFC. [Katsumasa Tanaka, Switzerland]	Taken into account. Will reword.
8-543	8	15	8	15	12	This doesn't seem to be referencable, and seems a bit too speculative for an IPCC report. I suggest deleting it. [Susan Solomon, USA]	There are several papers in the literature saying the time invariant GWP is not well suited for policies with RF or T targets. But we softened the wording regarding the GTP in the follwoing text.
8-544	8	15	8			"GTP(t) is generally", maybe "may be" is better than "is" [Glen Peters, Norway]	Accepted as suggested.
8-545	8	15	9			After the words "…it captures temporal behaviour." please, add the sentence "In principle GWP can also be presented as a function of time horizon t as GWP(t) instead of a fixed time horizon of 20, 100 or 500 years." [Ilkka Savolainen, Finland]	Rejected. We tried to include this earlier but due to space restrictions we had to leave this out.
8-546	8	15	14			Perhaps worth mentioning that multiple metrics can be used. For example, if comparing technology A with B, if A is better than B for all metrics and time horizons, then A is better for climate. But if B is better for some metrics, then more careful elaboration of the goals, time-scales, etc. is needed. [Glen Peters, Norway]	Rejected due to space restrictions
8-547	8	15	14			Perhaps mention that metric values can change for a variety of reasons (new knowledge, improved parameters, changing background, etc), and policy should allow for changes in metric values. For example, the GWP for Methane has changed from 21 to 23 to 25 and now higher estimates exist as well. Policy needs a way to incorporate these changes. [Glen Peters, Norway]	Taken into account elsewhere in the text.
8-548	8	15	19	15	35	The introduction can be shortened. Also, it might be better to clarify what species will be focued on in Section 8.2.For example aerosol simulations should not be a major issue here since they are described in Chapter 7. [Hong Liao, China]	Noted
8-549	8	15	19	23	11	At the beginning of this section, it would be helpful to state what will be covered in this section. When I first started reading through it, I wondered why there was nothing to put the overall radiative forcing of the various processes into perspective. Now I know that comes later. However, it would help if your goals for the section were clearly stated in section 8.2.1. [John Daniel, USA]	Noted
8-550	8	15	19		28	supporting reference may be useful [Muhammad Amjad, Pakistan]	Rejected: this is standard knowledge
8-551	8	15	19			Besdides CO2 seems strange as CO2 not discussed previously [Piers Forster, UK]	Taken into account: will be rewritten
8-552	8	15	21	15	22	The production for ODSs, for example, does not depend on environmental conditions. [John Daniel, USA]	Taken into account: add photolysis, light is listed as

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							environmental condition
8-553	8	15	22	15	24	Formation of aerosol (i.e., chemical formation of condensible compounds) should be included here - not just reactions on aerosol or in water [Oliver Wild, United Kingdom]	Noted: although aerosols are in Chapter 7, a few words will be included to mention this effect
8-554	8	15	25	15	25	"act on SOME chemical compounds" [John Daniel, USA]	Editorial
8-555	8	15	26	15	29	Remove sentence starting with "Overall,". This is what the whole chapter is on and is not specifically related to atmospheric chemistry and it doesn't fit with the rest of the introduction. [Robert Portmann, United States of America]	Taken into account: this is now removed
8-556	8	15	27	15	27	Also include influences on biosphere broadly (not just biogeochemical cycles). [Larry Horowitz, USA]	Noted
8-557	8	15	27	15	28	Point (4) - why only deposition on the cryosphere - what about ozone deposition impacts on the carbon cycle? [Katharine Law, France]	Rejected: this is included in the coupling with biogeochemistry
8-558	8	15	30	15	35	It might flow better if this information came before most of the information in the previous paragraph [John Daniel, USA]	Noted
8-559	8	15	35	15	35	is there not any better reference than Raes et al (2010) to make the point of non-linearities? [Olivier Boucher, France]	Noted
8-560	8	15	37	17	3	This section deals with the ACCMIP simulations - it is true that uncertainty in emissions has not been taken into account (page 16, line 7) but it could following approaches discussed in a recent paper by Wild et al. (2012), ACPD. The were run for timeslices but have not, in my opinion, undergone, "extensive model evaluation" (line 26, page 16). This a rather broad brush statement and should be avoided. Relevant references for each model could be added to Table 8.1 with regard to evaluation of each models' performance. [Katharine Law, France]	Taken into account: relevant references will be included as they become available
8-561	8	15	37			Section 8.2.2: what happened? Where are the model intercomparison results? [Robert Waterland, United States of America]	Taken into account: those papers are not submitted yet
8-562	8	15	43	15	43	Replace "As for the CMIP5 models" with "As illustrated by the Coupled Model Intercomparison Project 5 (CMIP5) models". [Robert Waterland, United States of America]	Editorial
8-563	8	15	43	15	46	Change "models differ in their" to "models differ not only in their". Also, this sentence is a run-on and should be re-worded. [Robert Portmann, United States of America]	Editorial
8-564	8	15	43	15	50	There exist some global modeling studies on atmospheric chemistry, which were not ACCMIP runs but should be summarized whenever possible. [Hong Liao, China]	Taken into account - included in Table 8.1
8-565	8	15	46	15	46	Replace "models" with "modelling groups". [Robert Waterland, United States of America]	Editorial
8-566	8	15	52	16	7	I do not understand the focus on biomass burning here. Wouldn't this discussion also apply to most other forcing agents prescribed in the RCP scenarios? [John Daniel, USA]	Rejected:biomass burning is one element we know for sure is not taken into account at all, and this is therefore different than other forcing agents
8-567	8	15				GTP does not capture a temporal bahavior. GTP is an end-point index, not an integrated index. [Katsumasa Tanaka, Switzerland]	Noted
8-568	8	16	1	16	1	Fig. 8.6 only shows BC emissions, but text says “anthropogenic emissions” [Helen Worden, USA]	Rejected: fig 8.6is now removed
8-569	8	16	7	16	7	Delete "historical", since discussion concerns both historical and projected future emissions [Larry Horowitz, USA]	Taken into account: this is now removed
8-570	8	16	10	16	12	How much detail will be provided concerning the chemistry in the models? Tropospheric/stratospheric chemistry? Aerosols? Coupling between gas and aerosol schemes? Number of species/reactions? [Larry Horowitz, USA]	Rejected: very limited space is available and instead references to ACCMIP publications will be used

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8-571	8	16	10	16	12	Table 8.1 not complete [Helen Worden, USA]	Accepted:, ACCMIP results are still coming in
8-572	8	16	15	16	16	About Figure 8.5. No indication of the unit related to the variable represented in the vertical axis is given. [Rubén D Piacentini, Argentina]	Taken into account - figure updated
8-573	8	16	15	16	16	About Figure 8.6.No indication of the unit related to the variable represented in the vertical axis is given.. [Rubén D Piacentini, Argentina]	Taken into account - figure deleted
8-574	8	16	21	8	23	Is there evidence for a global tropospheric ozone increase over the last decades, or just locally? [Susan Solomon, USA]	Taken into account: qualifieris added
8-575	8	16	22	16	22	Identify the different inventories represented by the black dots. [John Daniel, USA]	Rejected: figure is now removed
8-576	8	16	24	17	3	I don't think this chapter should describe the ACCMIP process, just assess its results as an indpendent bit of research, will this section say anything about forcing, or just emissions? [Piers Forster, UK]	Noted: the description was following approaches in previous A, but text will reflect this review
8-577	8	16	28	16	29	You may wish to define what chemistry-transport models are and how the CMIP5 models differ from them [John Daniel, USA]	Taken into account -description added
8-578	8	16				Not surprisingly, I have sympathy for documenting important model intercomparisons. However, it would be good to harmonize such documentation across the report and chapters. While ACCMIP is rightly explained, little is found on AeroCom. A lot of effort from several groups went into producing AeroCom results in chapters 7 and 8 and this should be traceable to my opinion. [Michael Schulz, Norway]	Rejected: AeroCOM should be in Chapter 7, not 8.
8-579	8	16				Table 8.1: This list can be expanded with the models that recently submitted data to the ACCMIP ozone and methane RF experiment led by David Stevenson. [Twan Van Noije, Netherlands]	Taken into account: list was revised according to the models used for this analysis
8-580	8	17	1		22	this text isn't entirely consistent with section 2.4.2.1, which suggests past trends in tropospheric ozone are not as clear cut. [Stephen Montzka, USA]	Taken into account: text is now made consistent with Chapter 2
8-581	8	17	12	17	12	Prefer "obtained" over "attained" here. [Larry Horowitz, USA]	Accepted - text revised as suggested
8-582	8	17	12	17	12	Replace "know" with "estimate". [Robert Waterland, United States of America]	Accepted - text revised as suggested
8-583	8	17	17	17	19	Please use original references when discussing photochemical production and loss processes! [Katharine Law, France]	Noted: references will be changed
8-584	8	17	20	17	28	It would be useful here to refer to the ozone section in Chapter 2 and make sure the messages are consistent. [Katharine Law, France]	Noted: text will be checked against chapter 2
8-585	8	17	22	17	22	Change to "The major loss pathway for ozone is through its photolysis …" [Larry Horowitz, USA]	Editorial
8-586	8	17	22	17	22	Replace "Its major loss pathway is through" with "Ozone's major loss pathway is via". [Robert Waterland, United States of America]	Editorial
8-587	8	17	22			Ozone loss through O3+HO2 is as important as the O(1D) + H2O reaction in the troposphere [Terje Berntsen, Norway]	Noted:statement will be checked for next draft
8-588	8	17	24	17	26	About the sentence: "Observed surface ozone abundances typically range from less then 10 ppb over the remote tropical oceans to more than 100 ppb downwind of highly polluted regions". Please, verify if it is "less then 10 ppb" or "less than 10 ppb". [Rubén D Piacentini, Argentina]	Editorial: typo is corrected
8-589	8	17	24	17	28	You need references for these statements about the abundance and lifeime of ozone. [Robert Waterland, United States of America]	Taken into account: references are added
8-590	8	17	25	17	25	More specifically, surface ozone minimizes over the tropical Pacific. [Olaf Morgenstern, New Zealand]	Noted
8-591	8	17	26	17	28	Replace "Its residence time in the troposphere varies strongly with season and location. It can be as little as one day in the boundary layer to several weeks in the remote atmosphere, leading to a global estimated lifetime of approximately 25 days." with "The residence time of O3 in the troposphere varies strongly with	Taken into account - text revised as suggested

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						season and location: it may be as little as 1 day in the tropical boundary layer, or as much as 1 year in the upper troposphere. The global mean lifetime of ozone is approximately 25 days." [Robert Waterland, United States of America]	
8-592	8	17	27	8	27	global mean [David Stevenson, UK]	Taken into account - text revised as suggested
8-593	8	17	28	17	28	Can you give a reference for the 25 days residence time [Guus Velders, Netherlands]	Taken into account: references are added
8-594	8	17	30	17	30	Replace "For conditions relevant to the recent decade" with "For present conditions". [Robert Waterland, United States of America]	Taken into account - text revised as suggested
8-595	8	17	30	18	15	This section will obviously be re-visited once the analysis of the ACCMIP results are finished and published. However, a range of estimates will be needed in Figure 8.7 for the different budget terms. In addition, I suggest that the authors need to decide what they mean by "a reasonable representation" given that biases in some regions are as much as 40% in the ensemble mean which is presented. Also, the correlations need to be explained. How can they be equal to 1.0 - there must be systematic offsets? This needs to be explained. In any case, such mean correlations hide a lot of variability giving a more favourable picture of the model performance than is probably the case. [Katharine Law, France]	Rejected: fig 8.7 is now removed
8-596	8	17	31	17	31	Replace "simulations since AR4" with "simulations conducted since AR4". [Robert Waterland, United States of America]	Accepted. Text revised as suggested
8-597	8	17	32			Add multi-model results from Stevenson et al. 2006 as baseline [Ruth Doherty, UK]	Taken into account - text revised as suggested
8-598	8	17	33	17	33	Please check "32" value. I calculate that 27 DU is equivalent to 300 Tg [John Daniel, USA]	Accepted: this was checked and corrected
8-599	8	17	33	17	33	The 300 Tg is recorded as 330 Tg in Figure 8.7. I realize that you say the numbers are currently only approximate [John Daniel, USA]	Accepted: changed to 330
8-600	8	17	33	17	33	Doesn't it say 330 Tg in Figure 8.7? [Robert Waterland, United States of America]	Accepted: changed to 330
8-601	8	17	33	17	33	300+/-50 Tg is a rather low estimate given the results shown in Table 8.3, where the burdens from the 7 new studies average 330 Tg. Note also that the Dobson Unit conversion here is incorrect (330 Tg is just over 30 DU) [Oliver Wild, United Kingdom]	Accepted: changed to 330
8-602	8	17	34	17	35	Need a reference and more discussion for issues of tropopause definition. [Larry Horowitz, USA]	Noted
8-603	8	17	35	17	35	Differences in the definition of the tropopause introduce differences in ozone burden of the order of 10%, see Table 2 of Wild 2007 [Oliver Wild, United Kingdom]	Taken into account: text was added accordingly
8-604	8	17	35	17	36	Replace "The global annual tropospheric ozone burden estimate has not significantly changed since the ACCENT-AR4 estimates" with "The model estimate of global annual tropospheric ozone burden has not significantly changed since the ACCENT-AR4 estimates" [Robert Waterland, United States of America]	Editorial
8-605	8	17	36	8	36	You use 'ACCENT-AR4' to refer to results from ACCENT PhotoComp (Experiment 2) model intercomparison. Whilst the ACCENT intercomparison was quoted in AR4, it was, of course, not directly linked to AR4.This may be a handy shorthand, but it should be clarifi [David Stevenson, UK]	Taken into account: text was changed accordingly
8-606	8	17	40	17	40	Table 8.3: Give the units [Guus Velders, Netherlands]	Taken into account - revised as suggested
8-607	8	17	41	17	43	Probably there should be a column here giving the name of the model / dataset which the calculation is based on. [Olaf Morgenstern, New Zealand]	Rejected: this is considered not useful
8-608	8	17	41	17	44	I am biased, but I think the O3 budgets from Stevenson et al. (2006) should feature in Table 8.3, as this remains the largest coherent multi-model analysis (other studies are either single/few models, or didn't run a specified experiment, so will inherent [David Stevenson, UK]	Taken into account: text was added
8-609	8	17	41			Summary of model and observations of tropospheric ozone budget estimates for the year 2000 conditions. [Pieter Aucamp, South Africa]	Taken into account: qualifier is added

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8-610	8	17				Nice to see obs and models compared [Piers Forster, UK]	Noted
8-611	8	17				Table 8.3: The article by Huijnen et al. (The global chemistry transport model TM5: description and evaluation of the tropospheric chemistry version 3.0, Geosci. Model Dev., 3, 445-473, 2010) gives the tropospheric ozone budget for another state-of-the-art global model, published since AR4. Please include these numbers (see Table 11 in the paper). [Twan Van Noije, Netherlands]	Taken into account:text & reference added
8-612	8	17				Table 8.3: given the weaknesses in some of the early ozone budget assessments, it might be more appropriate to present the summary of studies since 2000 ("post-2000", 17 studies) from Wild 2007 rather than the summary of all 33 studies presented in the table. [Oliver Wild, United Kingdom]	Taken into account: table was redone accordingly
8-613	8	18	4	18	4	Replace "approximative" with "approximate" in the caption of Figure 8.7 [Robert Waterland, United States of America]	Editorial: typo is corrected
8-614	8	18	7	18	9	This chapter is about radiative forcing, so I would suggest removing figures (Figs. 8.8 and 8.9) on model evaluation. The model performance can be described with references. [Hong Liao, China]	Rjected this is the only place model evaluation for chemistry can be handled. Only one figure is kept.
8-615	8	18	11	18	13	The ACCENT-AR4 simulations were also carried out with common datasets for anthropogenic and biomass burning emissions. The explanation is more likely that the presented analysis is based on a relatively small number of models, namely the ACCMIP models included in Table 8.1. The ACCENT model intercomparison was based on a much larger model ensemble. [Twan Van Noije, Netherlands]	Taken into account: text was a misstatement
8-616	8	18	11	18	13	The ACCENT-AR4 simulations also used common emission datasets (although in practice there may have been some minor deviations from these), see Stevenson et al., 2006. Does the reduced spread reflect the smaller number of models involved, or reduced model diversity as characterised by removal of outliers evident in Figs 8 and 9 in Stevenson et al., 2006? [Oliver Wild, United Kingdom]	Taken into account: text was a misstatement
8-617	8	18	12	18	12	most likely coming from the use of common anthropogenic and biomass burning emission datasets'. This seems unlikely, as the ACCENT-AR4 results came from models using specified anthropogenic emissions, and recommended values for biomass burning and natural [David Stevenson, UK]	Taken into account: text was a misstatement
8-618	8	18	12			smaller than ACCENT-AR4; quantify or add to Table 8.3 [Ruth Doherty, UK]	Taken into account: table was redone accordingly
8-619	8	18	13			Additional aspects of? And do you mean differences in natural emissions? [Ruth Doherty, UK]	Taken into account: qualifier is added
8-620	8	18	18	18	19	About Figure 8.8. It must be indicated if "ozone" is actually "tropospheric ozone".Also these words (tropospheric ozone) must be added to the vertical axis indication, since only the unit is indicated (ppbv), without the variable. [Rubén D Piacentini, Argentina]	Rejected: tropospheric is by definition for pressure > 250 hPa
8-621	8	18	22	18	23	About "Figure 8.9: Comparison of ACCMIP ensemble mean (second column) with observations (left column). Bias (in %) and correlation are shown in columns 3 and 4".The chemical substance must be indicated, for example, by adding "tropospheric ozone" to the end of the first sentence: "... with observations of tropospheric ozone (left column)". [Rubén D Piacentini, Argentina]	Rejected: figure is now removed
8-622	8	18	27	18	27	Change "continuous" to "continual" [Larry Horowitz, USA]	Editorial: typo is corrected
8-623	8	18	27	18	28	Make it clear you are talking about inreases in models. [John Daniel, USA]	Taken into account: text was changed accordingly
8-624	8	18	30	18	31	Stevenson et al (2006) Table 5 reports ozone budgets of: deposition 953+/-154 Tg/yr (subset of models) or 1002+/-200 Tg/yr (all models); and stratospheric influx of 556+/-154 Tg/yr (subset of models) or 552+/-168 Tg/yr (all models). I think these are the [David Stevenson, UK]	Taken into account: it was the wrong reference (should be Wild). Table will be revised.
8-625	8	18	30	18	33	The central estimate of 636 Tg/yr is really on the high side of the observational estimates. According to Gettelman et al. the best estimate for the flux at 100 hPa is 510 Tg/yr with a range of 450-590 Tg/yr. Please clarify this issue. [Twan Van Noije, Netherlands]	Taken into accout: the flux at 100 hPa might not be complete representation of STE. Numbers have been changed by focusing on post-2000 estimates from Wild
8-626	8	18	33	18	35	Quantify. Also give estimates for pre-industrial gross P and gross L [Larry Horowitz, USA]	Taken into account: numbers will be coming from

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							ACCMIP
8-627	8	18	37	18	41	It can also be mentioned that modelsrun with time dependent emissions from 1980 to 2000 have problems reproducing observed ozone trends. This discussion can be linked to Chapter 2 on observational changes. [Katharine Law, France]	Taken into account: text was added
8-628	8	18	38			Figure 8.10- add to caption that ACCENT AR4 is Stevenson et al, 2006 [Ruth Doherty, UK]	Taken into account: text was changed accordingly
8-629	8	18	38			Table 8.4 A-F need explaining [Ruth Doherty, UK]	Taken into account: these indicate various models,and this infornation is not included
8-630	8	18	40			the text regarding "ozone fields" is not that clear, does this mean the models show the same sign and direction of change? [Ruth Doherty, UK]	Taken into account: sentence was rewritten to be clearer
8-631	8	18	42	18	42	May I suggest that the authors consider a brief point on the potential for O3 to exert additional indirect radiative forcing via impacts on land carbon sinks (Sitch et al, 2007b in reference list), ie: this may affect the airborne fraction of CO2 - I think this should be mentioned here and not just buried in he "open questions" section. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Taken into account: the section on "open questions" is removed and information such as mentioned will be included as much as possible
8-632	8	18	44	18	44	Define how "tropospheric ozone column" is calculated here (i.e., what tropopause definition) [Larry Horowitz, USA]	Taken into account: added defintion
8-633	8	18	44	18	45	I would replace "A", "B", "C" etc. with explicit model names. [Olaf Morgenstern, New Zealand]	Noted
8-634	8	18	44	19	1	Are models A-F defined somewhere? Why not report the model names in this table so those familiar with them can understand their differences? [Susan Anenberg, USA]	Noted
8-635	8	18	44	19	1	The standard deviation seems to me to not encompass the true uncertainty in this quantity. Model agreement in this case likely results from similar inputs that are not known at that precision. This should be noted in text. [Robert Portmann, United States of America]	Noted: this is correct, it is only a standard deviation amd not an implication on true uncertainty.
8-636	8	18				Table 8.4: Please indicate the model names in the table. I assume that in a later stage all models that contributed to the ACCMIP O3/CH4 RF experiment led by David Stevenson will be included. [Twan Van Noije, Netherlands]	Noted
8-637	8	19	4	19	5	About "Figure 8.10: Time evolution of tropospheric ozone column (in DU) from 1850 to 2005 from ACCMIP results and Kawase et al. (2011)". It must be indicated if the results are for all the planet, ie, including the word "global" in this sentence: "Time evolution of global tropospheric ozone column......" [Rubén D Piacentini, Argentina]	Taken into account: text was added accordingly
8-638	8	19	10		15	the more significant (and better characterized) natural emissions are for CH3Cl and CH3Br--it would seem that these are worth mentioning here instead of or in addition to the 4-8 ppt of inorganic bromine potentially contributed by short-lived chemicals. [Stephen Montzka, USA]	Taken into account: text was added accordingly
8-639	8	19	11	19	12	This sentence ignores nitrous oxide's contribution to stratospheric ozone loss. [Robert Waterland, United States of America]	Taken into account: text was changed accordingly
8-640	8	19	12	19	15	You do not mention the primarily natural emissions of CH3Cl. This compound contributes significant chlorine to the stratosphere. [John Daniel, USA]	Taken into account: text was added accordingly
8-641	8	19	12	19	15	This sentence gives the impression that the natural emission is only of bromine, while it is the natural chlorine emission that play a larger role in the ozone budget. [Robert Portmann, United States of America]	Taken into account: text was changed accordingly
8-642	8	19	15	19	15	“WMO, 2011” while “WMO, 2010” in the references list. Also on other places. [Dirk Olivié, Norway]	Editorial: typo is corrected
8-643	8	19	16	19	18	This should be reworded. There are still significant levels of chlorine and bromine in the stratosphere, but the current ODP-weighted N2O emissions are still larger than any other ODP-weighted ODS emission. [John Daniel, USA]	Taken into account: this section is rewritten

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8-644	8	19	16	19	18	Ravishankara et al. state that present-day N2O emissions dominate all other emissions in their ozone depletion potential. In this sense N2O is the leading ozone depleting agent. This reflects the success of the Montreal Protocol under which emissions of other substances have been cut sharply. However, chlorine and bromine continue to be the leading causes of anthropogenic ozone depletion because of historical emissions. [Olaf Morgenstern, New Zealand]	Taken into account: this section is rewritten
8-645	8	19	16	19	18	This statement understates the conclusion of Ravishankara et al. 2009. In fact, anthropogenic N2O became the largest contributor to ozone loss even with significant anthropogenic halogen emissions. [Robert Portmann, United States of America]	Taken into account: this section is rewritten
8-646	8	19	16		18	The main point of the Ravishankara paper was not what is mentioned in the text, but that when weighted by ODP, N2O emissions are currently larger than emissions of ODSs… [Stephen Montzka, USA]	Taken into account: this section is rewritten
8-647	8	19	19	19	19	Odd nitrogen is involved in at least two ozone depletion cycles (e.g., Lee et al., JGR, 107, D11, 2002) and plays a key role in polar PSC chemistry (PSCs partly consist of odd nitrogen). [Olaf Morgenstern, New Zealand]	Taken into account: this section is rewritten
8-648	8	19	21	19	23	You should not neglect mid-latitude depletion since that will also affect radiative forcing. [John Daniel, USA]	Taken into account: text was changed accordingly
8-649	8	19	23	19	23	There are quite a few more papers making this point, and strong model evidence. A review was published by Thompson et al., Nature Geosci., 4, 741-749, doi:10.1038/NGEO1296, 2011. [Olaf Morgenstern, New Zealand]	Taken into account: references are added
8-650	8	19	26	19	26	Salby et al., GRL, 2011, suggest that Antarctic ozone recovery (as opposed to just stabilization) is now discernible, albeit only with a sophisticated analysis. I think this paper needs to be mentioned here. Please also cite Maeder et al., ACP, 10, 12161-12171, 2010. [Olaf Morgenstern, New Zealand]	Taken into account: clarification added
8-651	8	19	28	19	28	Please explain the acronym "CCMVal". [Olaf Morgenstern, New Zealand]	Taken into account: text was added accordingly
8-652	8	19	28			explain CCMval- could add website link [Ruth Doherty, UK]	Taken into account: text was changed accordingly (not web site)
8-653	8	19	29	19	29	“2032”: or I would give uncertainty estimates around this year, or I would write “around 2030”. [Dirk Olivié, Norway]	Rejected: this is stated as multi-model mean
8-654	8	19	31	19	31	Increased circulation is only certain in the modeling world. Do not be too strong with the wording here. [John Daniel, USA]	Rejected: this is only talking about combination would lead to stronger stratosphere-troposphere exchange, which is much more established.
8-655	8	19	32	19	32	Zeng et al., GRL, 2010, can be cited here too. [Olaf Morgenstern, New Zealand]	Taken into account: reference is added
8-656	8	19	33	19	34	How many of the ACCMIP models include stratospheric halogen chemistry? [Katharine Law, France]	Noted: more than 80%
8-657	8	19	36	19	43	A word about the discrepancy in trends between the Boulder stratospheric water vapor series and satellite records would be good here. In particular whether this discrepancy still exists or has been resolved. [Olaf Morgenstern, New Zealand]	Noted: needs further evaluation
8-658	8	19	36	19	43	The story is considerably more complex than this. See section 2.4.2.3 of Chapter 2. [Robert Waterland, United States of America]	Noted: text will be checked against chapter 2
8-659	8	19	38	19	41	"Consequently" should be removed from this sentence since the causes for stratospheric H2O changes are not understood (a least the significant non-methane component). [Robert Portmann, United States of America]	Taken into account: text was removed
8-660	8	19	40			Wording, seems that "the stratospheric" should be removed. [Terje Berntsen, Norway]	Editorial
8-661	8	19	42			Not consistent with what is said on page 8-2, line 52 [Henning Rodhe, Sweden]	Noted: text will be checked in next draft
8-662	8	19	47	20	53	My interpretetaion of the methane section is that we don't know exactly why the methane trend first decreased	Taken into account: text was changed accordingly

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						and then started to increase again about 5 years ago. It could be either changes in emissions or losses or both. This could be more clearly stated at the end of the section. [Terje Berntsen, Norway]	
8-663	8	19	47	20	53	One of the particularly useful aspects of AR4 were the budget tables for CH4, N2O, NOX, etc. Please consider including such tables with updates included where available, unless this will appear in another chapter. [John Daniel, USA]	Rejected: not enough space. Such tables will most likely be in Annex 2.
8-664	8	19	50	19	50	About the expression: "(Figure X in Chapter 2)", do not forget to replace the X symbol by the corresponding number. [Rubén D Piacentini, Argentina]	Noted: text will be checked against chapter 2
8-665	8	19	50	19	50	Replace "are indicating" with "indicate". [Robert Waterland, United States of America]	Editorial: typo is corrected
8-666	8	19	50			reference made to a figure in Chapter 2 showing methane projections to 2100--I don't see any such projections in a chapter 2 figure. [Stephen Montzka, USA]	Taken into account: this will be included in next draft of Ch 8.
8-667	8	19	51	19	51	The CH4 concentration has continued to increase and has not been steady - the growth rate has been generally positive although slower or negative for short periods (see Chapter 2). [Katharine Law, France]	Taken into account: text has been harmonized with chapter 2 and 6
8-668	8	20	1	20	1	Add "Recent trends are discussed in Chapter 6" [Larry Horowitz, USA]	Taken into account: text has been harmonized with chapter 2 and 6
8-669	8	20	1	20	53	The extensive description of methane sources here using Bergamschi (2009) is inconsistent with the statement in Chapter 2 for methane that satellite methane from SCIAMACHY is not precise enough. It should be checked that statements are more consistent. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been harmonized with chapter 2 and 6
8-670	8	20	4	20	4	Source and sink terms for the CH4 budget have been summarised in Table 6.7 for the past 3 decades. For the 2000-2009 decade, they have estimates of wetland emissions of 159-184 TgCH4/year from a top-down approach and 174-280 TgCH4/year from bottom-up estimates. Emissions quoted here should be consistent with those in Chapter 6. [Fiona O'Connor, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been harmonized with chapter 2 and 6
8-671	8	20	4	20	7	Replace "Natural emissions come primarily from wetlands with an amplitude of 150–180 Tg yr-1 (Bergamaschi et al., 2009; Bousquet et al., 2006), which respond to climate through variations in temperature and water table. While present-day emissions are dominated by the tropics, the potential melting of the permafrost" with "Current natural emissions - amounting to 150-180 Tg yr-1 - come primarily from wetlands, predominantly from the tropics (Bergamaschi et al., 2009; Bousquet et al., 2006). Wetland emissions respond to variations in temperature and water table. Any substantial melting of the permafrost". [Robert Waterland, United States of America]	Editorial
8-672	8	20	4	20	11	In chapter 6, they also discuss geologic sources of methane and following a synthesis by Etiope et al. (2008), the magnitude of geologic sources is larger than previously thought. [Fiona O'Connor, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been harmonized with chapter 6
8-673	8	20	4	20	20	The numbers in this paragraph (9 years for CH4 lifetime and 32% and 25% increase for pulse lifetime) give pulse lifetimes of 11.8 and 11.2 years. Yet in Table 8.10 the CH4 pulse lifetime is given as 13.7 years. Why such a big difference? [Robert Portmann, United States of America]	Rejected: this is tropospheric lifetime only.
8-674	8	20	4		53	it would seem important to ground this discussion by indicating that the global mean OH concentration is derived independently from an analysis of methyl chloroform observations. As a result, OH calculated by models can be tested for accuracy independent of uncertainties in the methane budget. As a result, new findings about OH (lines 46-53) are interesting but will likely not substantially alter our understanding of the methane lifetime... [Stephen Montzka, USA]	Taken into account: text was added accordingly
8-675	8	20	8	20	8	I think you are incorrectly representing the Jung paper. It shows no obvious change in ET trend for high latitudes in the NH. [Robert Waterland, United States of America]	Noted: this will be checked for the next draft
8-676	8	20	8			explain what "permafrost drying" means. [Ruth Doherty, UK]	Noted: this will be clarified
8-677	8	20	9	20	11	What are the relative contributions of the different CH4 sources? This is important information for the sector-	Noted: this information will most likely be in Annex 2

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						specific focus introduced later in the chapter. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	
8-678	8	20	13	20	13	There is a potential for misunderstanding here. 1st, there is some spread in the assessment of methane chemical lifetime (M. Prather, e.g., seems to suggest a CH4 tropospheric chemical lifetime with respect to OH of ~10.5 years and reaction with O(1D) and Cl in the troposphere does not contribute significantly), 2nd the atmospheric lifetime is actually a result of chemical lifetime plus soil deposition plus stratospheric methane destruction which shaves off approximatly a year of the tropospheric chemical lifetime. Finally, sometimes chemical and total atmospheric lifetimes are confused with the methane perturbation lifetime which is on the order of 12 years. I think a sdisambiguation at that point would help. [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Rejected: the numbers only refere to the tropospheric OH loss, as indicated in the text
8-679	8	20	15	20	15	Isotope-resolving measurements of CH4 suggest that there could be a roughly 4% sink of CH4 due to CH4 + Cl in the marine PBL; this reaction has a large kinetic isotope effect (Allen et al., JGR, 112, D04306, 2007). [Olaf Morgenstern, New Zealand]	Taken into account: reference is added
8-680	8	20	15	20	40	The OH feedback estimates are not consistent with the section in Chapter 2 about OH trends suggesting no trend. It should also be mentioned that the ACCMIP models (I believe) were run using fixed CH4 concentrations at the surface and a comment should be added to explain the affect this might have on their results. Figure 8.11 shows large differences and not results which "vary quite widely" as stated in the text (line28). Given that models have difficulty reproducing observed ozone trends over the last 2 decades, what confidence does that give in their ability to model the CH4-OH feedback? [Katharine Law, France]	Rejected: there is more to OH trends than just methane.It is true that all ACCMIP models were run with observed CH4 as boundary condition. The strength of the OH-CH4 feedback is independent of the ozone trend.
8-681	8	20	18	20	20	Add "Thus, a xx% increase in CH4 emissions would, after feedback, result in a yy% increase in CH4 concentrations" [Larry Horowitz, USA]	Taken into account: text was added
8-682	8	20	23	20	23	recent variations -> recent observed variations (highlight observations here to contrast with surrounding paragraphs) [Oliver Wild, United Kingdom]	Editorial
8-683	8	20	24	20	25	Add a remark and reference for isotopic constraints on methane lifetime in recent decades that suggests a methane lifetime decrease as also shown by (some?) models in figure 8.11 : Monteil, G., Houweling, S., Dlugockenky, E. J., Maenhout, G., Vaughn, B. H., White, J. W. C., and Rockmann, T.: Interpreting methane variations in the past two decades using measurements of CH4 mixing ratio and isotopic composition, Atmos. Chem. Phys., 11, 9141-9153, doi:10.5194/acp-11-9141-2011, 2011. [Michiel van Weele, The Netherlands]	Taken into account: reference and discussion added.
8-684	8	20	27	20	40	Global and annual average OH concentrations are also obtained from observed changes in methyl chloroform concentration. I think you should include this approach in this paragraph. [Robert Waterland, United States of America]	Taken into account: text was added accordingly
8-685	8	20	27		40	issues regarding this paragraph: the uncertainty in the methane lifetime with respect to OH hinges primarily on our understanding of OH and of the OH + methane rate constant, and less, I would think, on the distribution and variability of natural sources of methane. Also, photodissociation of O3 is an important source of OH, yet on a global scale as much as 50% of OH is formed through secondary reactions--see Lelieveld et al., 2004 (ACP). [Stephen Montzka, USA]	Taken into account: clarificationis added
8-686	8	20	29			could add accent-AR4 ranges [Ruth Doherty, UK]	Taken into account: range added
8-687	8	20	30	20	31	Give an older reference for such a well-known process, this makes it seem this is a new result. [Robert Portmann, United States of America]	Taken into account: reference is changed
8-688	8	20	30	20	33	Mention "recycling" of OH by Nox [Larry Horowitz, USA]	Taken into account: text was added accordingly
8-689	8	20	31	20	31	Wennberg (2006) is not an original reference for this process. [Katharine Law, France]	Taken into account: reference is changed
8-690	8	20	32	20	35	It is argued that the main atmospheric OH sink are reaction with methane and CO. But what about biogenic VOCs? I would argue that the approximately 1 Pg of carbon emitted as VOC (predominantly isoprenoids but also methanol and acetone) have a larger impact on OH than CO unless only the anthropogenic influence is discussed here. [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Noted: additional calculations will be made(before the SOD) to prove or disprove this argument.

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8-691	8	20	35	20	40	Note sensitivity to other parameters here, particularly humidity, cloud cover and solar radiation [Oliver Wild, United Kingdom]	Taken into account: text was changed
8-692	8	20	43	20	44	Observations should be contrasted with this figure. The recent lifetime behavior seems inconcsistent with the Montzka work (Science, 2011). [John Daniel, USA]	Taken into account: text was added to discuss recent trends
8-693	8	20	46	20	53	The OH recycling idea is largely based on measurement campaigns that took place close (in relative terms) to the ground at tropical rain forest sites. Given the fact that the BVOC chemistry involved plays on rather short lifetimes and that the measurements were made in close proximity to a rather complex environment (with huge surface areas for heterogeneous processes and a complex micro-meteorology including some small-scale convection and also substantial water vapour fluxes, all substantially affection the OH balance) can we feel confident that OH recycling is a purely chemical effect? And can we freely extrapolate from such a specific environment to the whole of the troposphere that easily? [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Noted: this is very much a research topic and we can only discuss and assess existing literature, not conjectures.Additional references are added to broaden the discussion.
8-694	8	20	48	20	49	We don't yet understand how important these high-isoprene low-NOx conditions are on a global scale; should highlight that the wider importance of these chemical uncertainties remains unclear [Oliver Wild, United Kingdom]	Noted: this is very much a research topic.
8-695	8	20	48	20	51	The discussion of the underestimated OH should also definitely cite the study of Butler et al. (2008), which went beyond Lelieveld et al. (2008) in a more detailed analysis of the degree of OH recycling which would be necessary in the isoprene oxidation mechanism in order to acheive an agreement with the observed OH levels. Citation: Butler, T. M., Taraborrelli, D., Brühl, C., Fischer, H., Harder, H., Martinez, M., Williams, J., Lawrence, M. G., and Lelieveld, J., Improved simulation of isoprene oxidation chemistry with the ECHAM5/MESSy chemistry-climate model: lessons from the GABRIEL airborne field campaign, Atmos. Chem. Phys., 8, 4529-4546, 2008. [Mark Lawrence, Germany]	Taken into account: reference Is added.
8-696	8	20	53	20	53	What about the possible role of halogens in tropospheric CH4 oxidation? [Katharine Law, France]	Taken into account: it does not seem to influence much (Saiz-Lopexz et al 2011) and text will be changed to reflect this information
8-697	8	20	55	22	2	I don't understand the sections on N2O and ODSs? What purpose do they serve - there are no comparisons with model results or placeholders to do so? They are already discussed in these terms in Chapter 2. Liekwise for the aerosol section - aerosols have their own chapter and the CMIP5 results are discussed in Chapter 12. However, it would make more sense to include the discussion about aerosol burdens and changes with time, comparison with observations in Chapter 8. [Katharine Law, France]	Taken into account: this section is rewritten
8-698	8	20				Section 8.2.3.3: consistency needed with chapter 2, section 2.4.2.4 and Chapter 6, section 6.3.3.3 [Michiel van Weele, The Netherlands]	Taken into account: text has been harmonized with chapter 2 and 6
8-699	8	21	6	21	6	Add discussion of recent trends in concentrations (and estimated emissions) [Larry Horowitz, USA]	Taken into account: text was added
8-700	8	21	8	21	18	Please cite an discuss Velders et al., PNAS, 2007 and 2009 here. Velders et al state that the Montreal Protocol was actually more effective at curbing global warming than the Kyoto Protocol, and that with umimpeded growth HFCs would substantially offset any progress in mitigating CO2. [Olaf Morgenstern, New Zealand]	Rejected: this is not the place for this discussion
8-701	8	21	10	21	10	"Stratospheric ozone depletion" would be preferable to "Stratospheric ozone hole" [John Daniel, USA]	Editorial: text is changed
8-702	8	21	10	21	10	Change to "ODSs, as stratospheric ozone depletion is …." [Larry Horowitz, USA]	Editorial: text is changed
8-703	8	21	10	21	11	Th ozone hole is the largest manifestation of ozone depletion, but not necessarily the most signiicant environmental impact. The smaller decreases in ozone at other latitudes probably affect human s and ecosystems more. Please rephrase. [Guus Velders, Netherlands]	Taken into account: text was added accordingly
8-704	8	21	10		1	seems subjective (and unnecessary) to state that ozone depletion is the most signiifcant environmental impact of ODSs. [Stephen Montzka, USA]	Rejected: this is the justification for the terminology of ozone-depleting substances.

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8-705	8	21	12	21	12	CH3Br does indeed have natural emissions [John Daniel, USA]	Rejected: this is why "most" is used
8-706	8	21	12	21	12	Methyl bromide actually is about half of natural origin, as is indicated by a substantial preindustrial abundance. [Olaf Morgenstern, New Zealand]	Rejected: this is why "most" is used
8-707	8	21	12	21	12	CH3Br is mostly from natural origin. Remove this compound from this list. [Guus Velders, Netherlands]	Rejected: this is why "most" is used
8-708	8	21	12		12	methyl bromide should be removed from the parenthetic list of primary ODSs that do not have natural emissions [Stephen Montzka, USA]	Rejected: this is why "most" is used
8-709	8	21	13	21	14	Give the number for the peak emission (about 9.4 GtCO2) [Guus Velders, Netherlands]	Taken into account: text was added
8-710	8	21	15	15	46	Use the correct citation for the WMO 2010 Scientific Assessment of Ozone Depletion. It is NOT WMO 2011 or UNEP 2011 [Pieter Aucamp, South Africa]	Taken into account: this was a problem with referencing reports.
8-711	8	21	15		15	the main loss of CH3CCl3 is not photolysis, it is oxidation by OH [Stephen Montzka, USA]	Taken into account: text was changed accordingly
8-712	8	21	18		18	the Montzka et al., 2010 citation is a paper on HFC-23, a chemical not used primarily as a substitute for ODSs. A better citation would be Montzka et al., GRL, 2009, if a citation in addition to the WMO Ozone assessment was desired. [Stephen Montzka, USA]	Taken into account: reference is changed
8-713	8	21	22	22	2	This section is very patchy and it is difficult to see the wood for the trees. The third paragraph actually contradicts some of the material in chapter 7. Aerosol size is of course very important for the direct effect (not mentioned here). The indirect effect is much less sensitive to chemical composition than usually thought (as discussed in chapter 7) at least in particular locations. [Olivier Boucher, France]	Taken into account: text will be harmonized with chapter 7
8-714	8	21	41	21	41	Replace "atmosphere of a few days" with "troposphere of a few days". [Robert Waterland, United States of America]	Taken into account: text was changed accordingly
8-715	8	21	43	21	44	Run-on sentence. Start new sentence at beginning of line 44. [Larry Horowitz, USA]	Editorial: typo is corrected
8-716	8	21	51			Where is "downwind of Greenland"? [Henning Rodhe, Sweden]	Taken into account: this typo is fixed
8-717	8	21	56	22	2	Another major development in the chemistry of aerosols with implications for climate forcing is mixing state. Adding this could refer back to Chapter 7 where mixing state is discussed. [Susan Anenberg, USA]	Taken into account: referenceto Chapter 7 is added
8-718	8	21	57	22	2	Add the Goldstein et al. 2009 (doi:10.1073/pnas.0904128106) paper. [Robert Portmann, United States of America]	Taken into account: reference is added
8-719	8	21				Section 8.2.3.6: There is substantial overlap with chapter 7. [Twan Van Noije, Netherlands]	Taken into account: text isbeing harmonized with chapter 7
8-720	8	22	1	22	11	This is a very important topic but this is too short to be useful - either extend it here or drop and refer to other chapters where it is dealt with in more detail, particularly the clathrate issue. [Susan Solomon, USA]	Rejected: section is removed in next draft
8-721	8	22	4	23	11	Interesting to say how important these limitaitons are for RF and CMIP5 models. What is the effect on RF caused by missing processes? [Piers Forster, UK]	Rejected: section is removed in next draft
8-722	8	22	4	23	11	The section 8.2.4 seems out of place and the focus of certain sub-sections rather odd (e.g. section on low surface ozone in the tropics). A shortersectionfocusing on poitns which are relevant for radiative forcing estimates and making use of recent reviews would be more useful. [Katharine Law, France]	Rejected: section is removed in next draft
8-723	8	22	8	22	14	But observations provide information about these historical emissions. For example, historical ODS emissions in the RCPs had their origin from atmospheric observations. [John Daniel, USA]	Rejected: section is removed in next draft
8-724	8	22	8	22	14	Add some more discussion on potential impacts on ozone and aerosols (e.g., from AEROCOM) if available [Larry Horowitz, USA]	Rejected: section is removed in next draft
8-725	8	22	16			Section on missing chemistry is devoted solely to halogens. What about missing hydrocarbon chemistry (particularly biogenic) or heterogeneous processes? Note also that "missing" refers to models; these are gaps	Rejected: section is removed in next draft

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						in our current understanding. [Oliver Wild, United Kingdom]	
8-726	8	22	18	22	28	Also discuss issues about halogen chemistry in polar regions [Larry Horowitz, USA]	Rejected: section is removed in next draft
8-727	8	22	18	22	28	Here, also refer back to discussions of isoprene chemistry, SOA, and heterogeneous reactions [Larry Horowitz, USA]	Rejected: section is removed in next draft
8-728	8	22	18	22	28	I agree with the sentiment that the role of marine halogen compounds in tropopheric ozone chemistry is far from clear. There are quite a few more papers that deal with this, not just Read et al., 2008. For example, Yang et al., JGR, 2005, and Yang et al., GRL, 2008, to name just two. [Olaf Morgenstern, New Zealand]	Rejected: section is removed in next draft
8-729	8	22	26	22	28	However, low values from near the surface can affect the upper troposphere where they are radiatively important (see Solomon et al. 2005, doi:10.1029/2005GL024323). [Robert Portmann, United States of America]	Rejected: section is removed in next draft
8-730	8	22	37	22	37	Same previous comment : There are experimental evidences that biogenic emissions can be inhibited during ambient stress conditions. Under these conditions (high air temperature or/and water vapour deficit) the Guenter et al. 1993 algorithm does not reproduce the real emissions of some species. This effect is related to the diurnal physiological cycle of the vegetation that close the stomata as response to water and temperature stress and under these conditions no emissions are produced. These stress ambient conditions can be frequently recorded in SOuth of Europe and habe their major imapcts during extreme events (heat waves and droughts) that affect sensitive areas in a climate change scenario. In a specially sensitive area like the Mediterranean, this effect have been observed and experimetally documented as reported in Plaza et al., 2005. Reference: J. Plaza, L. Núñez, M. Pujadas, R. Pérez-Pastor, V. Bermejo, S. García-Alonso and S. Elvira (2005), Field monoterpene emission of Mediterranean oak (Quercus ilex) in the central Iberian Peninsula measured by enclosure and micrometeorological techniques: Observation of drought stress effect. Journal of Geophysical Research, vol. 110, D03303, doi:10.1029/2004JD005168) . A reference to this particular behaviour from vegetation in sensitive areas should be included in the text. [BEGONA ARTINANO, SPAIN]	Rejected: section is removed in next draft
8-731	8	22	38	22	39	Replace "decrease moving to a high-CO2 environment" with "may decrease as atmospheric CO2 increases". [Robert Waterland, United States of America]	Rejected: section is removed in next draft
8-732	8	22	41	22	41	Change "possible" to "possibly" [Larry Horowitz, USA]	Rejected: section is removed in next draft
8-733	8	22	41	22	43	If there is only one study on isoprene emissions and aerosol formation, does it really deserve mentioning? [Ulrike Lohmann, Switzerland]	Rejected: section is removed in next draft
8-734	8	22	42	22	42	The conclusion of the Sitch et al (2007b) study is not actually stated, but should be - it should at least be specifically pointed out that there may be a further indirect RF from O3 acting via impacts on the large C sink and hence influencing the rate of CO2 rise. I think this proposal should also be given greater prominance, eg: a mention in the tropospheric ozone section, due to its potential importance (with appropriate caveats on the limited nature of the information of course) [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Rejected: section is removed in next draft
8-735	8	22	46	22	46	A very comprehensive review is now in print (Viviane R. Després, J. Alex Huffman, Susannah M. Burrows, Corinna Hoose, Aleksandr S. Safatov, Galina Buryak, Janine Fröhlich-Nowoisky, Wolfgang Elbert, Meinrat O. Andreae, Ulrich Pöschl, and Ruprecht Jaenicke (2012): Primary Biological Aerosol Particles in the Atmosphere: A Review. Tellus B, 57 p) and is available on request. Please let me know. [Ruprecht Jaenicke, Germany]	Rejected: section is removed in next draft
8-736	8	22	49	22	49	Recent studies (Orellana et al, 2011) have shown that marine microgels play an important role in regulating ocean basinscale biogeochemical dynamics. In this paper, it is demonstrate that, in the high Arctic, marine gels with unique physicochemical characteristics originate in the organic material produced by ice algae and/or phytoplankton in the surface water. The polymers in this dissolved organic pool assembled faster and with higher microgel yields than at other latitudes. The reversible phase transitions shown by these Arctic marine gels, as a function of pH, dimethylsulfide, and dimethylsulfoniopropionate concentrations, stimulate the gels to attain sizes below 1 μm in diameter. These marine gels were identified with an antibody probe specific toward	Rejected: section is removed in next draft

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						material from the surface waters, sized, and quantified in airborne aerosol, fog, and cloud water, strongly suggesting that they dominate the available cloud condensation nuclei number population in the high Arctic (north of 80°N) during the summer season. Knowledge about emergent properties of marine gels provides important new insights into the processes controlling cloud formation and radiative forcing, and links the biology at the ocean surface with cloud properties and climate over the central Arctic Ocean and, probably, all oceans. REF:Orellana, M. n. V., Matrai, P. A., Leck, C., Rauschenberg, C. D., Lee, A. M. and Coz, E. (2011). Marine microgels as a source of cloud condensation nuclei in the high Arctic.Proccedings of the Nacional Academy of Sciences of USA (PNAS), doi: 10.1073/pnas.1102457108. [BEGONA ARTINANO, SPAIN]	
8-737	8	22	55		55	this is written as if we know that the offset will be complete (100%), some uncertainty in this offset magnitude seems worth including [Stephen Montzka, USA]	Rejected: section is removed in next draft
8-738	8	23	5	23	7	It is probably worth explicitly mentioning here that sulphur deposition is possibly masking present-day wetland emissions. [Fiona O'Connor, United Kingdom of Great Britain & Northern Ireland]	Rejected: section is removed in next draft
8-739	8	23	5	23	7	This is misleading, because sulfur emissions are expected to go down in the future. [Twan Van Noije, Netherlands]	Rejected: section is removed in next draft
8-740	8	23	7	23	11	Might be good to cross-reference chapter 6 here. [Olivier Boucher, France]	Rejected: section is removed in next draft
8-741	8	23	10	23	11	Shallow water hydrates in the Arctic could be vulnerable to climate change on a timescale of less than 100 years. Indeed, there have been observations of methane bubbling along the Svalbard margin seafloor associated with hydrates by Westbrook et al. 2009, which they hypothesised was due to the warming of the West Spitsbergen current during the past 30 years. Indeed, a modelling study of the region by Reagan and Moridis (2009) supports that hypothesis although they applied the observed warming trend over a period of 100 rather than 30 years. [Fiona O'Connor, United Kingdom of Great Britain & Northern Ireland]	Rejected: section is removed in next draft
8-742	8	23	11	23	11	The statement ending in (Kley et al) is not consistent with the discussion of methane clatherates in chapter 6, page58. [Wayne Evans, USA]	Rejected: section is removed in next draft
8-743	8	23	11	23	11	Nor is this conclusion necessarily accurate since the clatherate issue is still an active debate. [Wayne Evans, USA]	Rejected: section is removed in next draft
8-744	8	23	15	23	15	Replace "There are several drivers of climate change operating" with "Several drivers of climate change operate". [Robert Waterland, United States of America]	Accepted-text changed as suggested
8-745	8	23	15	23	17	Changes in insolation and large volcanic episodes are natural drivers of climate change on multiple timescales. The stellar evolution of the Sun operates on timescales of billions of years. The changes in insolation due to changes in orbital parameters occurs on timescales of 10-100 thousand years. The Sun is known to have basic activity cycles of about 11 and 22 years that create effects that may have direct and indirect climate forcing effects. The amplitude of these effects are uncertain. There is little, if any, evidence of intrinsic solar variability of century to millennia timescales. [Bo Andersen, Norway]	Taken into account- Most models attribute all TSI changes exclusively to magnetic phenomena on the solar surface (sunspots, faculae, magnetic network) and can successfully reproduce the measured TSI changes between 1978 and 2003 . In fact there are not direct measurements of TSI variations on century time scales , but there are indiret evidence of these variations through sunspots for the last 400 years and cosmogenic isotopes (10Be and 14C) for the past millennia. Although the physical model connecting TSI and these proxies is still under developing.
8-746	8	23	16	23	17	Replace "The astronomical alignment between the Sun and Earth caused RF" with "Changes in the astronomical alignment of the Sun and Earth induces cyclical changes in RF". [Robert Waterland, United States of America]	Accepted-text changed as suggested
8-747	8	23	23			great section on solar. It would have been nice to have this synthesised and perhaps more about non-TSI effects making it to the ES? Text rather long-winded in palces [Piers Forster, UK]	Taken into account-The non-TSI efects (in particular cosmic ray effects on clouds) are just very briefly discussed in 8.4.1.5 (previously 8.3.1.5). As there is a large section devoted to cosmic rays and clouds in Chapter 7, section 7.4.7, we do not consider that

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							further comments on this aspect should be in the ES of this chapter.
8-748	8	23	23			Sec 8.3.1: This section is rather short and weak relative to the others, given the recent interest in this area. Please extend and add more references (and shorten aerosol section that has its own chapter) [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Taken into account- Text revised, extended where possible and more references added. Due to page number requiremenst, section 8.4.1 must not exceed three to four pages. Section 8.3.4 (previously 8.4.4) will be shortened.
8-749	8	23	25	23	28	I don't think these calculation have to repeated here (textbook knowledge) [Hubertus Fischer, Switzerland]	Taken into account- text changed, see new introduction of 8.4.1.
8-750	8	23	25	23	28	Not sure if maths lesson needed in an ipcc report. And each CMIP5 model won't fit this equation perfectly… [Piers Forster, UK]	Taking into account-text changed. Please see the new introduction of 8.4.1.
8-751	8	23	25	23	34	Comment on text: This organisation of the chapter supports the common belief that the radiative forcing can be essentially reduced to one single quantity, which is TSI. There are several major concerns here: 1) The physics behind radiative forcing really shows up in the SSI, and not in the TSI. 2) Differing evolutions of the various bands of the SSI may actually have a more significant impact on climate than what the sole TSI suggests [e.g. L. J. Gray, et al., Solar influences on climate, Rev. Geophys., 48 (2010), pp. 1–53.]. 3) These individual contributions of the SSI on different parts of the atmosphere may actually lead to a global uncertainty that exceeds what is supported by the figures from the TSI only. So, the prime conclusion here is that our level of scientific understanding is definitely lower than what the figures of the TSI would suggest and that the RF metric based on TSI only is not appropriate. [Thierry Dudok de Wit, France]	Taken into account- The reviwer rises an important point. Totally agree with your comments, we think that the sub-section 8.4.1.4 (previously 8.3.1.4) emphazises these points.
8-752	8	23	25	23	34	It is impossible to believe any of these figures. There were no reliable measures of solar irradiance in 1750, so there is no accurate knowledge of any change since. Averaging conceals its measured variability and also its different effect, both between day and night during each period. These efffects are dependent on poorly characterised, non linear relationships which defy any averaging process. [VINCENT GRAY, NEW ZEALAND]	Taken into account- Most models attribute all TSI changes exclusively to magnetic phenomena on the solar surface (sunspots, faculae, magnetic network) and can successfully reproduce the measured TSI changes since 1978. In fact there are not direct measurements of TSI in 1750, but there are indiret evidence of its variations through sunspots observed for the last 400 years. As, here we are interested in the radiative forcing on decadal or cetennial time-scales, then smoothing out variability on smaller time-scales is adequate for our purposes.
8-753	8	23	30	23	32	Can the "78%" value quoted be used with changes in TSI on long time-scales? Gray et al. 2009 appear to associate this value with "solar cycle forcings" and say "Of particular importance here is that the 11-yr SC forcings are not directly comparable to the effects of slowly varying forcings ... the oscillatory nature of the SC forcing, at a frequency which is high compared to the response time of the climate system, means that the climate system only has time to partially respond to this forcing". Are there any estimates of how the AF relates to the instantanous TOA RF? [Gareth S Jones, UK]	Taken into account- We have added references to other studies pointing to the uncertainties in the 0.78 factor. It is also noted that the factor may be different for long-term and a solar cycle. AF should be similar to RF for perturbations on the stratosphere.
8-754	8	23	30	23	32	Should it be noted that this value could be model dependent? Gregory et al. GRL 2004, with a AO-GCM with spectrally varying SI (albeit with coarse SW/LW bands) estimate a RF (should be equivalent to strat-adjusted) that is slightly lower but within error bars of the 0.25*0.7*delta TSI. Hansen et al. JGR 2005 different ways of calculating forcing have (I think) a maximum correction of -13% ish to the 0.25*0.7*delta TSI estimate. [Gareth S Jones, UK]	Taken into account- In the text it is emphazised that this factor is model dependent. We have also added references to other studies pointing to the uncertainties in the 0.78 factor.
8-755	8	23	38			Consistent TSI values should be used. Chapter 1 uses 1368 W/m2, Chapter 2 seems to prefer the new value of 1361 W/m2 (Kopp, G., and J. L. Lean (2011), A new, lower value of total solar irradiance: Evidence and climate significance, Geophys. Res. Lett., 38, L01706, doi:10.1029/2010GL045777), and Chapter 8, while discussing the new values, sticks with 1365 W/m2. [Georg Feulner, Potsdam]	Taken into account- In Chapter 1 it is mentioned the widely used *mean* value of 1368 W/m2. In section 8.4.1.1.1 (previously 8.3.1.1.1) we use the TSI between the minima of 1986 and 2008, and also the TSI between 1986 and 2011, we are not using mean TSI values. On the other hand, the 1361 W/m2 is the observed TSI-TIM value for the year 2008.

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8-756	8	23	39	24	17	The complications in satellite based TSI reconstructions are not mentioned in chapter 5, but have a direct influence on long-term TSI reconstructions. At least chapter 5 should refer to chapter 8.3.1.4.1 for details [Hubertus Fischer, Switzerland]	Taken into account-We will comment to Chapter 5 this suggestion of referring to 8.4.1.1.1 (previously 8.3.1.1.1) and 8.4.1.4.1 (previously 8.3.1.4.1).
8-757	8	23	41	23	48	(IRMB) should be replaced by (RMIB) (Royal Meteorological Institute of Belgium) which is legal in Belgium also as well as the French and Dutch names. This should be done also in further occurrences of RMIB. Moreover, the PMOD and RMIB data sets represent slightly differing interpretations of the same instrument, RMIB flyes now a similar instrument on the CNES solar monitor PICARD and it is hoped that a new composite using the PICARD data will be published in time for this report. [Christian Muller, Belgium]	Taken into account- We replaced IRMB by RMIB. Also we will wait for the new RMIB composite to include it in Fig.8.13 (previously Fig. 8.12) and change the discussion in 8.4.1.1.1 (previously 8.3.1.1.1) accordingly.
8-758	8	23	43	23	43	Replace "long-term trends" with "long-term trends of TSI". [Robert Waterland, United States of America]	Taken itno account- However, this subsection has been considerably changed.
8-759	8	23	45	23	46	Perhaps more explanation would help. It looks like all three show similar changes in figure 8.12. Perhaps give an idea of the size of change we are talking about. [John Daniel, USA]	Taken into account- We have modified Fig. 8.13 (previously Fig. 8.12), as we have matched (normalize) the composites to TIM at the year 2003, the initial year of TIM measurements. Now it is clearerwhich are the differences among the composites.
8-760	8	23	50	23	50	Add "TSI" before "variations" [Larry Horowitz, USA]	Accepted-text changed as suggeted
8-761	8	23	55	23	55	Add "TSI" before "for September" [Larry Horowitz, USA]	Accepted-text changed as suggeted
8-762	8	23	56	23	56	Misprint in last uncertainty, should be 0.1 and not 0.01 [Bo Andersen, Norway]	Taken into account-The paper indicates this uncertainty (Fröhlich, A&A, 501, L27-L30, 2009). Nevertheless, it will be checked if there is a misprint in the paper itself.
8-763	8	23		26		Good discussion para 8.3.1.1 thru 8.3.1.6. [Terje Wahl, Norway]	Noted
8-764	8	24	1	24	1	Run-on sentence. Start new sentence, "Thus, between the minima …" [Larry Horowitz, USA]	Accepted-text changed as suggested
8-765	8	24	2	24	2	Clarify why estimates are provided both for (1986-2008) and (1986-2010). Presumably, for 1750-2010 forcing, averages over a solar cycle are the relevant metrics [Larry Horowitz, USA]	Taken into account-We are including the RF for the three minima between 1986 and 2008, and also the forcing between the 1986 minima to present (2011). The relevant metrics for the 1750-2010 forcing is the solar cycle.
8-766	8	24	2			Not consistent with what is said on page 8-2, line 19 [Henning Rodhe, Sweden]	Taken into account. Note that the text has been changed on the whole. For the satellite era we use the RF of the last three minima, but we also include the forcing between the 1986 minima to present (2011).
8-767	8	24	4	24	12	It seems very likely that the Kopp and Lean assessment of TSI is correct (and ACRIM 3 corrections now match the TIM SORCE values), thus this section should be less tentative. [Gavin Schmidt, USA]	Accepted- The section is now rewritten in a less tentative way.
8-768	8	24	4	24	17	Include SORCE in Fig 8.12 and comment on comparisons with ACRIM, IRMB [Larry Horowitz, USA]	Accepted- Fig. 8.13 (previously Fiog. 812) now includes the TIM/SORCE results. The comoposites are matched (normalize) to TIM at the year 2003, the initial year of TIM measurements, and the proper comparison has been made.
8-769	8	24	4	24	17	Discuss implications (or lack thereof) of absolute calibration for RF [Larry Horowitz, USA]	Taken into acount-text changed. There is now an absolute SI-traceable calibration of TSI through the TIM and PREMOS instruments, and an absolute calibration of TSI is definitely possible. SI-traceable means that these calibrations are linked to national

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							standards laboratory references (USA's NIST, Great Britain's NPL, and Switzerland's PMOD/WRR) for end-to-end accuracy with quantified uncertainties. The new TSI Radiometer Facility (described in Kopp & Lean 2011) provides this traceability, having been calibrated by NIST cryogenic radiometer and linked by subsequent tests to NPL and the WRR. This new facility allows TSI calibrations to the SI standards of watts and meters. The TIM value is 4.46 W m-2 lower than the PMOD; nevertheless, the PMOD results are widely used in climatic models.This is presnted in 8.4.1.1.1, and it is also pointed out that the climatic models are calibrated to incorrectly higher values.
8-770	8	24	11	24	17	The SORCE TSI values are so important (high accuracy) that the report should be be updated as soon as the new data are published, otherwise, I would not object that the report mentions the unpublished data, especially those concerning 2010 and 2011 (transition from solar cycle 23 to solar cycle 24). These data have already been mentioned until the middle of 2011 in figure 17 of the refereed paper: J. Hansen, M. Sato, P. Kharecha, and K. von Schuckmann Earth’s energy imbalance and implications,Atmos. Chem. Phys., 11, 13421–13449, 2011 [Christian Muller, Belgium]	Taken into account- SORCE results are now included in Fig 8.13 (previously Fig. 8.12), and the section is updated. Also we matched the comoposites to TIM at the year 2003, the initial year of TIM measurements. However, as we point out in 8.4.1.1.1 : "As the maximum to minimum percentage change is well-constrained from observations, and historical variations are calculated as percentage changes relative to modern values, a revision of the TSI affects RF by the same percentage as it affects TSI. The downward revision of TSI, being 0.3%, thus has a negligible impact on RF."
8-771	8	24	12	24	13	Please provide expert judgement about how much such an incorrect calibration might matter. This is about a 0.3% offset; I cannot believe it would matter much. [John Daniel, USA]	Taken into account- Text changed. Although it is generally considered that a few tenths percent change in the absolute TSI value is of minimal consequences for climate simulations (see comment below by Gavin Schmidt). However model parameters are adjusted to ensure adequate representations of current climate for which incoming solar radiation is a decisive factor. Underway are experiments, for instance with GISS Model 3, to investigate the sensitivity of model performance to the TSI absolute value during present and pre-industrial epochs. When the related papers are available we will include the results.
8-772	8	24	12	24	13	“probe”--> prove. But the conclusion should be that this does imply that most GCMs are calibrated to the wrong TSI. It should also be added that the difference to climate is minimla because of the tuning for radiative balance - the difference of ~5W/m2 in TSI corresponds to a change of only 0.03 in the plantetary albedo. [Gavin Schmidt, USA]	Taken into account. Text changed, and in the paragraph we mention that the GCM are calibrated to incorrectly high values.
8-773	8	24	14	24	14	Clarify what you mean by "maximum-to-minimum". Is this solar max to solar min (11-yr cycle)? [John Daniel, USA]	Taken into account- Text changed to clarify, It is solar maximum to solar minimum.
8-774	8	24	20	24	22	About "Figure 8.12: Annual average composites of measured Total Solar Irradiance: The Active Cavity Radiometer Irradiance Monitor (ACRIM) (Willson and Mordvinov, 2003) , the Institut Royal Meteorologique Belgique (IRMB) (Dewitte et al., 2004) and the Physikalisch-Meteorologisches Observatorium Davos (PMOD) (Frohlich, 2006)". This figure includes three important time series, but not the SORCE one, which seems to be the most accurate one. Please consider to include the SORCE data. [Rubén D Piacentini, Argentina]	Accepted-Fig. 8.13 (previously Fig. 8.12) include now the annual TIM/SORCE series. Also we matched the comoposites to TIM at the year 2003, the initial year of TIM measurements.
8-775	8	24	24	25	24	This section needs to be made more coherent [Larry Horowitz, USA]	Accepted-text revised and changed.

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8-776	8	24	26	24	28	It is explained earlier but it would perhaps be better to refer to the 0.12 value first (so not to confuse AR4 fans when they go looking for the 0.09 number in the AR4) then the 0.09 value - corrected to compare with the AR5 value. [Gareth S Jones, UK]	Accepted-text changed as suggeted.
8-777	8	24	26	24	39	You do a good job of explaining how the current range is obtained, but can you clarify why the older range was too big? Do you now not believe some of the older data used to estimate the range in AR4? [John Daniel, USA]	Taken into account-The AR4 range was 0.06-0.3 w/m2 (0.05-0.23 w/m2 if we consider the 0.78 factor used in AR5). The upper limit corresponds to the reconstruction of Lean (2000), based on the reduced brightness of non-cycling Sun-like stars assumed typical of a Maunder minimum state. The use of such stellar analogs was based on the work of Baliunas an Jastrow (1990), but more recent surveys have not reproduced their results and suggest that the selection of the original set may have been flawed (Hall and Lockwood, 2004; Giampapa, 2004). The lower limit corresponds to the increase in the 11-years cycle amplitude since 1750. As we point out in the text, the uncertainty bar has been obtained in quite different way in AR5 compared to AR4.
8-778	8	24	31	24	31	Define "open flux" [Larry Horowitz, USA]	Taken into accoun.- The text has been considerably changed.
8-779	8	24	38	24	39	I appreciate the reasons for the "low confidence", but it could seem quite odd that despite confidence not increasing since the AR4 that the value is getting smaller with substantially smaller uncertainty bars. [Gareth S Jones, UK]	Takent into account- The RF in AR5 has changed considerably (decreased about 50%) with respect to AR4. The uncertainty bars have been obtained in quite different way in AR5 compared to AR4. In AR4 the upper limit came from the reconstruction of Lean (2000) based on the reduced brightness of non-cycling Sun-like stars assumed typical of a Maunder minimum state (that is considered no longer valid); the lower limit corresponded to the increase in the 11-years cycle amplitude since 1750. We point out in the text the AR4 and AR5 difference concerning RF and its uncertainty.
8-780	8	24	41	24	48	This paragraph is somewhat unclear. In the last sentence you talk about 1850 for the first time. How does this statement fit with the earlier discussion in the paragraph? More discussion of the statement "because they are no longer considered valid" is necessary. Otherwise, it is not clear to the non-solar specialist what this is referring to. [John Daniel, USA]	Taken into account- The text has been changed considerably. Some TSI reconstructions were based on the reduced brightness of non-cycling Sun-like stars assumed typical of a Maunder minimum state. The use of such stellar analogs was based on the work of Baliunas an Jastrow (1990), but more recent surveys have not reproduced their results and suggest that the selection of the original set may have been flawed (Hall and Lockwood, 2004; Giampapa, 2004). We explain this in the text.
8-781	8	24	47	24	48	The last sentence of this paragraph seems out of place in the discussion here [Larry Horowitz, USA]	Taken into account- The text has changed considerably, due to page limit.
8-782	8	24	50	24	50	Specify what is meant here by "this range" [Larry Horowitz, USA]	Taken into account- Due to page limit we have included this part in the Supplementary Material section. Were the text has changed as: "..falls outside the range 0.08–0.18 W m-2 reported above,…"
8-783	8	24	50	24	50	The Shapiro analysis doesn't 'find' anything, they assume the value during a grand minima.See discussion in	Taken into account-Paragrap changed as: "The

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						Schmidt et al (2012, GMD, v1.1) [Gavin Schmidt, USA]	analysis of Shapiro et al. (2011) falls outside the range 0.08–0.18 W m-2 reported above, with a larger MM-to-present RF of 0.78 Wm-2. They derive a very large decrease in solar output during the MM, assuming that in addition to the cyclic variation in active regions, there is a background change in the Sun so that during the MM every part of the Sun was only as bright as the dimmest part of the modern ‘quiet’ area observations." However, due to page limit we have included this part in the Supplementary Material section.
8-784	8	24	50	24	57	The Shapiro model is semi-empirical, fitting the different solar physics effects so that the obtained variations of TSI fit current observation, it is not yet a demonstrated technique to infer future solar activity nor to deduce past solar effects. [Christian Muller, Belgium]	Taken into account- Paragrap changed as: "The analysis of Shapiro et al. (2011) falls outside the range 0.08–0.18 W m-2 reported above, with a larger MM-to-present RF of 0.78 Wm-2. They derive a very large decrease in solar output during the MM, assuming that in addition to the cyclic variation in active regions, there is a background change in the Sun so that during the MM every part of the Sun was only as bright as the dimmest part of the modern ‘quiet’ area observations." However, due to page limit we have included this part in the Supplementary Material section.
8-785	8	24	53	24	55	A reference for large amount of magnetic activity in "quiet" areas. [Gareth S Jones, UK]	Taken into account. The section has been changed considerably and some parts are now in the Supplementary Material section.
8-786	8	24	54	24	54	“is a still a large” should be “is still a large”. [Dirk Olivié, Norway]	Taken into account. Due to page limit requirements, the section has been changed considerably a several part are now in the Supplementary Material section.
8-787	8	24	56			write: follow ice core 10Be records [Hubertus Fischer, Switzerland]	Taken into account. Due to page limit requirements, the section has been changed considerably and several part are now in the Supplementary Material section.
8-788	8	25	3	25	7	This is a circular argument amd should not be used to defend choice of TSI reconstruction. It wouldn't matter if all the assessed range of TSI is used for is scientific curiosity of past solar changes. But as it will be used to assess the contributions to past climate change, either by comparing the radiative forcings with those from other factors or for inclusion in climate models, then it does matter. Climate is used to constrain the TSI which is then used to see what caused the change in climate ... See Rodhe et al., Avoiding circular logic in climate modeling, Climatic Change, 2000 if I am not making myself very clear. [Gareth S Jones, UK]	Taken into account- The reviwer is addressing an important point. Rodhe et al. (2000) say that "..it would be circular to use forcings so derived in a climate simulation that is then tested against the temperature record." Feulner (2011) uses three different TSI reconstructions, derived from proxies of solar activity such as sunspots (in one of the reconstructions,being sunspots independent of the climate record) or cosmogenic isotopes (in two reconstructions, although 10Be is not completely independent of the climate record), to model the Nothern-hemisphere temperature, these models are then compared to an ensemble temperature reconstruction computed by Frank et al. (2010). The TSI reconstruction based on the sunspot record and one of the TSI reconstructions based on 10Be are very close, the other TSI 10Be reconstruction (the Shapiro et al.) presents changes that are very large

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							comapred to the changes seen in the other two reconstructions. Then we can conclude that the climate record in the case of the TSI reconstructions using 10Be is not playing an important role. Therefore, In this paper we cannot appreciate how climate is constraining TSI, and we do not see a circular argument. However, due to page limit requirements, the section has been changed considerably and several part are now in the Supplementary Material section.
8-789	8	25	9	25	18	Important, this abnormal solar minimum will have consequences on spectral analysis of solar variations as we left it only a few months ago, this paragraph will be updated by publications expected in 2012. [Christian Muller, Belgium]	Accepted-We shall update the paragraph with the new publications
8-790	8	25	13	25	13	Delete "hence" [Larry Horowitz, USA]	Taken into account. Due to page limit requirements, the section has been changed considerably and several part are now in the Supplementary Material section.
8-791	8	25	13	25	14	When discussing the Schrijver et al. (2011) study, one might add that climate model simulations using their solar irradiance reconstruction are compatible with NH temperature reconstructions (see Feulner, G. (2011), Are the most recent estimates for Maunder Minimum solar irradiance in agreement with temperature reconstructions?, Geophys. Res. Lett., 38, L16706, doi:10.1029/2011GL048529). [Georg Feulner, Potsdam]	Taken into account-Taken into account. Due to page limit requirements, the section has been changed considerably and several part are now in the Supplementary Material section.
8-792	8	25	13	25	18	I am not keen on using "Maunder" by itself as a shorthand. Should use Maunder Minimum or "MM". [Gareth S Jones, UK]	Accepted- MM will be used.
8-793	8	25	21	25	24	About "Figure 8.13: Annual mean reconstructions of Total Solar Irradiance since 1750: Wang et al. (2005), with and without an independent change in the background level of irradiance, Steinhilber et al. (2009) (here we show an interpolation of their 5-year time resolution series), The Krivova et al. (2010) time series, and the PMOD composite time series (Frohlich, 2006)."The Krivova et al curve cannot be seen in the colour line representation. Please, include the correct line. [Rubén D Piacentini, Argentina]	Accepted- Figs. 8.13 and 8.14 (previously 8.12 and 8.13) are now improved. Also, the comoposites are matched (normalize) to TIM at the year 2003, the initial year of TIM measurements.
8-794	8	25	34	25	43	This paragraph ignores the papers cited in the introduction of this referee's report. To consider the TSI only is reductive. In particular, I recommend discussing the origin of the observed 60-years decadal oscillation in this Chapter. [François GERVAIS, France]	Taken into account-We think that the sub-section 8.4.1.4 (previously 8.3.1.4) illustrates the importance of Solar Spectral Irradiance, and it has been emphazised that metric based on TSI only is not appropriate.Concerning the 60-years decadal oscillation inclusion in Chapter 8 it is something that the CLAS shoul look at.
8-795	8	25	35	25	35	Replace "SC" with "Sunspot Cycle". [Robert Waterland, United States of America]	Taken into account- We defined Sunpot Cycle (SC) in 8.4.1´s introduction, in order to reduced the number of words, as this is very much restricted.
8-796	8	25	35		43	Here and throughout. Use of first person plural "we" inevitably leads to shifting reference. Most use of the word in this chapter is we, the authors. All of a sudden line line 34 "we are within a grand activity maximum"; clearly it is not the authors, but that this condition characterizes the present geophysical epoch of the planet. Then to line 43 "we have very low confidence" back to the authors. Suggest pay attention to this throughout. Same issue page 29 line 3: "How well can we predict" Who is doing the predicting? The authors? The scientific community? Ditto "us" at page 29, line 18-19, "a natural experiment ... that can inform us" whom? the authors? Page 8-29, line 53 "Our understanding" Just omit "our". OK, no further comments on this subject. But do go throught it and restrict first person plural to the chapter authors in the context of making a definition or restricting scope of coverage. [Stephen E Schwartz, USA]	Accepted-text revised and changed when appropriate.
8-797	8	25	41	25	42	Should this sort of statement be in chapter 12 instead? [Gareth S Jones, UK]	Accepted- Text changed and a reference to Chapter

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							12 has been added.
8-798	8	25	41	25	43	There have been two studies showing this, one using a simple radiative forcing argument (Wigley, T. M. L., and P. M. Kelly (1990), Holocene climatic change, 14C wiggles and variations in solar irradiance, Philos. Trans. R. Soc. A, 330, 547–558) and one using a simplified coupled climate model (Feulner, G., and S. Rahmstorf (2010), On the effect of a new grand minimum of solar activity on the future climate on Earth, Geophys. Res. Lett., 37, L05707, doi:10.1029/2010GL042710). Both should be cited here. [Georg Feulner, Potsdam]	Taken into account- Text changed and a reference to Chapter 12 has been added.
8-799	8	25	41	25	43	Delete last sentence in para 8.3.1.3. Statement may well be true, but should be discussed/justified elsewhere. [Terje Wahl, Norway]	Accepted- Text changed and a reference to Chapter 12 has been added.
8-800	8	25	42	25	42	"WILL continue to be dominated" is too strong here. Perhaps use the IPCC likelihood wording. [John Daniel, USA]	Accepted-text changed as : "...there is a high confidence that .."
8-801	8	25	43	25	43	Feulner and Rahmstorf (2011) [Gavin Schmidt, USA]	Taken into account--Due to page limit requirements, this part is now in the Supplementary Material section.
8-802	8	25	55		56	What does "the latter years show similar behaviour to prior observations" mean? Give years, reference. [Joanna Haigh, UK]	Taken into account- We have changed the text due to page limit requirements, now it reads: "The Spectral Irradiance Monitor (SIM) on board of SORCE (Harder et al., 2009) indicates over the SC 23 declining phase measurements that are rather inconsistent with prior knowledge, indicating that additional validation and uncertainty estimates are needed (DeLand and Cebula, 2012; Lean and DeLand, 2012). "
8-803	8	25	56	25	56	Change "latter" to "later" [Larry Horowitz, USA]	Accepted-text changed
8-804	8	26	9	26	9	About "8.3.1.4.3 Impacts of UV variations on the stratosphere ". Taking into account the very important UNEP Reports: "Environmental Effects of Ozone Depletion and its Interactions with Climate Change", I suggest, mainly with respect to Surface UV-Climate change interactions, to introduce at the end of this item (line 25), another item that could have the following title: "8.3.4.4 Impact of surface UV variations on climate change" (or a similar one). Its content could be a summary of the main items related to the present AR5-WGI of the UNEP 2010 Report. Another possibility is to extend the title to: "8.3.1.4.3 Impacts of UV variations on the stratosphere and at the Earth´s surface" and to incorporate in the same item, some paragraphs related to this last subject (surface UV-climate change interactions). [Rubén D Piacentini, Argentina]	Taken into account-This important report focuses on the effects of ozone changes on climate. Being ozone one of the main causes of UV-B changes, the report also discusses the impacts of UV-B changes on human health, terrestrial and aquatic ecosystems, biogeochemical cycles, air quality, and damage to materials. However, the impacts of UV-B changes themselves on climate are not treated. On the other hand, section 8.4.1 (previously 8.3.1) deals with solar radiation changes (including UV) due to intrinsic solar phenomena and their related forcing at the Top of the Atmosphere, we do not discuss the complex interactions of the solar radiation with the troposphere or its impact on surface. Moreover, ozone is treated in several other sections of Chapter 8.
8-805	8	26	9	26	24	UV variations due to the solar 11-year cycle affect the stratosphere through two pathways as radiative response. One is direct UV heating of the background ozone and the other is indirect solar and terrestrial radiation through the solar cycle-induced ozone change. The direct effect is dominant in the upper stratosphere and above, while the indirect effect is domnant below about 5 hPa down to the middle stratosphere (Shibata and Kodera, 2005). Please refre to these direct and indirect effects of the solar 11-year cycle, in particular their altitude partitioning, on the stratosphere. Shibata, K. and K. Kodera: Simulation of radiative and dynamical responses of the middle atmosphere to the 11-year solar cycle, Journal of Atmospheric and Solar-Terrestrial Physics, 67, 125-143, doi:10.1016/j.jastp.2004.07.022, 2005. [Kiyotaka Shibata, Japan]	Taken into account- Text changed as suggested.

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8-806	8	26	12	26	12	I may have missed it, but if not define "SC" [John Daniel, USA]	Taken into account-It is defined in the introduction of 8.4.1.
8-807	8	26	14	26	15	This sentence seems to be getting into the response, and I comment later that some of the text under "response" in 8.6.2.2 is really about the forcing. I realise it isn't an easy separation to make. [Leon Rotstayn, Australia]	Taken into account- The text has been considerably changed, and this part appears no longer.
8-808	8	26	26	26	36	The redaction should be clearer, high solar activity means a stronger heliospheric magnetic field and thus a more efficient screen against galactic cosmic rays. This should be stated more clearly in order to avoid misinterpretations. This argument is typically used outside the "established" science world to attribute climate variations to solar forcing as a dominant effect, so it is important that it is well charcterized from the refereed litterature in a convincing way for a broad public. [Christian Muller, Belgium]	Accepted-text changed.
8-809	8	26	26	26	36	effects of GCR on clouds – any references to include here? [Helen Worden, USA]	Taken into account- references included
8-810	8	26	28	26	31	Explain the link between GCRs and solar activity [Larry Horowitz, USA]	Taken into account-text changed according to this suggestion
8-811	8	26	28	26	36	This paragraph needs references, eg: for the hypothesised mechanism, studies which indicate a correlation between GCRs and cloud cover, and those with contradictory evidence. For example, Svensmark et al have a paper in open review in Atmos. Chem. Phys. Discuss., 12, 3595-3617, 2012 www.atmos-chem-phys-discuss.net/12/3595/2012/ doi:10.5194/acpd-12-3595-2012 while Laken et al are in press with Journal of Climate 2012 ; e-View doi: http://dx.doi.org/10.1175/JCLI-D-11-00306.1 . Both these papers use MODIS data but appear to draw different conclusions. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Taken into accoun-the references have been included. I have also read the recommended papers and realize the controversy.
8-812	8	26	34	26	36	In my view, the sentence "There is no evidence that their effect…." reads as unnecessarily dismissive - section 7.4.7 actually concludes in a way which has important subtle differences: "there is medium evidence and high agreement that the cosmic ray-ionization mechanism is too weak to influence global concentrations of CCN or their change over the last century or during a solar cycle in a climatically-significant way." which I think is a more useful and accurate statement. Relevant papers are; Pierce, J. R., and P. J. Adams (2009), Can cosmic rays affect cloud condensation nuclei by altering new particle formation rates?, Geophys. Res. Lett., 36, L09820, doi:10.1029/2009GL037946; and Kazil, J., K. Zhang, P. Stier, J. Feichter, U. Lohmann, and K. O'Brien (2012), The present-day decadal solar cycle modulation of Earth's radiative forcing via charged H_2SO_4/H_2O aerosol nucleation, Geophys. Res. Lett., 39, L02805, doi:10.1029/2011GL050058. The review article by Gray et al (2010) already in this chapter's reference list also contains relevant discussion. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Taken into account- Sentence changed, however, the text of this subsection is a summary of Section 7.4.7.3. I have also read the recomended papers and realize the controversy.
8-813	8	26	38	26	53	I am not sure I understand the purpose of this section beyond what is already there in the previous two sections. I thought efficacy for the solar forcing is less than 1 (at least this is what we see in the G1 GEOMIP experiment, see Schmidt et al, ESDD, 2012). Also I don't quite get how the SORCE results (isn't SORCE a satellite?) can provide information on climate efficacy). Finally I would argue that the fact that RF is not a good indicator of regional climate changes is true for all forcing mechanisms, so I'm not sure why the solar forcing is singled out here. [Olivier Boucher, France]	Taken into account- The section is deleted but the information (corrected with the help of this comment) has been incorporated in other sections. SORCE is a satellite, the results come from TIM and they do not provide information on climate efficacy.
8-814	8	26	41	26	43	Some have argued that the transient response to solar TSI changes (over solar cycles) are larger than the models would indicate and thus that the efficacy is larger than 1 (see Tung et al. DOI:10.1029/2008GL034240). [Robert Portmann, United States of America]	Taken into account-The section has been deleted and the information is incorporated in other sections. Tung et al. adopt an efficacy factor close to 1, lower o equal (0.7 to1), and find that the transient climate response in most of the current general circulation models is too low compared with the observed range. However, the authors do not make any implications on the efficacy factor itself.
8-815	8	26	42	26	42	Explain what is meant here. How will SORCE results challenge efficacy=1 for solar forcing. Is this referring to the Haigh et al. study cited above? Explain [Larry Horowitz, USA]	Taken into account-The section has been deleted and the information is incorporated in other sections. It was a misteake, the SORCE results do not challange efficacy ~1

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8-816	8	26	48	26	49	Solar forcing has also been shown to potentially influence ENSO [Mann, M.E., Cane, M.A., Zebiak, S.E., Clement, A., Volcanic and Solar Forcing of the Tropical Pacific Over the Past 1000 Years, Journal of Climate, 18, 447-456, 2005] and the South Asian Summer Monsoon [Fan, F., Mann, M.E., Ammann, C.M., Understanding Changes in the Asian Summer Monsoon over the Past Millennium: Insights From a Long-Term Coupled Model Simulation, J. Climate, 22, 1736-1748, 2009]. [Michael Mann, USA]	Taken into account- The section has been deleted and the information is incorporated in other sections. But the references are icluded.
8-817	8	26	48	26	49	Please see previous comment. [Leon Rotstayn, Australia]	Taken into account-The section has been deleted and the information is incorporated in other sections.
8-818	8	26	49			and Southern Annular Mode (Kuroda and Kodera, GRL, 2005; Roscoe and Haigh, QJRMS, 2007) [Joanna Haigh, UK]	Taken into account- The section has been deleted and the information is incorporated in other sections. But the reference is included.
8-819	8	26	55	28	56	I feel the section 8.3.2 "Volcanic Radiative Forcing" is too long. [Hong Liao, China]	Rejected - It is of the length allowed in chapter outline, and the reviewer does not explain why it is too long and what should be cut.
8-820	8	27	1	27	6	The excellent and interesting papers referenced here don't actually support the claim in this sentence. The first and last reference are purely modeling studies and the 2nd reference looks at solar influences since the pre-industrial era. Maybe a link to 10.7.2 would be appropriate. [Gareth S Jones, UK]	Accepted - additional reference added.
8-821	8	27	3	29	40	This section is well written. It might be worth adding that the radiative forcing of e.g. Sarychev has been estimated as reaching a global mean peak value of around -0.13Wm-2 (Haywood et al., 2010), and that the mean volcanic radiative forcing over the period 2000-2010 has been estimated as around -0.1Wm-2 (Solomon et al., 2011). Given the climatic unimportance of Eyja, perhaps there is rather too much text devoted to it. It might be worth concentrating a little more description of what is shown in Figure 8.15 - this would link to the radiative forcing estimates. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland]	Accepted - Changes made.
8-822	8	27	9			Over what spatial (global, I suppose) and temporal time scales? [Henning Rodhe, Sweden]	Accepted - Changes made.
8-823	8	27	14	27	14	I would rewrite the sentence and say “efficacy” only once. [Dirk Olivié, Norway]	Accepted, but this sentence has been removed.
8-824	8	27	14	27	16	About the sentence: "The efficacy of the RF for volcanic aerosols with the standard definition in Section 8.1.1., the efficacy of volcanic forcing has been determined to be 0.91 (Hansen et al., 2005)".This sentence seems to be redundant in relation to the "efficacy of volcanic forcing". A possible version could be: "The efficacy of the RF for volcanic aerosols, with the standard definition given in Section 8.1.1., has been determined to be 0.91 (Hansen et al., 2005)". [Rubén D Piacentini, Argentina]	Accepted, but this sentence has been removed.
8-825	8	27	15	27	15	Delete "the efficacy of volcanic forcing" [Larry Horowitz, USA]	Accepted, but this sentence has been removed.
8-826	8	27	15	27	15	Remove ", the efficacy of volcanic forcing". [Robert Portmann, United States of America]	Accepted, but this sentence has been removed.
8-827	8	27	15	27	15	What is this standard definition? [Robert Waterland, United States of America]	Accepted, but this sentence has been removed.
8-828	8	27	15			Hansen is only one model so effiacy statement I too strong. But again an excellent section on volcanoes, but could be shortened [Piers Forster, UK]	Accepted, but this sentence has been removed.
8-829	8	27	18	27	23	The influence of extended volcanic outbreaks on the atmosphere best is documented by times series of atmospheric turbidity. 100 year records of atmospheric turbidity on a global basis are available, but never used to correlate to global temperature. Helmes, L., R. Jaenicke (1986) Atmospheric Turbidity Determined From Sunshine Records. J. Aerosol Science 17, 261-263; Jaenicke, R. (1988) Aerosol Physics and Chemistry. Landolt-Börnstein Numerical Data and Functional Relationships in Science and Technology New Series Group V: Geophysics and Space Research, 4 Meteorology, Subvolume b [Ruprecht Jaenicke, Germany]	Rejected - There are no such global records. And correlation with temperature is too simple a method. But I cannot get access to these papers.
8-830	8	27	18	27	46	"There have been no large volcanic eruptions with a detectable climatic response since the 1991 Mt. Pinatubo eruption …. The background stratospheric aerosol concentration has had an upward trend for the past decade ... and had a small, but important impact on RF" These statements appear contradictory - no detectable climatic response vs. important impact on RF [Richard Keen, USA]	Accepted, but actually the distinction is between detecting forcing and detecting response, and this has been clarified.

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8-831	8	27	25	27	34	This paragraph sounds more like an advertisment for the respective research but no consensus knowledge is presented. I would suggest to delete this paragraph. [Hubertus Fischer, Switzerland]	Accepted - The paragraph has been reduced substantially.
8-832	8	27	25		34	GOMOS (Kyrola reference) is stellar occultation and not limb scanning. Three references on a system where there is still questions regarding the quality of its data (OSIRIS) seems a bit much. No mention of the historical record by SAGE II and other solar occultation instruments, which appear in the following section and figures, (the whole SAM-SAGE series, HALOE, POAM II/III, etc.) (unless this section is strictly to current systems) seems remiss. No mention of SCIAMACHY also seems odd. NPP could also be mentioned since it is now operating. Some easily obtainable references for CALIPSO should be included (see papers by Vernier for instance one of which is used in the following section). Could mention TOMS, OMI, etc that measure column SO2 from volcanos since the topic is mentioned already. Maybe a reference to the SPARC aerosol assessment would be appropriate since many systems and records are discussed there. On the other hand maybe this isn't a crying need to include any of them other than something on the order that stratospheric aerosol measurement sby space-based sensors have been made on a continuous basis since 1978 by a number of instruments employing solar and stellar occultation, limb scattering, limb emission, and lidar strategies (possibly listing representative members of each group). [Larry Thomason, United States of America]	Accepted - The paragraph has been reduced substantially.
8-833	8	27	28	27	28	I don't know what "There is no organized system to be ready for the next big eruption" means. [Robert Waterland, United States of America]	Accepted - The paragraph has been reduced substantially.
8-834	8	27	29	27	30	Specify which satellites/instruments. [Larry Horowitz, USA]	Accepted - The paragraph has been reduced substantially.
8-835	8	27	29	27	31	There are at least 3 limb scanning satellites that can measure stratospheric aerosol. You have certainly left out MIPAS. A good reference for this (will be shortly on AMTD) is Sembhi et al, submitted to AMTD, 2011 (abstract in next comment) [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Accepted - The paragraph has been reduced substantially.
8-836	8	27	29	27	31	Sembhi et al, AMTD, 2011, MIPAS detection of cloud and aerosol particle occurrence in the UTLS with comparison to HIRDLS and CALIOP (H. Sembhi, J. Remedios, T. Trent, D. P. Moore , R. Spang, S. Massie and J-P. Vernier. Abstract: Satellite infra-red emission instruments require efficient systems that can separate and flag observations which are affected by clouds and aerosols. This paper investigates the identification of cloud and aerosols from infra-red, limb sounding spectra recorded by the Michelson Interferometer for Passive Atmospheric Sounding (MIPAS), a high spectral resolution, Fourier transform spectrometer on ENVISAT. Specifically, an existing cloud index method for detection of cloud and aerosol (particle) emissions is simulated, with a radiative transfer model, in order to establish for the first time limits to confident detection of particle effects in MIPAS data. The newly established thresholds improve confidence in the ability of MIPAS to detect particle injection events and plume transport in the UTLS as well as better characterised cloud distributions. The method also provides a fast front-end detection system for the MIPClouds processor, a processor designed for the retrieval of macro- and microphysical cloud properties from the MIPAS data. It is shown that across much of the stratosphere, the threshold for the standard cloud index in band A is 5 although values of over 6 occur in restricted regimes. Polar regions show a surprising degree of uncertainty at altitudes above 20 km due to potential high ClO formation and also poor signal-to-noise due to low atmosphere temperatures. The optimised thresholds of this study can be used for much of the time, but time/composition dependent thresholds are recommended for MIPAS data for the strongly perturbed polar stratosphere. In the UT, thresholds of 5 apply at 12 km and above but decrease rapidly at lower altitudes.The new thresholds are shown to allow much more sensitive detection of particle distributions in the upper troposphere and lower stratosphere (UTLS), with detection limits above 13 km often better than 10exp(-4) km-1, with values approaching 10exp(-5) km-1 in some cases. Comparisons of the new MIPAS results with data from HIRDLS and CALIOP, outside of the poles, establishes a very good agreement in distributions (Cloud occurrence frequencies and clouds and aerosol top heights) with an offset between MIPAS and the other instruments of 0.5 km between 12 km and 20 km. We conclude that current infra-red limb sounders provide a very consistent picture of particles in the upper troposphere and lower stratosphere (UTLS), allowing detection limits which are consistent with the lidar observations. Investigations of the MIPAS data for the Kasatochi volcanic eruption and the Black Saturday fires in Australia are used to exemplify the usefulness of MIPAS limb sounding data for monitoring aerosol injections into the UTLS and into the stratosphere in particular over	Accepted - The paragraph has been reduced substantially.

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						monthly timescales. It is shown that the new thresholds allow such events to be much more effectively monitored from MIPAS with detection limits for these case studies of 1x10-5 km-1 at 12 microns. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	
8-837	8	27	36	27	40	Additional ways volcanoes influence climate - changes in preciptiation due to SW and evaporation changes (Lambert et al, GRL, doi:10.1029/2004GL019545), influences on the North Atlantic overturning circulation (Jones et al, climate dynamics, DOI: 10.1007/s00382-005-0066-8), impacts on carbon cycle (Jones and Cox, Global Biogeochemical Cycles, 2001) and longer timescale temperature and sea level variations via ocean heat transport (Church et al, Nature, doi:10.1038/nature04237) [Gareth S Jones, UK]	Accepted - Changes made.
8-838	8	27	44	27	45	Here also Nagai et al., SOLA, 6, 69-72, 2010, can be cited. [Olaf Morgenstern, New Zealand]	Accepted - Changes made.
8-839	8	27	44	27	46	It should be mentioned that there is a possibility that some of the increase in background stratospheric aerosol may be anthropogenic in origin. Myhre et al, Tellus, 2004, Vernier et al, GRL, doi:10.1029/2010GL046614, Siddaway and Petelina, GRL, doi:10.1029/2010JD015162 [Gareth S Jones, UK]	Rejected. The latest research by Toon and colleagues shows that there is little anthropogenic component.
8-840	8	27	45	27	45	"WAS PRODUCED" is too strong. It certainly was partially caused by volcanic activity, but it has also been suggested (and not disproved) that anthropogenic emissions have played a role. [John Daniel, USA]	Rejected. The latest research by Toon and colleagues shows that there is little anthropogenic component.
8-841	8	27	45	27	46	I disagree that the trend was small. Stratospheric aerosol over New Zealand increased by 40% since the minimum in ~2000. I also don't think the last word has been spoken on what the cause of the trend is. The alternative explanation of industrial emissions (probably of Chinese origin) being lofted into the tropical lower stratosphere had been proposed by Hofmann et al.; a recent turnaround in the trend of the stratospheric aerosol layer would coincide nicely with changes in emissions in China due to fitting of flue gas filters in power plants. I don't know of any refereed literature to support this, though. To be on the safe side, a qualifier such as "possibly" or "probably" would be good in this sentence. Also the Chinese emission explanation could be mentioned. The mentioned small northern mid-latitude eruptions would not have affected aerosol loadings in the Southern extratropics (unless the aerosol reached a considerable height) yet the trend in aerosol was observed in both hemispheres. [Olaf Morgenstern, New Zealand]	Rejected. The latest research by Toon and colleagues shows that there is little anthropogenic component.
8-842	8	27	46	27	46	How important? [Olivier Boucher, France]	Accepted - Changes made.
8-843	8	28	1	28	2	skip Eyjafjallajökull - it had no global climatic relevance, as explained in the chapter [Ruprecht Jaenicke, Germany]	Rejected - It has to be mentioned and quantified, as everyone knows about it.
8-844	8	28	5	28	5	Change to "a factor of 100" (or thereabout), e.g., 20/(0.01*14) = 143 [Larry Horowitz, USA]	Accepted - Changes made.
8-845	8	28	5	28	6	What do you mean by "lifetime"? Also, if the total emission is smaller by a factor of 1000, if total is the cumulative emissions over the entire eruption period, it is not clear why you also multiply by 50. If it is the daily emission rate (<0.01 Tg/day) consider not using 'total'. [John Daniel, USA]	Accepted - Changes made.
8-846	8	28	5	28	6	You could also mention that the aerosols did not make it to the stratosphere as the Pinatubo aerosols did, leading to a much shorter atmospheric residence time. [John Daniel, USA]	Accepted - Clarified. This was included in the factor because of the different lifetimes.
8-847	8	28	20	28	20	It would be preferable to have the x-axes line up with each other in the two panels. [John Daniel, USA]	Noted.
8-848	8	28	20	28	33	The significance of the QBO winds on the figure should be discussed or it should be removed from the figure. [Robert Portmann, United States of America]	Noted.
8-849	8	28	21	28	21	About "Figure 8.14". It must be improved significantly, since it is of very low resolution with respect to the other figures of this Chapter 8. [Rubén D Piacentini, Argentina]	Noted.
8-850	8	28	21	28	21	About "Figure 8.15: (a)".Please, explain what the horizontal dashed line at AOD(50 nm) = 0.002 means. [Rubén D Piacentini, Argentina]	Noted.
8-851	8	28	35			8.3.2.3 Is this subsection heading appropriate? The next two paragraphs talk about volcanic records over the last millennium and the Tob eruption. A different heading or include details here about how volcanoes can	Accepted - Changes made.

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						have long term effects on climate? e.g. Church et al, Nature, doi:10.1038/nature04237? [Gareth S Jones, UK]	
8-852	8	28	37	28	38	Tree-rings may underestimate the magnitude of past explosive volcanic eruptions because of threshold-dependent growth effects [Mann, M.E., Fuentes, J.D., Rutherford, S., Underestimation of Volcanic Cooling in Tree-Ring Based Reconstructions of Hemispheric Temperatures, Nature Geosciences (in press)]. [Michael Mann, USA]	Noted.
8-853	8	28	37	28	39	Eclipses directly measure the very property of the aerosols that is radiatively effective, namely, a globally integrated optical depth. However, eclipse records are sparse before about 1800. Refs. Below [Richard Keen, USA]	Noted.
8-854	8	28	37	28	39	Keen, R., 1983 "Volcanic aerosols and lunar eclipses", Science, 222, 1011-1013. D. Hofmann, D., J. Barnes, E. Dutton, T. Deshler, H. Jäger, R, Keen, and M. Osborn, 2004. “Surface-Based Observations of Volcanic Emissions to the Stratosphere” , in Volcanism and the Earth’s Atmosphere, Geophysical Monograph 139, American Geophysical Union. [Richard Keen, USA]	Noted.
8-855	8	28	37	28	39	Replace "While lunar brightness and colour during eclipses (Stothers, 2007) and tree ring records (Salzer and Hughes, 2007) are useful for producing records of past volcanism, because ice cores actually preserve the very material that was in the stratosphere they are the most useful way of producing such records" with "Lunar brightness and colour during eclipses (Stothers, 2007) and tree ring records (Salzer and Hughes, 2007) are useful for producing records of past volcanism. Ice cores provide the best evidence of historic volcanism because they preserve remnants of the actual material injected into the stratosphere." [Robert Waterland, United States of America]	Noted.
8-856	8	28	38	28	39	Is it really correct that "ice cores preserve the very material that was in the stratosphere"? How do you distinguish tropospheric vs. stratospheric aerosol origination in the ice? How do you know at one time it was in the stratosphere. Perhaps this is just done by looking at the lifetime of the decay. However it is done, please provide a reference or another description. [John Daniel, USA]	Noted.
8-857	8	28	38	28	39	Change to "past volcanism, ice cores, which actually presere the very material that was in the stratospherre, are the …" [Larry Horowitz, USA]	Noted.
8-858	8	28	46	28	46	About the sentence: "simulations for this period (see Section [x])", do not forget to include the corresponding number in place of "x". [Rubén D Piacentini, Argentina]	Accepted - Changes made.
8-859	8	28	48	28	56	Consider giving a bottom line of how such a supereruption might affect RF. [John Daniel, USA]	Noted.
8-860	8	28	49	28	51	First introduce how a 100x Pinatubo simulation didn't suggest feedbacks would cause ice age conditions (Jones et al, climate dynamics, DOI: 10.1007/s00382-005-0066-8) even when radiative forcing impact may have been overestimated and then lead into the more sophisticated modelling of the aerosols etc in the Robock 2009 and subsequent studies. [Gareth S Jones, UK]	Accepted - Changes made.
8-861	8	28				Figure 8.14. Is the volcanic forcing record used in this figure the same as in chapter 5? In chapter 5 Crowley and Unterman (submitted) is cited but not here. This should be the same. In fact I think this should be discussed in chapter 5 and referred to in chapter 8 [Hubertus Fischer, Switzerland]	Noted - we have coordinated with Chapter 5. We discuss the forcing since 1750 and they discuss it for the past millennium.
8-862	8	28				Again in 8.3.2.3 Crowley and Unterman (submitted) is not cited [Hubertus Fischer, Switzerland]	Rejected - not needed in revised text.
8-863	8	29	12	29	14	The Tie and Brasseur GRL 22 1995 paper should be referenced here. [Robert Portmann, United States of America]	Rejected - not needed in revised text.
8-864	8	29	12			I recognize that this expert review is meant to be technical, not grammar, but I suggest that in formal writing "While" means simultaneity, not simple contrast by a semicolon or "although", "whereas". Also pay attn to placing of restrictive adverbs:" future forcing will depend only on", not "future forcing will only depend on". Grammatical lapses such as these undermine the perceived authority of the document. [Stephen E Schwartz, USA]	Accepted - Changes made.
8-865	8	29	13	29	14	You could mention the change in sign of the ozone effect. [John Daniel, USA]	Rejected - not needed in revised text.

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8-866	8	29	16	29	40	Also comment in this section on the use of historical volcanic eruptions and the temperature response they induce as a test of climate models (in particular, of climate sensitivity) [Larry Horowitz, USA]	Accepted - Changes made.
8-867	8	29	18	29	26	There are also some important imperfections in the analogy that should be mentioned. [John Daniel, USA]	Accepted - Changes made.
8-868	8	29	18	29	26	There is no mention here about the possibility that even knowing the potential problems, if future climate changes become severe enough, geoengineering may be tried. Also, even just the serious discussion of geoengineering can affect the will to mitigate GHG emissions. [John Daniel, USA]	Rejected - this is for another working group.
8-869	8	29	20	29	20	I suggest you refer to section 7.5 rather than Chapter 7 as a whole. [Olivier Boucher, France]	Accepted - Changes made.
8-870	8	29	22	29	23	Run-on sentence. Start new sentence with beginning of line 23. [Larry Horowitz, USA]	Accepted - Text has been rewritten.
8-871	8	29	28	29	40	I wonder if this is the right place to discuss this. You could have a subsection in the future RF section that discusses "RF surprises" from future volcanic activity, permafrost thawing and other things such as a nuclear war. [Olivier Boucher, France]	Noted - but we agreed with Thomas Stocker to leave it here.
8-872	8	29	28	29	40	This text is more hypothetical and not that relevant for IPCC [Ruth Doherty, UK]	Rejected - All of the future is hypothetical. And Chapter 8 of the report is supposed to "Cover emissions from fires and their effects" according to the scoping document.
8-873	8	29	28		40	The nuclear winter material is interesting and valid but I wonder if it shouldn't be in a separate section (from volcanos) to help draw attention to it. [Larry Thomason, United States of America]	Noted - It would be great if there were such a section, but there is not.
8-874	8	29	29	29	29	Please define what you mean by "soot". The common usage of soot refers to light absorbing, carbonaceous aerosols, which are quite different from the sulphuric acid and mineral ash produced resulting from volcanic eruptions. [JOHN OGREN, USA]	Accepted - This is the correct definitiont.
8-875	8	29	30	29	40	I think the discussion of a nuclear war here is not justified and should be entirely deleted. Instead an assessment of the feasibility and requirements of climate engineering using SO4 aerosol injections into the stratosphere could be fruitful here based on the volcanic experiences [Hubertus Fischer, Switzerland]	Noted - but we agreed with Thomas Stocker to leave it here.
8-876	8	29	30		40	I concur that climate change due to large-scale nuclear war is a threat. But I question whether this is the venue for making that point. Might end up bringing report into question for straying from topic. [Stephen E Schwartz, USA]	Noted - It would be great if there were such a section, but there is not.
8-877	8	29	42			Section 8.4: spell out in detail the originof the RF numbers in Table 8.5. Which models were used? [Robert Waterland, United States of America]	Taken into account - text revised in section 8.3.2 (previously section 8.4.2) to include description of methods for calculation of RF values in Table 8.5
8-878	8	29	44	29	51	Anthropogeic influences on the climate are many. Humans reduce convection cooling in the daytime by measures to reduce the impact of winds such as shelter belts and even windmills, and measures to interfere with evaporative cooling by reservoirs,, calverts,, buildings, and roads over soil, the growing of crops and pastures and the draining of wetlands. Humans reduce radiation by buildings, solar heating, even greennouses. You mention none of these. The role of greenhouse gases is minor and mainly involves water vapour. and aerosols. [VINCENT GRAY, NEW ZEALAND]	Rejected - No scientific publication provided to support changes suggested by the reviewer. This section quantify all anthropogenic influences on the radiation budget of major importance.
8-879	8	29	44	29	51	Reference the discussion in Section 8.2.3 [Larry Horowitz, USA]	Accepted - a reference to section 8.2.3 is included as suggested.
8-880	8	29	48			suggest rewording to avoid the misinterpretation that the lifetime of any single gas can vary substantially [Stephen Montzka, USA]	Taken into account - text revised to clarify the variation in lifetimes
8-881	8	29	53	30	50	This section doesn't fit well here. Move to 8.1? [Larry Horowitz, USA]	Rejected - the section is not moved to 8.1, but the chapter has been re-ordered so this will now be section 8.3.1.
8-882	8	29	53			Good section that wasn't in AR4. In contrast to some of the rest of the report it's concisely written [Piers	Noted.

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						Forster, UK]	
8-883	8	29	57	29	57	page 8-29; Section 8-4-1,line 57, after " transfer codes"09 02 2012 10h15 These codes are based on approximation of the interactions between the electromagnetic (e.m.) field with the vibrations-rotations, collisions of the molecules of the atmosphere. As we are interested by the exchange of energy, focused on the exchange of energy between these two fields, we used the approximation of transport for the description of the e.m. field during this interaction. The correspondent equation, the equation of transport . The flux of energy of the e.m. field of frequency v in a point M, at a time t, crossing a surface dM , in a direction Ω, called the intensity I(M, v,Ω,t) is non linear in the variable M because the term of interaction between the two fields is also a function of the widening of the lines of interactions between the two fields, because of the collisions. between the molecules. As the collisions and the transitions between , states of the molecules are so small, compared to the durations of thermodynamics (In the troposphere, the mass density is high enough to obtain a great collision rate among the molecules, and so , an Local Thermodynamic Equilibrium, and then a local temperature. It is in fact the definition of troposphere.) and fluid dynamics in the atmosphere, we can use stationary equations. To obtain a description of the variations of collisions in function of the altitude, we must add the phenomena of convection, where the energy flux is also a (high heat capacity per unit of volume act as a reservoir of energy, slow to empty [Robert DAUTRAY, France]	Rejected-outside of the scope of the chapter.
8-884	8	30	1	30	1	page 8-30; line 1; after HITRAN HITRAN (HIgh-resolution-TRANsmission- molecular absorption database). In addition to this huge amount about line s of interaction between the e.m. field and the vibration-rotation-electronic data, beginning in the late 1960, HITRAN contains now new files concerning aerosols, UV line-per-line cross-sections and new IR data. [Robert DAUTRAY, France]	Noted.
8-885	8	30	18	30	45	This paragraph reads a lot like a list. Could this information be put into a table with the implications of the agreement/disagreement summarized more fully in the text? [John Daniel, USA]	Rejected-the information here is not as simple as a table with the agreement/disagreement.
8-886	8	30	18		45	This para is very important. But it should make clear which studies are for clear sky; which are for cloudy or all-sky conditions. I suspect most are for clear sky, which is a much easier problem. The next para sweeps the problem under the rug. Yes "the accurate expression of clouds ... is very important", indeed. But the para just stops with that statement. How much does presence of clouds diminish forcing from doubled CO2 relative to cloud free? Collins 06 suggests that the forcing is diminished from 5.25 to 3.7 or whatever is the concensus (not necessarily accurate) value. But more impt, what is the uncertainty assoc with doubling CO2 in all sky conditions, with real citations. Myhre's 1998 GRL paper widely cited does not present enough information to evaluate the accuracy of the 5.35 ln (CO2/CO2_0) expression presented there, putatively for all sky conditions. [Stephen E Schwartz, USA]	Accepted-text revised.
8-887	8	30	19	30	21	"Some researchs compared different LBL models…" Please consider Jacobson, M.Z., A refined method of parameterizing absorption coefficients among multiple gases simultaneously from line-by-line data, J. Atmos. Sci., 62, 506-517, 2005 [Mark Z. Jacobson, U.S.A.]	Accepted-the referece is added.
8-888	8	30	22	30	22	Page 8-30; section8.4.1; line 22 After "Fomin and Fallaleena, 2009) The line-per-line models, gives us the description , with each widened line of the permitted transitions between the vibration-rotation states and their harmonics, an accurate physical description, the spectral aspect of the e.m. comprised, at each point in the atmosphere, and also, for each frequency the e.m. field-i.e. the intensity. It give us not only these information on the energy exchanges between the these fields, but also, useful physical quantities like the opacity in each point of the troposphere, for each frequency, defined by Opacity (M, v) = (mass density of air at point x Mean free path) - 1 [M²/kilogram]. The mass of a vertical column of the troposphere permits several paths, which mean that the troposphere is optically thick, permitting the approximation of LTE. But it is not optically thick enough to make a mean of all the frequencies of the e.m. field as the "Rosseland mean", (this later quantity is depending then on temperature, which is the non linear term of the transport equation) and too optically thick to use the approximation of Planck (Planck mean absorption coefficient[Gary E. Thomas, Knut Stamnes: Radiative transfer in the atmosphere and ocean; Cambridge University Press; 2002: sections 12.2 and 12.3]) On the contrary, radiative transfer in the upper layer of the ocean is optically thick. The e.m. field entering the	Rejected-outside of the scope of the chapter.

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						surface is absorbed in about 100 meters, heating it and by mixing and convection with the down layers, conducting the majority of the increase of energy due to the increased opacity of the atmosphere by radiation forcing, to heat the deep ocean. . Another question may be: Is the radiative forcing of a GHG (i.e. the CO2) a linear function of the total increased concentration of this GHG, for the to day concentration? With the tools concerning the non linearity terms in the radiative transport equation [Gary E. Thomas, Knut Stamnes: Radiative transfer in the atmosphere and ocean; Cambridge University Press; 2002], the answer is no, for the CO2, but Yes for the methane! It is important to stress that an important term in the interaction of the e.m. field and the matter of the atmosphere is the convection of this matter due to the change of mass density induced by increased temperature. [Robert DAUTRAY, France]	
8-889	8	30	28	30	28	I think the sentence becomes slightly more fluent if one puts “but” or “howeve” before “problems remained”. [Dirk Olivié, Norway]	Editorial-copyedit to be completed prior to publication.
8-890	8	30	30	30	30	“were” should be “ where”. [Dirk Olivié, Norway]	Editorial-copyedit to be completed prior to publication.
8-891	8	30	35	30	35	“has” after “Zhang et al. (2003)” should be “have”. [Dirk Olivié, Norway]	Editorial-copyedit to be completed prior to publication.
8-892	8	30	37	30	37	Maybe replace “in simulation” by “in simulating”. [Dirk Olivié, Norway]	Editorial-copyedit to be completed prior to publication.
8-893	8	30	44	30	44	Replace “participation” by “participating”. [Dirk Olivié, Norway]	Editorial-copyedit to be completed prior to publication.
8-894	8	30	45	30	45	About the sentence: "The mean shortwave shortwave forcings by CO 2 are consistent with the LBL estimates". Delete one of the "shortwave" words. [Rubén D Piacentini, Argentina]	Editorial-copyedit to be completed prior to publication.
8-895	8	30	47	30	50	This comes across as an afterthought. How accurate is our understanding of clouds? How much error do cloud uncertainties introduce to the forcing changes over time? etc. [John Daniel, USA]	Accepted-text revised; please also see 8.5.
8-896	8	30	47	30	50	"-25%" should be changed to "25%" since it is stated that it is a decrease earlier in the sentence [Robert Portmann, United States of America]	Accepted-text revised.
8-897	8	30	48	30	50	The uncertainties due to the attenuation by clouds of the radiative forcings and the GWPs are not adequately dealt with in this paragraph. A 25% uncertainty needs to be applied across the board to radiative forcings by long lived greenhouse gases if the work by Zhang is accepted. There have been few experimental measurements on the effects of clouds on radiative forcings. It has mainly been modelled.This is a large uncertainty and needs to be dealt with in this report instead of buried in a short pragraph.The water vapour overlap with GHG lines is accentuated in cloudy conditions. Any trends in cloudiness may affect the long term trends of the RF by LLGHG. [Wayne Evans, USA]	Accepted-text revised; please also see 8.5 and 8.7.
8-898	8	30	52	33	6	This whol;e section is misleading. First of all, by far bthe most important greenhouse gas is water vapour. You completely ignore it partly because it overwhelms all the others, so they do not matter, partly because you cannot possibly derive any sort of average, because it varies so widely in space and time. So you tuck it under the carpet and pretend that it is a constant, depenedent only on mean global temperature. Then, what, exactly is meant by "long-lived"? How long is a piece of string? All these gases undergo chemical changes all the time with a whole range of "lifetimes". You also completely fail to face the fact that all of them are highly variable in the atmosphere, so that you have imposed severe resrtrictions on the times and places they are allowed to be measured, so that you can pretrend that there are constant figures that can be tabulated in Table 8.5.in order to use them in models that assume "well-mixed" gases of unvarying concentration. You then create panic about the alleged cobsequences of increases in your notional figures without their concentrations being measured at all above the places subject to the panic instructions. [VINCENT GRAY, NEW ZEALAND]	Taken into account: Water vapour is addressed thoroughly in section 8.4.3.3 (will be renumbered) and FAQ 8.1. The terms long-lived and well mixed will be explained more fully in the SOD and used to distinguish those gases for which surface point measurements are sufficient to characterise their atmospheric abundance (as listed in table 8.5) from those that are more variable. The lifetimes of these gases are listed in table 2.12 (may be renumbered). Details of the observational evidence for these gases is discussed in section 2.4.1.
8-899	8	30	52	42	51	The uncertanity estimates in the present day forcing section seem rather ad-hoc and ill defined [Piers Forster, UK]	Taken into account: the uncertainties are more carefully described for all the present day forcings
8-900	8	30	52			Good sections on the different gases - short and to the point [Piers Forster, UK]	Noted: Thank you for the comment

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8-901	8	31	1	31	1	I don't know whether “due to climate” is the most appopriate expression here. [Dirk Olivié, Norway]	Editorial: Re-phrased as "due to changing climate"
8-902	8	31	6	31	6	"miixng ration" should be "mixing ratio" [BEGONA ARTINANO, SPAIN]	Editorial: Changed to "ratio"
8-903	8	31	6	31	6	typo: should read "…the atmospheric mixing ratio of CO2…" [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Editorial: Changed to "ratio"
8-904	8	31	6	31	6	Change "ration" to "ratio" [Larry Horowitz, USA]	Editorial: Changed to "ratio"
8-905	8	31	6	31	6	“ration” should be “ratio”. [Dirk Olivié, Norway]	Editorial: Changed to "ratio"
8-906	8	31	11	31	14	Is this paragraph required here? [Gareth S Jones, UK]	Taken into account: This paragraph has been shortened and combined with the previous one
8-907	8	31	12	31	12	The airborne fraction itself may be directly affected by human activity, eg: O3 physiological effects (reducing the terrestrial carbon sink) and land cover changes also weakening the terrestrial carbon sink by reducing the coverage of forests. This latter point was already included in the AR4 estimates of the contribution of land use to CO2 RF as cited in line 19 of this page. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Rejected: While we accept the comment, the sentence on airbourne fraction has been removed following comments from other referees.
8-908	8	31	12			Is it really "cement production"? Or do you mean " kilns". What about smelters? [Pieter Aucamp, South Africa]	Rejected: The sentence referring to cement production has beeen removed following comments from other reviewers.
8-909	8	31	16			I suggest to show the formula. This could be done in a table as in TAR, showing the formulas for CO2, CH4, N2O, etc. [Glen Peters, Norway]	Accepted: The table will be added.
8-910	8	31	17	31	18	What are the sources of this uncertainty? [John Daniel, USA]	Taken into account: Uncertainty described in section 8.3.1 based on available scientific litterature
8-911	8	31	18	31	18	"There has been no new information to update this uncertainty". Really? The line parameters have been updated. There have been several intercomparisons that include LBL calculations. Rapid adjustments have been quantified. I'm OK with keeping the uncertainty 10% but I do think there has been new information since 2001. [Robert Portmann, United States of America]	Taken into account: see comment 8-910
8-912	8	31	18	31	20	Good to see the fossil fuel / land cover change split mentioned - this is important information that should be retained in future drafts. I see the AR4 assessment is cited with no further updates, but a new estimate ought to be possible based on more recent literature. Also, just as a heads-up, the authors should check for consistency with Chapter 9 (land use emissions are discussed on page 9-38 line 19, and although a slightly different point is made there - current emissions not total historical contribution to past CO2 rise and RF - but even so it will important to check that sources of information are consistent. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted: Chapter 6 only has land use effects in GtC/yr. This sentence will have to be removed (in spite of the favourable comment) if there's no new data.
8-913	8	31	18			"no new information to update this uncertainty". Is this because no one has done the necessary analysis. That is, if the analysis was redone, the uncertainty would still be 10%, or, if the analysis was redone using latest models, the uncertainty would decrease. Perhaps worth indicating if new studies are required to improve the estimate? [Glen Peters, Norway]	Taken into account: see comment 8-910
8-914	8	31	18			10%. I have already expressed my concern over the asserted uncertainty. Just saying it over and over (from 2001 till now) perpetuates the myth of that uncertainty but doesnt prove the case. I rest my case on the Collins 06 paper. See my comment on chapter 1 page 6, above. Concern over 10% applies to all ghgs, not just CO2. And certainly the 10% does not take cognizance of the rapid adjustments that are the focus of much discussion in Chapter 10, in which it is the adjusted forcing, as inferred from Forster-Taylor type analysis, that is the actual driver of climate change. [Stephen E Schwartz, USA]	Taken into account: see comment 8-910. We clearly distinguish between RF estimates from detailed radiative transfer codes used in the IPCC estimates and the uncertainty in RF estimates from GCM.
8-915	8	31	22	31	31	There is almost no discussion of Table 8.5 and figure 8.16. At the very least the very noisy variations in the year-to-year changes in CO2 RF (factor of 4) are noteworthy. Is this real? What is the cause? [Robert Portmann, United States of America]	Accept: Add text

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8-916	8	31	25			why not be more quantitative here and provide the percentage increase. I calculate nearly 85% of the increase in direct RF from all LLGHGs over the past 15 years is accounted for by CO2. [Stephen Montzka, USA]	Accept: Will be quantified.
8-917	8	31	28	31	28	About "Figure 8.16":i) There is no indication of (a), (b) and (c) in each figure; ii) it is very difficult to read the chemical compounds (in different colours) written to the right. Please, modify this (as in Figure 8.19) and eventually build two different figures with all the curves included in the figure placed in the middle; iii) include the name of the variables in the vertical axis (Radiative forcing in the first two and Radiative forcing change in the last one), like in Figure 8.18; iv) the lines are very thin, please made them more visible. [Rubén D Piacentini, Argentina]	Accept: Figure will be redone.
8-918	8	31	33	31	35	A very important point. What is the confidence here? I am not aware of other studies that have specifically looked at the effect of CO2 physilogical forcing on cloud cover, but man studies have looked at the impact on evapotranspiration, with many different models. This point has more literature behind it than would appear from the citation of this single study. I thinkk it should be mentioned in the Exec Summary as it is relevant to the comparison of CO2 with other GHGs by policymakers. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Taken into account: This paragraph will be expanded to discuss adjusted forcing including Andrews et al. 2012.
8-919	8	31	33	31	35	Is this included in the stated uncertainty? Can you assess this result? [John Daniel, USA]	Taken into account: This paragraph will be expanded to discuss adjusted forcing including Andrews et al. 2012.
8-920	8	31	33	31	35	This paragraph should be somewhere else (e.g., earlier in the chapter where rapid responses to CO2 are discussed; even though this is an indirect forcing). Is the 10% estimate used? [Robert Portmann, United States of America]	Taken into account: This paragraph will be expanded to discuss adjusted forcing including Andrews et al. 2012.
8-921	8	31	33	31	36	Is this 10% enhancement included in the RF bar chart, either as part of the central estimate or the error bars? [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted: Because this contributes to the adjusted forcing is is not included in the bar chart
8-922	8	31	33	31	55	It actually increases the "adjusted forcing" by 10% not the "radiative forcing". [Olivier Boucher, France]	Accepted: This paragraph will be expanded to discuss adjusted forcing including Andrews et al. 2012.
8-923	8	31	36	31	36	A further potentially important point regarding the physiological forcing by CO2 is the impact on surface albedo via enhanced vegetation growth. Unfortunately I am not sure that an estimate of RF due to this process has specifically been made. There are several studies using models to estimate the impacts of surface albedo change driven by this process in the future (eg: O'ishi, R., A. Abe-Ouchi, I. C. Prentice, and S. Sitch (2009), Vegetation dynamics and plant CO2 responses as positive feedbacks in a greenhouse world, Geophys. Res. Lett., 36, L11706, doi: 10.1029/2009GL038217) which may be relevant. Also there is evidence of greening of the Arctic and boreal regions (eg: Esper and Schweingruber 2004, GRL, amongst others, although this does of course present an attribution issue - is it CO2 effects (in which case this does count as a forcing) or a response to climate change (in which case it's a feedback and therefore outside the scope of this chapter). Nevertheless, I think this issue still warrants discussion here. If this issue is discussed, it would be useful to discuss with authors of WG2 chapter 4 (Terrestrial and Inland Water Systems) which is covering the issue of observed ecosystem change (I am an author on that chapter and will be happy to act as a point of contact if required). [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Accepted: Will comment on the albedo effect (cite O'ishi without estimating a magnitude) and discuss adjusted forcing.
8-924	8	31	37	32	9	The discussion of CH4 and N2O here and in 8.2.3 is awkward and redundant [Larry Horowitz, USA]	Rejected: The discussion of CH4 and N2O is appropriate and necessary here
8-925	8	31	39	31	39	Change "global averaged" to "Globally-averaged" [Olivier Boucher, France]	Editorial: Change made.
8-926	8	31	45			and in Chapter 2 [Stephen Montzka, USA]	Accepted: Chapter 2 referrenced.
8-927	8	31	50	31	50	Doesn't the Montzka paper (Science, 2011) suggest the oxidizing capacity has not changed? [John Daniel, USA]	Accepted: Will cite Montzka 2011 and Rigby 2008 as opposing views
8-928	8	31	51	31	53	Description of the functional form of the dependence is a bit cryptic here; some explanation is needed. [Oliver Wild, United Kingdom]	Accepted: text removed

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8-929	8	31	52			expand on the text in parenthesis as to why RF is proportional to log for CO2 vs. square for CH4 concentrations [Ruth Doherty, UK]	Accepted: see 8-929
8-930	8	31				I appreciate the link to the data for Fig 8.16. Such links should be made available for all line graphs. [Stephen E Schwartz, USA]	Noted, most of the data will be made available in Annex II.
8-931	8	32	15	32	17	This is a true but incomplete (or misleading) sentence since without the Montreal Protocol the continued growth of the CFC would have caused a larger increase in RF than the HCFC's used as substitutes (since they have longer lifetimes). [Robert Portmann, United States of America]	Rejected: This discussion refers to observed changes rather than world-avoided scenarios
8-932	8	32	17	32	17	Do you mean "emissions" or "concentrations" here? [Olivier Boucher, France]	Accepted: This will be clarified to refer to concentrations.
8-933	8	32	19	32	19	Change subsection title to "CFCs and HCFCs" to distinguish from use of "halocarbons" as a generic term for halogen-containing species ini section 8.4.2.4 (line 14) [Larry Horowitz, USA]	Accepted: This will be changed
8-934	8	32	20	32	20	Spell out the particular Montreal Protocol gases - do you just mean Annex A gases? [Robert Waterland, United States of America]	Taken into account: Will be re-phrased in terms of CFCs and HCFCs
8-935	8	32	22	32	23	CFC-11 contributes almost as much to the decline (more than 70% of CFC-12 influence). [John Daniel, USA]	Taken into account: "and CFC-11" will be added to the text.
8-936	8	32	22	32	23	mention here that CFC-12 has the largest GWP of the Ozone depleting substances? [Rolf Mueller, Germany]	Rejected: GWPs are discussed later in the chapter
8-937	8	32	23			although it is true that over the past 5 years the decline in CFC-12 has resulted in a slightly larger decrease in RF than the decrease in CFC-11, overall, decreases in CFC-11 have contributed a decline in RF that is twice as large as those from CFC-12 [Stephen Montzka, USA]	Rejected: We are focussing on changes since AR4
8-938	8	32	27	32	27	Delete "(Table 8.5)" [Larry Horowitz, USA]	Accept: "Table 8.5" replaced by "table 8.10"
8-939	8	32	27	32	28	Change "therefore emissions essentially accumulate permanently in the atmosphere" to something like "therefore their loss processes are negligible for timescales considered here" or some other variant that is truthful. [Robert Portmann, United States of America]	Accept:This will be rephrased.
8-940	8	32	32	32	32	About the word "rapdidly", change it by: "rapidly". Please, in these and other cases, use the authomatic English grammar correction system. [Rubén D Piacentini, Argentina]	Editorial: Typo corrected.
8-941	8	32	32			"rapidly". [Timothy Wallington, USA]	Editorial: Typo corrected.
8-942	8	32	37	32	37	Table 8.5: should this table include lifetime information? at least in paragraph 8.4.2.4.2 reference is made to the table and lifetimes are discussed at the same time. [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Taken into account: Reference made to lifetimes in table 8.10
8-943	8	32	37	33	5	There should be references for these observations [John Daniel, USA]	Accept: Observational values will be provided by chapter2 and cited.
8-944	8	32	37	33	5	It would be good to explain why there are differences compared with Table 5A-3 from the last ozone assessment (WMO,2011). Some seem to just be different because of the time registration, but this does not account for all differences. [John Daniel, USA]	Taken into account: Value for halogenated species in FOD were from AGAGE only. Values in SOD will be from a combination of sources.
8-945	8	32	37	33	5	The CF4 radiative forcing values appear to be incorrect. The radiative efficiency is 0.10 W/m2/ppb. [John Daniel, USA]	Rejected: Forcing values are correct allowing for a 34.7 ppt pre-industrial background
8-946	8	32	37	33	5	Add to this table the 2005 AR4 values if any differ (otherwise not agreement in a table footnote) [Larry Horowitz, USA]	Taken into account: Differences due to changes in standards will be discussed in a footnote
8-947	8	32	37	33	5	Table 8.5. Sometimes AGAGE values from Chapter 2 are used, sometimes NOAA values. Neither is presumed more accurate than the other, why not take the average of the two? Some indication of the data source (at least refer reader to Chapter 2) should be given in the Table notes. [Stephen Montzka, USA]	Take into account: Values in SOD will be an average as calculated in chapter 2 which will be cited in the notes.

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8-948	8	32				Table 8.5: Please be clear with the last few items that (and how) they are aggregating previous items in the table. [Marcus Sarofim, USA]	Taken into account: This will be clarified
8-949	8	33	1			Do you get same forcing in 2005 as there was in AR4, if not why not? [Piers Forster, UK]	Taken into account: Differences due to changes in standards will be discussed in a footnote
8-950	8	33	3	33	4	“Totals” should be “Total”. [Dirk Olivié, Norway]	Editorial: This will be corrected.
8-951	8	33	10			Overalps with chemistry section and too long [Piers Forster, UK]	Taken into account: This will be re-written and linked to 8.2
8-952	8	33	12	33	12	In the final draft, I hope it will be appropriate to include Cionni et al. (2011); this paper has estimates of tropospheric and stratospheric O3 RFs and their evolution. I calculated the RFs and subsequently found rather different results from an updated [David Stevenson, UK]	Rejected: We can only consider the updated RFs if they are submitted before 31st July
8-953	8	33	12	33	18	General aspects related to ozone chemistry should be covered in Section 8.2.3.1. [Twan Van Noije, Netherlands]	Taken into account: This paragraph will be re-written and linked to 8.2
8-954	8	33	12	35	40	You now create an additional panic by another set of supposedly "short-lived"gases resulting from chemical changes in the first lot. They all depend on your absurdly oversimplified "greenhouse" theory which eliminates ordinary climate. [VINCENT GRAY, NEW ZEALAND]	Rejected: No suggestion proposed.
8-955	8	33	18	33	18	Add to end of last sentence in paragraph: "either by advection of ozone or modification of photolysis rates" [Larry Horowitz, USA]	Taken into account: This paragraph will be re-written and linked to 8.2
8-956	8	33	20	8	30	Measurements of the surface radiative forcing from tropospheric ozone have been conducted with a new technique using an FTS under cloud decks.Ozone mixing ratios have been measured simultaneously. Generally the measured fluxes have been consistent with model values around 0.4 W/m2 . The corresponding values of the NRF were .024 W/m2-DU. W.F.J. Evans and E. Puckrin, Remote Sensing Measurements of Tropospheric Ozone by Ground-Based Thermal Emission Spectroscopy, J. Atmos. Sci., 56, pp 311-318, (1999).. W.F.J. Evans and E. Puckrin, Measurement of Radiative Forcing Fluxes beneath Clouds, Proceedings of the 13th Symposium on Global Change and Climate Variations, American Meteorological Society Meeting, p. 170-172, 10.3, Orlando, January 13-17, (2002). [Wayne Evans, USA]	Rejected: We only consider tropopause forcing here.
8-957	8	33	20	33	20	Replace "The tropospheric ozone RF is often calculated by scaling the tropospheric column by" with "Tropospheric ozone RF is often calculated by scaling to the tropospheric column resulting in ". [Robert Waterland, United States of America]	Accepted: This change will be made.
8-958	8	33	20			Normalized radiative forcing. This is a useful concept and can be extended to other ghg's. Schwartz et al (Why Hasn't Earth Warmed as Much as Expected? Schwartz S. E., Charlson R. J., Kahn R. A., Ogren, J. A., and Rodhe H., J. Climate 23, 2453-2464 (2010); doi: 10.1175/2009JCLI3461.1.) did this for CO2, finding 0.0141 W m-2 ppm-1, which varies only slowly as a function of CO2 mixing ratio. I see that such a figure (0.0138) is given in table 8.10, so reference might simply be made here to that table. The normalized forcing is a great simplification and should be used more widely in this document and elsewhere. [Stephen E Schwartz, USA]	Taken into account: Reference will be made to table 8.10.
8-959	8	33	21			Dobson Unit. This unit is an artifact of the old means of measuring ozone column abundance and should be abolished, replaced by systematic units, eg mol m-2. Suggest use mol m-2 as primary unit and give dobson side by side for convenience of those who still think in that unit. [Stephen E Schwartz, USA]	Accepted: Both mol m-2 and DU will be used.
8-960	8	33	24	33	25	Clarify that the dependency on "which species generated the ozone" is (presumably) because of the resulting geographic/vertical/seasonal distribution of ozone [Larry Horowitz, USA]	Accepted:This will be clarified
8-961	8	33	24	33	25	The species (precursor?) dependence here is presumably driven by the location/distribution of the resulting ozone. It would help to state this here. [Oliver Wild, United Kingdom]	Accepted:This will be clarified
8-962	8	33	29	33	31	Is this sentence necessary? [Ruth Doherty, UK]	Accepted:This sentence will be removed

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8-963	8	33	37	33	5	Should indicate which gases are Montreal Gases and which are Halocarbons. [Robert Portmann, United States of America]	Accepted:This information will be added
8-964	8	33	43	33	44	There is also clearly a decceleration since 1990. [Fiona O'Connor, United Kingdom of Great Britain & Northern Ireland]	Accepted:This will be added.
8-965	8	34	6	34	7	About the sentence: "The ozone precursor forcing can be attributed to between the different precursor species." Please, explain "...attributed to between…". [Rubén D Piacentini, Argentina]	Editorial:Typo corrected
8-966	8	34	7	34	12	If the system is non-linear, why do the individual species' contributions sum to the total. Are the species increased successively from pre-industrial to present values, or are all but one species held at pre-industrial levels for each calculation? [Larry Horowitz, USA]	Taken into account: This paragraph will be re-written
8-967	8	34	9	34	12	Note also that changing emissions of more than one precursor at a time will also lead to different estimates of the response from each precursor. [Oliver Wild, United Kingdom]	Rejected:The non-linearity is already discussed
8-968	8	34	22	34	22	"indirect affect" should be "indirect effect" [BEGONA ARTINANO, SPAIN]	Editorial:Typo corrected
8-969	8	34	29	34	29	About: "Figure 8.17: Time evolution of the forcing of short-lived components from 1850 to 2010." In this sentence as well as in the vertical axis of the figure, please use the same expression, as in previous figures, for Radiative Forcing instead of only "forcing". [Rubén D Piacentini, Argentina]	Accepted:Will replace with "Radiative forcing"
8-970	8	34	34	34	34	Table 8.6: Table caption should state the forcing is both LW+SW. [Helen Worden, USA]	Accepted:Will add LW+SW forcing
8-971	8	34	34			Table 8.6 seems odd. Some our individual studies and others are generaly assessments - I don't think they mix or have equal weight [Piers Forster, UK]	Noted: There will be more values here for the SOD
8-972	8	34	37	35	18	As mentioned above, this radiative forcing for stratospheric ozone changes does not reflect its importance in the climate system. A word to this effect would be good, in order to link better to chapters 11 and 12 (where this point is also made). [Olaf Morgenstern, New Zealand]	Rejected:This chapter focusses on radiative forcing. Non-radiative effects are already mentioned sufficiently.
8-973	8	34	41	34	31	Change "lower stratosphere" to "lowermost stratosphere". [Robert Portmann, United States of America]	Accepted: Will change wording
8-974	8	34	43	34	43	I think this is confusing. Doesn't a decrease in stratospheric ozoen increase the short wave flux into the troposphere? [Robert Waterland, United States of America]	Accepted:Will rephrase
8-975	8	34				Table 8.6: It would be very useful to include the AR4 estimates of trop and strat O3 RF in here, too. The table would be easier to read if the trop and strat columns were placed side by side [Oliver Wild, United Kingdom]	Accepted: AR4 values will be added
8-976	8	35	8	35	8	I would avoid saying "It starts in the late 1970s". Ozone depletion began before this. [John Daniel, USA]	Accepted: Will change wording
8-977	8	35	8	35	8	Add "to decline" after "It starts" [Larry Horowitz, USA]	Accepted: This will be rephrased
8-978	8	35	11	35	15	There are two very different thoughts expressed in this paragraph. [John Daniel, USA]	Taken into account: This paragraph will be rephrased
8-979	8	35	17	35	18	see also Velders et al., PNAS, 2007. [Olaf Morgenstern, New Zealand]	Rejected:This paper refers to the impact of the Montreal Protocol that we aren't discussing here.
8-980	8	35	17			might mention that this refers to the aggregate; text isn't necessarily true for some chemicals (halons, for example) [Stephen Montzka, USA]	Taken into account: This paragraph will be expanded.
8-981	8	35	22	35	40	I realize that you have stated that stratospheric water vapor changes not due to CH_4 represent a feedback and not a forcing. However, somewhere (perhaps it is in another chapter?) you should discuss the changes (and the associated impacts on the Earth's energy balance) that have occurred recently (e.g., Solomon et al., Science, 2010). If this is omitted, modelers will be missing a term that is important for understanding climate changes on decadal-type time scales. [John Daniel, USA]	Rejected:This is not relevant to this chapter. It might be in chapter 10.
8-982	8	35	26	35	26	Solomon et al 2010b is a good citation for the impact of an H_2O increase in the stratosphere on radiatiove forcing. Is it also a good citation for the cause of a stratospheric H_2O trend? [Rolf Mueller, Germany]	Rejected:This is not relevant to this chapter as here we only discuss the changes driven by methane

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							oxidation
8-983	8	35	27	35	27	The increase of stratospheric water vapour through vulcanic eruptions is not an established fact as it sounds here. Suggest formulating more carefully here. [Rolf Mueller, Germany]	Rejected:We do not make any statement here about the magnitude of the volcanic impact, simply that it can happen.
8-984	8	35	33	35	36	Change "intercomparison studies of change in stratospheric water" to "intercomparison studies of the RF from stratospheric water". [Robert Portmann, United States of America]	Rejected: The current sentence indicates that the uncertainty is in the magnitude of the changes in water vapour.
8-985	8	35	39			This is the only mention of aircraft impacts on RF . Although it is discussed in chapter 7 somewhere in section 8.4 a sentence or two on contrails and their RF should be added as its impact is shown in figure 8.24 and Table 8.8 [Ruth Doherty, UK]	Taken into account: Contrails will be very briefly discussed in 8.3.4
8-986	8	35	42	35	42	The division of the discussion of aerosols is both Chapter 7 and Chapter 8 is confusing. It seems logical to have the full discussion in one place, but if that's not possible, it would be useful to clearly dilineate what is covered in Chapter 7 vs. Chapter 8. If the focus of Chapter 7 is on aerosol-cloud interactions, and not direct aerosol forcing, perhaps the chapter title could be "Clouds and Aerosol-Cloud Interactions." Estimates of the direct RF of aerosols are given in both chapters, and it confuses the discussion in Chapter 8. [Susan Anenberg, USA]	Taken into account - the discussion of aerosols in Chapter 8 is reduced and has more a summary. It is also stated clearer the division between Chapter 7 and 8.
8-987	8	35	42			Sec 8.4.4: Liaise with authors of Ch 7 to avoid duplication and ensure consistent terminology. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	See 986
8-988	8	35	46	36	12	I really like this approach of stating, up front, what AR4 found and then you provide an update. Please consider such an approach for all major sections. [John Daniel, USA]	Noted
8-989	8	35	46	40	3	Again you ignore the diference between day and night. By day clouds and aerosols cool the earth, By night they warm it. [VINCENT GRAY, NEW ZEALAND]	Rejected, diurnal and seasonal variations are taken into account in all model calculations
8-990	8	35	51	35	52	Please, specify clearly that you are talking about aerosol direct radiative forcing at the TOA (in contrast to surface radiative forcing). Same apply to whole page 36. It is necessary to specify the RF at TOA since situation is quite different at the surface. [Petr Chylek, USA]	Taken into account - it is now stated that RF for the direct aerosol effect is taken at TOA. By definition is RF at the tropopause but taken for aerosols at TOA since it is similar to tropopause values
8-991	8	35	51	35	52	Replace "Scattering aerosols exert a negative RF, whereas strongly absorbing components give a positive RF" with "An increase in the quantity of scattering aerosols exerts a negative RF, whereas increasing the amount of strongly absorbing components give a positive RF" [Robert Waterland, United States of America]	Taken into account - text revised as suggested
8-992	8	35	52	35	52	It depends on the underlying albedo, be it surface or cloud, rather than just the surface albedo. [Olivier Boucher, France]	Taken into account - text revised such that 'surface' is removed
8-993	8	35	53	35	53	Replace "the net direct aerosol effect and a" with "the change in the net direct aerosol effect since 1750 with a". [Robert Waterland, United States of America]	Taken into account - text revised as suggested
8-994	8	35		39		The separation of aerosol effects into direct, semi-direct, indirect, lifetime and albedo and, on top of that, all of them separated into RF and AF. This is indeed very confusing and hard to comprehend. [Henning Rodhe, Sweden]	Taken into account - a new terminology is introduced and we refer to a figure in Chapter 7 illustrating the new terminiology.
8-995	8	35				Section 8.4.4: There is a strong overlap with sections in chapter 7. [Twan Van Noije, Netherlands]	Taken into account - see comment 8-986
8-996	8	36	7	36	7	You need to define the cloud lifetime and semi-direct effects. [Robert Waterland, United States of America]	Taken into account - a new terminology is introduced and we refer to a figure in Chapter 7 illustrating the new terminiology.
8-997	8	36	7	36	9	With respect to the sentences: "and the semi-direct effect were not in accordance with the radiative forcing concept, because they involve tropospheric changes in variables other than the forcing agent, so no best RF estimates were provided in AR4 (see Section 8.1). However, the cloud lifetime effect". The same as before with respect to the use of "cloud lifetime effect", in the sense that it was not considered to be an appropriate	Taken into account - sentence modified to make it clearer that it refered to AR4.

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						expression for the definition of the effect in Chapter 7. [Rubén D Piacentini, Argentina]	
8-998	8	36	7	36	9	I disagree that these effects were not in accordance with the radiative forcing concept. The changes were just very difficult to estimate from a model. The cloud albedo effect also involves change in variables other than the forcing agent so the reason given is false. Here is another example, the indirect forcing of stratospheric water from methane would be estimated from a model and used as input in a RT code to compute this indirect RF. This is easier but conceptually no different than using a model to estimate changes in average cloud fields due to the cloud lifetime effect of aerosols and using them in a RT model to compute the indirect RF. [Robert Portmann, United States of America]	Taken into account - It is added 'strictly' as well as the following text 'at least in the available model estimates'
8-999	8	36	12	36	12	You may refer to section 7.3 rather than Chapter 7 as a whole. [Olivier Boucher, France]	Accepted - text revised as suggested
8-1000	8	36	13			Chapter 7 forcing numbers are weighted by observations so I think this is incorrect [Piers Forster, UK]	Taken into account - text revised with statement that also observations are used in the assessment of the estimates.
8-1001	8	36	14	37	26	Need to discuss implications of mixing state assumptions for calculated aerosol DRF in this section. What is known about mixing state from observations, what is commonly assumed in models, what are the implications for DRF? [Larry Horowitz, USA]	Rejected - such details belong to Chapter 7
8-1002	8	36	16	36	17	Replace "Several aerosol components contribute to the direct aerosol effect, most of them mainly scatter solar radiation whereas" with "All aerosol components contribute to the direct aerosol effect; most of them by scattering solar radiation, whereas". [Robert Waterland, United States of America]	Accepted - text revised as suggested
8-1003	8	36	16	36	24	There is a subsection in section 7.3 on this which you may want to cross-reference. SZA is also a very important parameter for the local RF. Line 20 refers to the "RF by the direct effect" rather than the "total direct aerosol effect". [Olivier Boucher, France]	Taken into account - text revised in accordance with the suggestions
8-1004	8	36	17	36	18	RF depends on changes in the mixture of aerosols and their properties as well as the aerosol loadings. [Robert Waterland, United States of America]	Accepted - text revised as suggested
8-1005	8	36	20			It is not mentioned here that anthropogenic dust is excluded. A simple sum of the direct effects of table 8.7 results in -0.4 W m-2. Is there ground to exclude anthropogenic dust from the total DRF? [Michael Schulz, Norway]	Taken into account - text revised so the importance of dust for the total is specific mentioned.
8-1006	8	36	21	36	21	Replace "thus weaker" with "lower". [Robert Waterland, United States of America]	Rejected, we prefer to use weaker and stronger about negative numbers so no confusion about lower means less or more negative
8-1007	8	36	23			could add aerocom website [Ruth Doherty, UK]	Accepted - website included as suggested
8-1008	8	36	26	36	28	Radiation forcing is discussed for mineral aerosols, a powerful source. The radiative forcing of sea-salt, another powerful source, is not discussed, but see J. Li, X. Ma, K. von Salzen, and S. Dobbie (2008): Parameterization of sea-salt optical properties and physics of the associated radiative forcing. Atmos. Chem. Phys., 8, 4787–4798. The radiative forcing of primary biological particles, another powerful source, is not discussed. While the source area for mineral dust and sea salt aerosol is only a fraction of the earth surface, the source area of the primaty biological particles is the whole earth. Excerpt from a review (available upon request) about primary biological aerosol particles: "The biosphere, or the system in which all living things interact, dominates the Earth's surface, influencing the composition of land, water, and air. Thus, PBAP can be released, both actively and passively, from every region of the globe. Key PBAP-producing systems include: Plants release PBAP in the course of decay processes (Sect. 2.7: Others) as well as for reproduction, including pollen from higher plants and spores from ferns and mosses (Sect. 2.3). Microorganisms inhabit most plant, soil and rock surfaces (Sect. 2.1: Bacteria and Archaea; 2.2: Fungi, 2.5: Algae and Cyanobacteria; 2.6: Biological crusts and lichen). These microorganisms can be very numerous, contributing huge number concentrations per unit surface area (104 to 108 cells/cm2) in various natural environments (Morris and Kinkel, 2002; Lindow and Brandl, 2003; Yadav et al., 2004). Further, the global leaf	Rejected, this chapter is about changes in anthropogenic or natural compoents. We discuss dust since there is indications of anthropogenic influence to emissions of dust. We have no information on change in PBAP.

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						surface area is estimated to be roughly four times the terrestrial ground surface area (~6.4*10^8 km^ vs. ~1.5*10^8 km^2), which provides a correspondingly large surface area for PBAP emission (Whittaker and Likens, 1973). PBAP originating from animals and humans include debris from skin or hair as well as, for example, excrements, brochosomes and eggs dispersed into the atmosphere by insects (see Sect. 2.7: Others). In areas of human activity such as cities or agricultural managed areas the numbers and composition of microorganisms like bacteria or fungi are often increased and altered with respect to rural areas (see Sects. 2.1.1: Urban airborne bacteria; 2.2: Fungi). The cryosphere (e.g., Greenland, Antarctica, glaciers) is formed largely from precipitation, and this may be triggered by PBAP in some situations (Sands et al., 1982; Christner et al., 2008b; Pöschl et al., 2010). Bacteria have been discovered in ice cores from Antarctica at depths up to 3519 m (Raymond et al., 2008), giving possible evidence to the idea that these organisms have been introduced through precipitation. Thus, surface snow under windblown conditions could be a powerful source for PBAP via resuspension (Pomeroy and Jones, 1996). Roughly 70% of the globe is covered by oceans. They are full of living and decaying organisms such as bacteria, archaea, fungi, and algae, which are ejected from the ocean surface by bubble bursting mechanisms, similar to the way other particles (e.g., sea salt) are emitted from such surfaces (Blanchard, 1983; O'Dowd et al., 2004), (see Sects. 2.1.3 and 2.5)." [Ruprecht Jaenicke, Germany]	
8-1009	8	36	26	36	28	If mineral dust is included in the exercise, than primary biological particles should be included as well. Those biological particles cover the size range effective on solar radiation and radiative forcing (smaller 1 µm). Based on Jaenicke, R. (2007): Is Atmospheric Aerosol an Aerosol? - A Look at Sources and Variability. Faraday Discussions 137, 235-243 a rough (back on the envelope) estimate for source strength could result in 500 Tg/yr biological material for particles smaller 1 µm in radius. That amount is comparable in that size range to sea salt (500 Tg/yr), continental particles (210 Tg/yr), desert dust (720 Tg/yr). [Ruprecht Jaenicke, Germany]	Rejected, this chapter is about changes in anthropogenic or natural compoents. We discuss dust since there is indications of anthropogenic influence to emissions of dust. We have no information on change in PBAP.
8-1010	8	36	26	36	45	There should be a discussion here on the effect of internal vs external mixing between light-absorbing and light-scattering aerosols. The degree of internal to external mixing influence the total effect of the aersol components in a non-linear way. It is also considerable importance what albedo the underlying surfaces have (ground or clouds). This is quite throughly discussed in : Seland, Ø., T. Iversen, A. Kirkevåg, T. Storelvmo. (2008) Aerosol-climate interactions in the CAM-Oslo atmospheric GCM and investigation of associated basic shortcomings Tellus 60A, 459-491. DOI: 10.1111/j.1600-0870.2008.00318.x [Trond Iversen, United Kingdom of Great Britain & Northern Ireland]	Rejected - This kind of information will be available in Chapter 7.
8-1011	8	36	26	37	20	Please be clear about what distinction you are making between biofuels and biomass burning - how these are defined - and check the use throughout to make sure you have been consistent. [Susan Solomon, USA]	Taken into account - covered in Chapter 7
8-1012	8	36	27	36	27	I would put "(BB)" after "biomass burning". [Dirk Olivié, Norway]	Accepted - text revised as suggested
8-1013	8	36	30			A source of confusion with BC RF effects is the assumption required concerning the amount of pre-industrial emisisons. I don't see that addressed in presenting results such as those in Fig. 8.18 and 8.19. [David Fahey, USA]	Rejected - this issue is most related to BC emissions from BB. The BC emissions from FF and BF at pre-industrial time is quite small. It is taken into account differences in BC emissions from 1750 to 1850.
8-1014	8	36	32	36	33	Some readers might think that BB RF that is close to zero means it is unimportant, so it might be worth pointing out that a substantial surface forcing and atmospheric heating is implied. [Leon Rotstayn, Australia]	Taken into account - covered in Chapter 7
8-1015	8	36	38			More modest - is this a problem. If things haven't changed perhaps it means our understanding is robust. These sections could be shorter and point to chapter 7, it would help avoid inconstitencies [Piers Forster, UK]	Rejected - the summary section is kept at the length of FOD to allow the reader to read Chapter as a stand alone chanpter.
8-1016	8	36	39	36	40	Say why it is weaker in a few words here. [Susan Solomon, USA]	Taken into account - RF of sulphate is after a re-evaluation from FOD the same as the AR4 value
8-1017	8	36	40	36	44	I realise that these sentences refer to biogenic SOA, which can also be altered by anthropogenic emissions. There is also evidence that SOA associated with combustion substantially enhances concentrations of OA (e.g., Grieshop et al., 2009) Iinuma et al., 2010), and these are not generally included in emission inventories.	Rejected - Chapter 8 just summarize the assessment in Chapter 7 and we find it sufficiently to refer to one review paper. Chapter 7 has more details.

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						Should this be mentioned somewhere? References: Grieshop, A. P., Logue, J. M., Donahue, N. M., and Robinson, A. L., 2009: Laboratory investigation of photochemical oxidation of organic aerosol from wood fires 1: measurement and simulation of organic aerosol evolution, Atmos. Chem. Phys., 9, 1263–1277, doi:10.5194/acp-9-1263-2009. Iinuma, Y., Boge, O., Grafe, R., and Herrmann, H., 2010: Methyl-Nitrocatechols: Atmospheric Tracer Compounds for Biomass Burning Secondary Organic Aerosols, Envir. Sci. Technol., 44, 8453–8459, doi:10.1021/es102938a. [Leon Rotstayn, Australia]	
8-1018	8	36	51	37	2	I like this table very much. Other sections could benefit by showing comparison tables with at least AR4 so it is clear how our understanding has changed. My only suggestion here is that you could add a column that contains references on which the revised estimates (or new estimates) are based. [John Daniel, USA]	Rejected - This kind of information will be available in Chapter 7.
8-1019	8	36	51	37	2	What assumptions are being made in this table regarding mixing state. If internal mixtures are considered, how is attribution to components handled? [Larry Horowitz, USA]	Rejected - This kind of information will be available in Chapter 7.
8-1020	8	36	51			The emissions from biomass burning has both negative and positive components. This should be explicitly mentioned (and possibly quantified). A change in burning conditions could change the relative importance of these components. Higher temp -> less BC. [Henning Rodhe, Sweden]	Rejected - This is already mentioned on page 36 line 32-35 in FOD
8-1021	8	36	51			Table 8.7: you need to reference the date from which changes in RF are computed. Was it 1750? [Robert Waterland, United States of America]	Taken into account - text revised as suggested
8-1022	8	36				Table 8.7: Having the comparison to earlier reports is very good. However, the discussion somehow belongs into chapter 7, since the changes require more explanations than provided in chapter 8. I guess thats a harmonization discussion. [Michael Schulz, Norway]	Rejected - it has been decided to have a summary and comparison with previous estimates in Chapter 8. This is also consistent with treatment in the synthesis part of Chapter 8.
8-1023	8	36				Tables 8-7, 8-9 are very valuable. Would be even more valuable with sums at the bottom of each column (with uncertainties). This would seem essential. [Stephen E Schwartz, USA]	Taken into account - the sum is included
8-1024	8	37	5	37	5	This sentence tries to compare apples (Wm-2yr-1) with oranges (Wm-2). I think it should be said that past RF are more uncertain than current RF, but even that is not quite true. The RF in 1750 is very well known as it is 0, but the current RF is not well known because we don't know the state of the atmosphere in 1750. [Olivier Boucher, France]	Taken into account - text revised to 'The time evolution of the RF of the direct aerosol effect at some time periods is more uncertain than the current RF'
8-1025	8	37	5			I'm not sure this use of RF is inconsistent with the basic RF definition given in the Introduction. The contribution of aerosols to the current radiative budget is better estimated but RF computation needs aerosol loading estimates from 1750 and those are poorly understood. It seems to me that changes in RF, particularly recent changes, are better understood than absolute RF values - is that what you were trying to say? [Robert Waterland, United States of America]	Taken into account - see 8-1024
8-1026	8	37	9	37	11	How significant are the uncertainties in pre-industrial emissions (particularly biomass burning) for estimates of aerosol DRF? [Larry Horowitz, USA]	Taken into account - the following modification to the text has been included 'The uncertainty in the biomass burning emissions increases backward in time and also uncertain in 1750. For the other aerosol components 1750 emissions are negligible.'
8-1027	8	37	12	37	12	Delete "and" [Olivier Boucher, France]	Taken into account - text revised as suggested (must be done)
8-1028	8	37	28	40	3	the indirect effect and BC on snow sections seem large and repeat a lot of chapter 7, I think these should only summarise chapter 7 and hadley have any independent references a all [Piers Forster, UK]	Noted - tighter coordiantion with chap 7 is done
8-1029	8	37	30	37	34	It would be nice to cross-reference section 7.4 here. [Olivier Boucher, France]	Noted
8-1030	8	37	33	37	33	Add "particles". "cloud water and ice particles" [Olivier Boucher, France]	Noted
8-1031	8	37	34	37	34	Clarify that "warm clouds" are clouds that have no ice. [John Daniel, USA]	Noted

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8-1032	8	37	42	37	44	This sentence isn't clear to me. [Leon Rotstayn, Australia]	Noted
8-1033	8	37	45	37	45	Where is the -1.3 W/m2 coming from? We don't have -1.3 W/m2 as a median value for the RF in chapter 7, but report a -1 W/m2 instead [Ulrike Lohmann, Switzerland]	Noted - it is meant to be consistent with the summary of chapter 7
8-1034	8	38	1	38	1	same comment, I think the -1.3 W/m2 should be a -1 W/m2 [Ulrike Lohmann, Switzerland]	Noted
8-1035	8	38	1	38	1	About: "Figure [7.1 x])". Remember to complete the "x" value. [Rubén D Piacentini, Argentina]	Noted
8-1036	8	38	1	38	2	This sentence also isn't clear to me. How can the adjustments enhance the RF in all models or compensate one another? [Leon Rotstayn, Australia]	Noted - this discussion is to be shifted to chapter 7
8-1037	8	38	4	38	5	"…observational estimates provide only a measure of the AF." Please clarify that observations can only provide very rough estimates of AF, since AF is defined such that surface temperatures must stay constant, yet observations allow surface temperatures to vary. [Mark Z. Jacobson, U.S.A.]	Noted
8-1038	8	38	4	38	19	Add discussion of Hansen's (ACP, 2011) AIE estimate, and include in other parts of the chapter where appropriate. [John Daniel, USA]	Noted
8-1039	8	38	4	38	19	This is an interesting paragraph, but some of it might fit better in Chapter 7, e.g. the discussion of open versus closed cellular convection is quite small-scale process-oriented. As a simple climate modeller, I tend to think of the problem with the "cloud-lifetime effect" as due to smaller droplets causing more evaporation and entrainment (e.g. Jiang et al., 2006, from Chapter 7), though I realise it is more complex than this. Probably, though I guess this is obvious, there needs to be a detailed discussion with Chapter 7 regarding what goes into which chapter, e.g. some of their material about inverse calculations of aerosol indirect effects (page 7-49) might fit better in Chapter 8. [Leon Rotstayn, Australia]	Accepted - much of this dicussion will be integrated in chapter 7
8-1040	8	38	6	38	6	Why are you only citing one study (Lebsock et al., 2008) to quote an indirect effect from observation and not the mean of the satellite studies that we report in chapter 7 on page 49? [Ulrike Lohmann, Switzerland]	Noted - this is because it is one of the very few stude is that separated water path changes from drop size changes as well as separating out effects of drizzle - practically all other satellite studies have a much limited and ambiguous scope. Furthermore this important difference between satellite observations was not acknowledged in chap 7 FOD and this will now be intregrated into ch 7.
8-1041	8	38	10	38	19	This is an extremely important point - and it's a key advance since AR4. Please ensure that it is highlighted better, including in the executive summary [Susan Solomon, USA]	Accepted - this will be a key conclusion drawn from chapter 7
8-1042	8	38	12	38	13	Compare apples (cloud albedo) with oranges (RF). [Olivier Boucher, France]	Noted
8-1043	8	38	15	38	15	Delete "which" [Larry Horowitz, USA]	Noted
8-1044	8	38	16	38	16	Change "dues" to "due". [Steven Ghan, USA]	Noted
8-1045	8	38	16	38	16	Change "dues" to "due" [Larry Horowitz, USA]	Noted
8-1046	8	38	17	38	17	Change "in the presence of" to "from the". [Steven Ghan, USA]	Noted
8-1047	8	38	21	38	22	Make it clear this is from models. [John Daniel, USA]	Noted
8-1048	8	38	21	38	23	Figure 8.20 needs more explanation. What is the source for the data shown? Which are modelling results, which are measurements? [Robert Waterland, United States of America]	Taken into account, figure removed.
8-1049	8	38	22	38	22	Is it really true that sulphate direct effect does not initiate rapid responses? It still strongly modifies the distribution of radiative fluxes within the atmosphere and at the surface, so responses must occur, even if they are smaller than those for absorbing aerosols. [Nicolas Bellouin, United Kingdom]	Accepted

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8-1050	8	38	24	38	24	Add "effect" after "semi-direct" [Larry Horowitz, USA]	Accepted, new terminology used.
8-1051	8	38	24	38	24	"semi-direct effect". [Leon Rotstayn, Australia]	Accepted, new terminology used.
8-1052	8	38	28	38	28	Change "adjusts" to "adjustments". [Steven Ghan, USA]	Accepted
8-1053	8	38	42	39	7	You could discuss the semi-direct effect immediately after the direct effect to follow the Chapter 7 approach. [Olivier Boucher, France]	Noted
8-1054	8	38	46	38	46	Not quite true as you need to know environmental conditions (clouds, albedo, etc…) pretty well as well. [Olivier Boucher, France]	Noted
8-1055	8	38	46	38	46	“concentrations are properties” should maybe be “concentrations and properties”. [Dirk Olivié, Norway]	Accepted
8-1056	8	38	48	38	52	Please clarify why this is considered a special type of forcing, as compared e.g. to carbon dioxide, whose warming effects could also reduce cloud cover in some regions. [Susan Solomon, USA]	Accepted This is now made clear in section 1 through a careful construced definition that categorie=zes adjustmetns by time scale - CO2 and semi-direct differn from a perspecitve of the time scale of eh adjustmem=nt.
8-1057	8	39	2	39	2	"…is weak in many GCMs". Does this mean the forcings are too weak? It isn't clear what is meant. [Leon Rotstayn, Australia]	Noted
8-1058	8	39	4	39	4	Add "effect", ie "The semi-direct effect …" [Olivier Boucher, France]	Accepted
8-1059	8	39	4	39	4	insert "effect" after "The semi-direct" [Gerd Folberth, United Kingdom of Great Britain & Northern Ireland]	Accepted
8-1060	8	39	4	39	4	Add "effect" after "semi-direct" [Larry Horowitz, USA]	Noted
8-1061	8	39	4			Change "Because absorption by ice is very week…" to "Because absorption by ice is very weak…" [Pierre Bernier, Canada]	Accepted
8-1062	8	39	10	38	55	This section seems more like a review than an assessment. Please assess what you have found. [Susan Solomon, USA]	Taken into account - we have added some assessment sentences.
8-1063	8	39	10	39	10	"weak" rather than "week" [Olivier Boucher, France]	Accepted - text revised
8-1064	8	39	10	39	10	Change "week" to "weak" [Larry Horowitz, USA]	Accepted - text revised
8-1065	8	39	10	39	10	Change "spectra" to "wavelengths" [Larry Horowitz, USA]	Accepted - text revised
8-1066	8	39	10	39	10	Change "make" to "makes" [Larry Horowitz, USA]	Accepted - text revised
8-1067	8	39	10	39	10	Replace "very week at visibke and UV spectra" with "very weak at visible and UV wavelengths". [Robert Waterland, United States of America]	Accepted - text revised
8-1068	8	39	10	39	55	Refer to recent AMAP (2011) report on black carbon impacts on Arctic climate and importance of source emitted near to the Arctic. Whilst actual changes to albedo might be small, longer term impacts such as earlier snow-melt, may be important. [Katharine Law, France]	Accepted - text revised
8-1069	8	39	10	39	55	The level of detail for BC on snow and ice is much more than elsewhere. I suggest to harmonize that in the chapter and reduce it here. [Ulrike Lohmann, Switzerland]	Taken into account - A lot of detail has been removed in this revsion
8-1070	8	39	10			Weak not week. [Pieter Aucamp, South Africa]	Accepted - text revised
8-1071	8	39	20	39	20	Change "is" to "are" [Larry Horowitz, USA]	Accepted - text revised
8-1072	8	39	20	39	26	It is interesting that for BC on snow, the efficacy is >>1, while it was also stated in 8.1.1 that AF/RF >> 1. Are	Taken into account - those sentences have been

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						we in danger of double-counting? I haven't actually compared all these papers, but does the large efficacy occur when the RF method is used (reflecting the excitation of the surface-albedo feedback), whereas does the AF method includes the efficacy in the "forcing", so that AF/RF is >>1? It would be good to clarify the relationship here. [Leon Rotstayn, Australia]	deleted
8-1073	8	39	22	39	23	“The mean efficacy in one model from five experiment/control pairs was ...”: a bit unclear. [Dirk Olivié, Norway]	Taken into account - those sentences have been deleted
8-1074	8	39	24	39	27	Somehow this very large efficacy does not make sense, and I wonder if it shoud be quoted in an assessment. [Olivier Boucher, France]	Taken into account - those sentences have been deleted
8-1075	8	39	29	39	29	change "albedo" to "surface albedo" [Olivier Boucher, France]	Accepted - text revised
8-1076	8	39	29	39	30	Replace "is simulated to –0.12% for the global mean, and –1.1% for the Arctic" to "is estimated to be reduced by 0.12% fpr the global mean, and by 1.1% for the Arctic". [Robert Waterland, United States of America]	Accepted - text revised
8-1077	8	39	33	39	35	These two sentences are contradictory [Larry Horowitz, USA]	Accepted - text revised
8-1078	8	39	33	39	35	The sentence beginning "Deposition of BC onto Greenland" and the following sentence seem to be inconsistent. [Robert Waterland, United States of America]	Accepted - text revised
8-1079	8	39	33	39	48	Most of that material could go in the "RF time evolution" section. [Olivier Boucher, France]	Rejected, the time evolution of BC on snow has been treated similar to the other forcing agents with a short text on the time evolution in each section. The text for the time evolution of BC on snow is shortened.
8-1080	8	39	46	39	46	About the sentence: "It is straightforward to deduce this radiative effect if the aerosol concentrations are properties are known." Please verify the use of the first word "are" before "properties". In principle, it should be: "It is straightforward to deduce this radiative effect if the aerosol concentrations properties are known." [Rubén D Piacentini, Argentina]	Taken into account - text revised.
8-1081	8	39	55	39	55	About the expression: "...reduction in the Artic region in accordance to measurements is a probably a major cause." Please verify the use of the first word "a". In principle, it should be: "….reduction in the Artic region in accordance to measurements is probably a major cause." [Rubén D Piacentini, Argentina]	Accepted - text revised
8-1082	8	39				Section 8.4.4.5: Again, there is a strong overlap with Section 7.3.5.5. [Twan Van Noije, Netherlands]	Taken into account - A lot of detail has been removed in this revsion
8-1083	8	40	5	42	41	Good land use section, could be slightly shorter [Piers Forster, UK]	Noted
8-1084	8	40	8	42	42	You ignore most of the antropogenic changes due to land us change, The most important is the attempts to reduce convection and the turbulence that enhances it. This convection cools the earth by day and humans put up shelter belts, entire cities, buldings of all sorts with the ob ject of reducing this daytime cooling, and so causeing warming. By night these changes provide a thermal reservoir so it causes warming in this way. Humans also carryouit measures to reduce evaporation by culverting rivers, reservoirs, cobering land with concrete and buildings. The largest anthropogenic effects on carbon dioxide are farming and forestry which remove it.. [VINCENT GRAY, NEW ZEALAND]	Rejected. We certainly do not ignore the anthropogenic changes due to land use change. The impact of deforestation on the Carbon Dioxide concentration in the atmosphere is dealt with in Chapter 6. The impact of town is very local and somewhat discussed in the section
8-1085	8	40	22	40	40	A new paper discussing the climate effects of all urban surfaces worldwide is Jacobson, M.Z., and J.E. Ten Hoeve, Effects of urban surfaces and white roofs on global and regional climate, J. Climate, 25, 1028-1044, doi:10.1175/JCLI-D-11-00032.1, 2012. [Mark Z. Jacobson, U.S.A.]	Taken into account - We agree that this reference is relevant. It will be added to section 8.4.5.4
8-1086	8	41	4	41	10	An important point moving beyond AR4, since observational data was very limited at that time. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted
8-1087	8	41	8	41	9	O'Halloran et al. used observational data and RF model kernals to estimate the change in albedo with disturbance from fire, forest mortality from insects, and hurricanes. - Albedo increases when mountain pine beetle-killed trees defoliate due to the exposure of underlying snow. This effect changes in time, as needles, branches and standing dead trees persist for different amounts of time and understory regrowth compensates	Rejected. The disturbances from insects, huricanes and fires are mostly "natural" and therefore not fully relevant to the RF concept.

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						for loss of overstory - the albedo effect offsets the radiative forcing associated with the CO2 release caused by the beetles over years after the event. - The large increase in albedo following fire due to exposure of underlying snow effectively offsets the heating associated with CO2 released from the fire. This is more of an issue in areas with prolonged snow cover. 11. O'Halloran, T., B.E. Law, M.L. Goulden, Z. Wang, J.G. Barr, C. Schaaf, M. Brown, J. Fuentes, M. Göckede, A. Black, V. Engel. 2011. Radiative forcing of natural forest disturbances. Global Change Biology, DOI: 10.1111/j.1365-2486.2011.02577.x. [Beverly Law, USA]	
8-1088	8	41	20	41	20	Striclty speaking, Lohila et al (2010) consider "forestation" (which may arise from either natural or anthropogenic processes) as opposed to "afforestation" as stated here. (This point is immaterial in the context of this particular sentence, which is only discussing the relative impacts of albedo vs CO2 effects - I merely point it out to avoid misunderstandings elsewhere.) [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted
8-1089	8	41	21	41	22	The results of Rotenberg et al (2010). On the contrary, [Pierre BRENDER, FRANCE]	Taken into account - We agree with the commentary. The text is slightly changed to make it clearer.
8-1090	8	41	21	41	22	the authors claimed that the reduction of Lwout due to the higher rugosity of the forest was doubling the SW effect. [Pierre BRENDER, FRANCE]	Taken into account. See 8-1089
8-1091	8	41	21	41	22	Together afforestation lead to a positive radiative forcing. See however the comments of Lee (2010) [Pierre BRENDER, FRANCE]	Taken into account. See 8-1089
8-1092	8	41	21	41	22	on why the metric comparing the radiative budget at the surface (and not the top of the atmosphere) overestimates [Pierre BRENDER, FRANCE]	Taken into account. See 8-1091
8-1093	8	41	21	41	22	the «RF itself (which should be computed at the top of the atmosphereng) [Pierre BRENDER, FRANCE]	Taken into account. See 8-1091
8-1094	8	41	21	41	22	1. Lee X. Forests and Climate: A Warming Paradox. Science. 2010 Jun 18;328(5985):1479. [Pierre BRENDER, FRANCE]	Taken into account. See 8-1091
8-1095	8	41	40	41	42	This is anecdotal. I suggest to remove this. [Olivier Boucher, France]	Rejected. The reference shows that land use change may have very large impacts localy. It is relevent in the chapter
8-1096	8	41	44	41	46	These remarks on the impact of ship wakes could go to the next subsection. [Twan Van Noije, Netherlands]	Accepted. Paragraph moved as suggested
8-1097	8	41	48	42	21	A very important paragraph, it is good to see discussion of non-radiative forcings, and I specifically support the use of that term instead of the cumbersome and confusing "initial non-radiative effects" used in AR4. It wil be important to retain this paragraph in future drafts, and ensure that the points it makes are mentioned in the Exec Summary, SPM and TS so that policymakers are aware of these important issues. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted
8-1098	8	41	51	41	54	Local temperature effects of land cover change: 9. Lee, X, M.L. Goulden, D.Y. Hollinger, A. Barr, T.A. Black, G. Bohrer, R. Bracho, B. Drake, A. Goldstein, L. Gu, G. Katul, T. Kolb, B.E. Law, H. Margolis, T. Meyers, R. Monson, W. Munger, R. Oren, K.T. Paw U, A.D. Richardson, H.P. Schmid, R. Staebler, S. Wofsy, L. Zhao. 2011. Observed sensitivity of local climate to deforestation in mid- and high latitudes. Nature 479: 384-387. DOI:10.1038/nature10588. [Beverly Law, USA]	Taken into account - We agree that this reference is relevant to the chapter. It is now included
8-1099	8	42	25	42	35	It is important to be really up-to-date with the text on these issues so that public or private investments in mitigation programs that involve land cover changes are done on projects that really achieve their stated goal. Although global modelling is certainly not my field, this conclusion appears too broadly neutral and potentially not consistent with current evidence. For example, see the modelling work of Aurora and Montenegro (Arora, V.K. and A. Montenegro. 2011. Small temperature benefits provided by realistic afforestation efforts. Nature Geoscience V. 4 , p. 514. DOI: 10.1038/NGEO1182) and of Betts et al (Betts, R.A., P.D. Falloon, K.K. Goldewijk, and N. Ramankutty. 2007.Biogeophysical effects of land use on climate: Model simulations of radiative forcing and large-scale temperature change. Agricultural and Forest Meteorology 142: 216-233 DOI: 10.1016/j.agrformet.2006.08.021) that both simulate a strong decrease in afforestation effectiveness for lowering total radiative forcing at high latitudes on account of albedo changes, pointing to a significant climate	Taken into account - The references have been assessed and are now included in section 8.4.5.4, except for Lacis et al (2010) which is not relevant in this section. We certainly agree with the conclusion of the comment

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						effect of land cover changes at these higher latitudes. The recent and excellent paper by Lee et al (Lee, X., et al. 2011. Observed increase in local cooling effect of deforestation at higher latitudes. Nature. 479:384-387. doi: 10.1038/nature10588) actually measure the local net cooling effect of deforestation through direct site-level air temperature measurements. In addition, increases in atmospheric water vapour concentration do not replace increases in temperature or available energy, but actually respond to them (Lacis, A.A, G.A. Schmidt, D. Rind, and R. A. Ruedy. 2010. Atmospheric CO2: Principal Control Knob Governing Earth's Temperature. Science, Vol. 330, No. 6002, pp. 356-359; DOI: 10.1126/science.1190653), and may in addition alter regional or global precipitation regimes (Swann, A.L.S., I.Y. Fung, and J.C.H. Chiang, 2012: "Mid-latitude afforestation shifts general circulation and tropical precipitation." Proceedings of the National Academy of Sciences, v. 109, no. 3, pp. 712-716, doi: 10.1073/pnas.1116706108.). So, even if the effects on air temperature balance out globally, the resulting increases in atmospheric water vapour would alter climatic patterns, a rather negative consequence. [Pierre Bernier, Canada]	
8-1100	8	42	25	42	35	The new central estimate of -0.15Wm-2 does not seem directly quantitatively supported in the text, is it just a slight downgrade from the 0.20 Wm-2 from AR4 because of the Myhre et al (2005) and Kvalevag et al (2010) arguments of a smaller albedo difference between vegetation and croplands mentioned on page 41? What about the contrasting conclusion from Nair et al (2007). I don't think the text as it stands fully justifies the change from the AR4 value, could a little more explanation be included? (For example, does revised information on historical land cover make any difference - I see the maps for 1750 RF from Pongratz et al (2009) in figure 8.22 show some areas with different RF than that estimated in the similar study by Betts et al (2007). The Pongratz et al study is probably better, so since the Betts et al (2007) study was used in AR4 as a way of normalised many studies to give RF estimates relative to 1750 rather than potential natural vegetation, the use of Pongratz instead of Betts may make a difference to the final RF estimate relartive to 1750. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	There are large uncertainties on the RF from land use change both in AR4 and the FOD of AR5. Although we suggest a slightly smaller value than in AR4, the uncertainty ranges overlap very much. In the SOD, we make it clearer that, although there are a few papers that sugggest a lower value than in AR4, the uncertainty range does not exclude the earlier estimate.
8-1101	8	42	25	42	35	Why aren't the direct measurements of the evolution of earth albedo performed at the Big Bear Solar Observatory mentioned, cited and discussed ? See Pallé, E., P.R. Goode, P. Montanes-Rodroguez, J. Geophys. Res. 114 (2008) 1029. [François GERVAIS, France]	Rejected. The Earth albedo is only slightly affected by the land surface albedo while main drivers are the clouds and snow cover. No conclusions can be drawn on the surface albedo changes, which is the main purpose of this section.
8-1102	8	42	44	46	3	Looking at the aerosol direct effect. It's changed from -0.5+/-0.4Wm-2 to -0.3Wm-2 +/- 0.3. However, one of the main reasons for this difference is that in AR4, the radiative forcing of mineral dust was estimated. Now it is not included (as far as I can tell). This should be explicitly mentioned in Table 8.9. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland]	Taken into account - this is discussed in section 8.3.4.2 with a total given including the dust.
8-1103	8	42	45	42	53	The convected air emits part of the energy orginally received by the earth from the sun, so that it provides an unknown fraction of the calculated radiative forcing.at the tropopause. [VINCENT GRAY, NEW ZEALAND]	Rejected, GCMs which include convection show that RF at the tropopause level is a good indicator of surface temperature changes
8-1104	8	42	55	46	3	The predominant importance of water vapour is ignored and that portion of the emitted RF coming from the heat taken from the surface by convection and evaporation is is also ignored. [VINCENT GRAY, NEW ZEALAND]	Rejected, the direct anthropogenic influence on water vapour is small, but water vapour as a feedback mechanisms is important and discussed elsewhere in the report (see FAQ8.1) In the RF concept which is just quatification of the radiative perturbation the tropospheric state are fixed (e.g. temperature and water vapour but also evaporation and convection). Changes in evaporation and convection are taken into account in quantification of feedback processes and simulations of surface temperature changes.
8-1105	8	43	1	43	14	The general discussion of confidence level based on evidence and agreement, as well as Fig 8.23, really belongs up in Chapter 1 as the concepts apply throughout the report. [JOHN OGREN, USA]	Taken into account, text is revised and Fig 8.23 is moved to Chapter 1. However, some text is kept since a small introduction to the discussion is needed in Chapter 8.

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8-1106	8	43	17	43	20	The assignment of "Medium" agreement and "High" confidence to the aerosol direct effect seems generous, given both the large change in central estimate since AR4 and the remaining large range (-0.6 to 0) [Larry Horowitz, USA]	Rejected, compared to several of the other forcing mechanisms the direct aerosol effect has a more well-documented range and also by different methods.
8-1107	8	43	17	43	20	Very low for confidence level for total aerosol indirect effect and semi-direct effect seem harsh to me. Low seems a better estimate. If nothing else the bounds on the total aerosol RF gives us some confidence in the estimates of these effect cannot be too far off. [Robert Portmann, United States of America]	Rejected - based on the evidence and the agreement our assessment is a very low confidence. The basis for the confidence level is further described.
8-1108	8	43	17	44	1	Do the evidence, agreement, and confidence level rankings refer to both the magnitude and direction of the RF estimate? If so, can the figure caption be more specific: "Confidence level for the RF estimate associated with each forcing agent for the 1750-2010 period"? [Susan Anenberg, USA]	Taken into account - text modifided as suggested in the Table 8.8 caption
8-1109	8	43	17	44	1	I like this table. It is good that you include the basis for uncertainty estimates and changes since AR4 columns. [John Daniel, USA]	Noted
8-1110	8	43	17	44	1	If there is a large difference over the last few decades compared with the entire period, you might separate the rows into the two distinct periods. [John Daniel, USA]	Rejected - the confidence level for most of the forcing agents is not substantially different from what shown in Table 8.8.
8-1111	8	43	17	44	1	For solar, you might want to separate solar cycle (11-yr) changes with long-term changes. You could also have separate rows for TSI and spectral changes. [John Daniel, USA]	Rejected, since this table focus on the changes over the indistrial era we do not see sufficiently strong arguments for singel out the 11-yr solar cycle. The solar section discuss further the smaller uncertainty in the change in TSI over the recent solar cycles compared to changes over longer time periods.
8-1112	8	43	17	44	1	Table 8.8. The uncertainy estimate seems confused with the evidence basis (e.g. for contrails). Where the uncertanity is derived from also seems vague and hard to trace. If you are making bould statements about decreased uncertainty compared to AR4, they need to be carefully supported. You could expand table to summarise the evidence and the agreement maybe. Each section should have a clear statement on exactly where uncertainty comes from [Piers Forster, UK]	Taken into account - by mentioning that further discussion of uncertainties are given in the various sections. Contrail has now a small sub-section in our chapter.
8-1113	8	43	17	44	1	We now return to your traditional inability to produce scientifically drived uncertainty estimates for all your various "forcing agents". What is the point of "improved understanding"?. How much is "robust"? [VINCENT GRAY, NEW ZEALAND]	Rejected, we follow the IPCC guidance note on the uncertainties
8-1114	8	43	20	43	20	Table 7.10a in AR4 provided levels of scientific understanding for indirect aerosol effects beyond the cloud albedo effect [Ulrike Lohmann, Switzerland]	Taken into account - by replacing 'Not available' with 'No major change'
8-1115	8	44	18	44	18	Replace "confindence" with "confidence". [Robert Waterland, United States of America]	Accepted - text revised as suggested
8-1116	8	44	20	44	22	Replace "The LOSU for direct aerosol effect, surface albedo, contrails and volcanic aerosols has been raised and are now at the same ranking as change in stratospheric and tropospheric ozone. This is due to an increased understanding of key parameteres and its uncertainty for the elevated RF agents" with "The LOSU values for the Direct aerosol effect, Surface albedo, Contrails and Volcanic aerosols have been raised and are now at the same ranking as those for change in Stratospheric and Tropospheric ozone. This is due to an increased understanding of key parameters and their uncertainties." [Robert Waterland, United States of America]	Accepted - text revised as suggested
8-1117	8	44	25	44	26	not true, see Table 7.10a in AR4 [Ulrike Lohmann, Switzerland]	Taken into account - text with the statement of cloud lifetime effect and semi-direct effect included for the first time is removed.
8-1118	8	44	26	44	26	Replace "confindence" with "confidence". [Robert Waterland, United States of America]	Accepted - text revised as suggested
8-1119	8	44	34	44	35	Replace "The RF bar chart with time evolution is shown in Figure 8.25a for the whole industrial era, whereas over the period 1980–2010 shown in Figure 8.25b." with "The time evolution of RF is shown in Figure 8.25a for the whole industrial era, and for the period 1980–2010 in Figure 8.25b." [Robert Waterland, United States of	Accepted - text revised as suggested

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						America]	
8-1120	8	44	35	44	35	Replace "due to" with "because". [Robert Waterland, United States of America]	Accepted - text revised as suggested
8-1121	8	44	39	44	39	Replace "over the last decades" with "since the 1960s". [Robert Waterland, United States of America]	Accepted - text revised as suggested
8-1122	8	44	41	44	41	Change "extend" to "extent" [Larry Horowitz, USA]	Accepted - text revised as suggested
8-1123	8	44	49	44	49	Replace "total anthropogenic RF has steadily increased (with a RF of around 0.7 W m-2) rather" with ""total anthropogenic RF has steadily increased to ~0.7 W m-2 rather" [Robert Waterland, United States of America]	Accepted - text revised as suggested
8-1124	8	45	5	45	15	It looks like volcanic forcing is positive in the recent periods. How can this be? I believe it just has to do with the baseline (zero) period definition. However, it could be confusing, so please provide some explanation. [John Daniel, USA]	Taken into account - the baseline have been changed
8-1125	8	45	5	45	15	The volcanic forcing at least since 2000 is inconsistent with the forcing in Solomon et al. (Science, 2011). By remaining constant over time, it is also inconsistent with your discussion earlier in the chapter where you talk about the increasing stratospheric aerosol optical depth. [John Daniel, USA]	Taken into account - the data for volcanic aerosols have been updated.
8-1126	8	45	5	45	15	This is another place where you could show stratospheric water vapor in panel a or b at least for the recent years. You could always label it as "feedback". [John Daniel, USA]	Rejected, Chapter 8 cover the forcing mechanisms and not the feedbacks
8-1127	8	45	5	45	15	Does the "Total GHG RF" in panel d include all anthropogenic forcing except for aerosols? Clarify the definition. It might be interesting to have just the well-mixed GHGs in one curve and also then well-mixed GHGs + all non-aerosol anthropogenic forcing agents. Even if the values are not that different, I expect the widths of the distributions to be different. [John Daniel, USA]	Taken into account - it is added in the text the following 'The GHG consists of WMGHG and ozone, and stratospheric water vapour'
8-1128	8	45	17		29	It might be mentioned in this para that a major change in thinking is that the several forcings can be treated additively to calculate the total forcing. [Stephen E Schwartz, USA]	Taken into account - the following text is included: 'An important assumption is that different forcing mechanisms can be treated additively to calculate the total forcing (see Haywood and Schulz, 2007; Boucher and Haywood, 2001)'
8-1129	8	45	24	45	26	Why is the change in the estimate for solar irradiance "caused mainly how the RF is calculated"? I would think for solar irradiance the method would not have much influence. Or is this a reference to the new SORCE uncertainty in solar cycle changes. Clarify. [Robert Portmann, United States of America]	Rejected, previous assessments have used the instantaneous RF, whereas we now use RF and the difference is around 20%. A reference to section 8.3.1 was already included, where a further description is given.
8-1130	8	45	26	45	27	again, not true, see Table 7.10a in AR4 [Ulrike Lohmann, Switzerland]	Taken into account - (text needs to be modified)
8-1131	8	45	31	45	47	Can you really assume that LLGHGs don't have an appreciable adjustment - what about Andrews and Forster, 2008 or Andrews et al. 2009 looking at the physiological forcing of co2? [Piers Forster, UK]	Taken into account - results from published litterature are used to update AF for CO2. A description of this is given in the chapter.
8-1132	8	45	33	45	35	The sentence does not make sense [Ulrike Lohmann, Switzerland]	Taken into account - sentence rewritten as 'The allowance of rapid adjustment for the aerosol cloud interaction results in important differences in the forcing as evident in the figure. '
8-1133	8	45	36	45	36	I would explain the meaning of «PDF» already on this line, instead of on line 40. [Dirk Olivié, Norway]	Accepted - text revised as suggested
8-1134	8	45	36			larger rather than stronger [Ruth Doherty, UK]	Rejected, we prefer to use weaker and stronger about negative numbers so no confusion about lower means less or more negative
8-1135	8	45	41			Define PDF (First use in chapter). Although it was defined and used in Figure 8.25 [Pieter Aucamp, South Africa]	Taken into account - see comment 8-1133

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8-1136	8	45	43	45	43	It would be worth refereing to the paper by Haywood and Schulz (2007) who looked at the change in the radiative forcing between IPCC 2001 and IPCC 2007. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland]	Taken into account - reference included
8-1137	8	45	43	45	43	It would be worth refereing to the paper by Haywood and Schulz (2007) who looked at the change in the radiative forcing between IPCC 2001 and IPCC 2007. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland]	Taken into account - reference included
8-1138	8	45	50	46	1	Comment on text: The contribution of solar irradiance has rather been re-evaluated to be stronger. As far as the TSI only is concerned, there was a growing consensus for a low impact until the recent papers by [Shapiro et al., 2011, Schrijver et al., 2011]. The spread of values has increased since. The same is true for the SSI, if the results from the SORCE satellite are confirmed. [Thierry Dudok de Wit, France]	Rejected - the solar RF is assessed to be weaker since RF (at the tropopause) is used in the calculation and not instantaneous RF at TOA as previously as well as the downward trend in TSI over the last few solar cycles.
8-1139	8	45	50	46	3	Also include in the table and estimate for total aerosol effect (direct + indirect) [Larry Horowitz, USA]	Rejected - the table is given for the various forcing mechanisms
8-1140	8	45	50			Table 8.9 There should be somesort of note for the solar irradiance numbers for the AR4 and AR5 being radiative forcings with different definitions - 8.3.1.2 [Gareth S Jones, UK]	Taken into account - this is given in the text.
8-1141	8	45				Figure 8.6: comment regarding AR5's near complete omission of the massive evidence for a solar-magnetic climate driver My training is in economics where we are very familiar with what statisticians call "the omitted variable problem" (or when it is intentional, "omitted variable fraud"). Whenever an explanatory variable is omitted from a statistical analysis, its explanatory power gets misattributed to any correlated variables that are included. This problem is manifest at the very highest level of AR5, and is built into each step of its analysis. For the 1750-2010 period examined, two variables correlate strongly the observed warming (and hence with each other). Solar magnetic activity and atmospheric CO2 were both trending upwards over the period, and both stepped up to much higher levels over the second half of the 20th century. This pair of correlations with temperature change give rise to the two main competing theories of 20th century warming. Was it driven by rapidly increasing human release of CO2, or by the 80 year "grand maximum" of solar activity that began in the early 1920's. ("Grand minima and maxima of solar activity: new observational constraints," Usoskin et al. 2007.) The empirical evidence in favor of the solar explanation is overwhelming. Dozens of peer-reviewed studies have found a very high degree of correlation (.5 to .8) between solar-magnetic activity and global temperature going back many thousands of years (Bond 2001, Neff 2001, Shaviv 2003, Usoskin 2005, and many others listed below). In other words, solar activity "explains," in the statistical sense, 50 to 80% of past temperature change. Such a high degree of correlation over such long time periods implies causality, which can only go one way. Global temperature cannot be driving solar activity, so there must be some mechanism by which solar activity is driving or modulating global temperature change. The high degree of correlation also suggests that solar activity is the PRIMARY driver of global temperature on every time scale studied (which is pretty much every time scale but the Milankovitch cycle). In contrast, CO2 and temperature records reveal no discernable warming effect of CO2. There is a correlation between CO2 and temperature, but with CO2 changes following temperature changes by an average of about 800 years (Caillon 2003), indicating that it is temperature change that is driving CO2 change (as it should, since warming oceans are able to hold less CO2). This does not rule out the possibility that CO2 also drives temperature, and in theory a doubling of CO2 should cause about a 1 degree increase in temperature before any feedback effects are accounted, but feedbacks could be negative, so there no reason, just from what we know about the greenhouse mechanism, that CO2 has to be a significant player. The one thing we can say is that whatever the warming effect of CO2, it is not detectable in the raw CO2 vs. temperature data.	Rejected, as described in section 8.3.1 there is no consensus in the litterature for a strong signal from solar changes on the radiative budget.

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						This is in glaring contrast to solar activity, which lights up like a neon sign in the raw data. Literally dozens of studies finding .5 to .8 degrees of correlation with temperature. So how is it that the IPCC's current generation of general circulation models start with the ASSUMPTION that CO2 has done 40 times as much to warm the planet as solar activity since 1750? This is the ratio of AR5's radiative forcing estimates for variation in CO2 and variation in total solar effects listed in table 8.9 on page 8-45. RF for CO2 is entered as 2.79 W/m^2 while RF for total solar effects is entered as .07 W/m^2. The 50% driver of global temperature according to mountains of temperature correlation data is ASSUMED to have 1/40th the warming effect of something whose warming effect is not even discernable in the temperature record. And this is on the INPUT side of the GCM's. The models aren't using gigaflops of computing power to FIND that CO2 has that much larger a warming effect. The warming ratio is fixed at the outset. Garbage in, garbage out. The "how" is very simple. The 40 times greater warming effect of CO2 is achieved by blatant omitted variable fraud. As I will fully document, all of the evidence for a strong solar magnetic driver of climate is simply left out of AR5. Of the many careful empirical studies that show a high correlation between solar activity and climate, not a single one is even mentioned ANYWHERE in the First Order Draft. On page 7-50, line 52, there is a single reference to a single paper (Kirkby 2007) where the text suggests some correlation between solar activity and climate, but it fails to mention even that the correlation to temperature is positive, never mind its dramatic magnitude, or the numerous repeated findings of this result. And that's it. One oblique reference in the entire report. A person reading AR5 from cover to cover would come away with not even a hint that for more than ten years a veritable flood of studies have been finding solar activity to explain something on the order of half of all past temperature variation. It is COMPLETELY omitted. [Sorry for using ALL CAPS for emphasis but Excel is not letting me use italics.] As a result, AR5 misattributes virtually all of the explanatory power of solar-magnetic activity to the correlated CO2 variable. This misattribution can be found both in AR5's analytical discussions and in its statistical estimations and projections, and the error could not be more consequential. If it is solar-magnetic activity that drives climate then the sun's recent descent into a state of profound quiescence portends imminent global cooling, possibly rapid and severe, and unlike warming, cooling is actually dangerous, and really can feed back on itself in runaway fashion. Nothing could be more perverse in such a circumstance than to unplug the modern world in a misbegotten jihad against CO2. The IPCC's omitted variable fraud must stop. AR5's misattribution of 20th century warming to CO2 must stop. The EVIDENCE overwhelmingly supports the solar-magnetic warming theory. The only support for the CO2 theory is the fact that models built on it can achieve a reasonable fit to the last couple centuries of temperature history, but that is only because CO2 is roughly correlated with solar activity over this period, while these models themselves are invalidated by their demonstrable omitted variable fraud. If warming is attributed to solar-magnetic effects at all in accordance with the evidence then the warming that is left to attribute to CO2 becomes utterly benign. With natural temperature variation almost certainly both substantially larger than CO2 effects, and headed in the cooling direction, the expected external value of CO2 is unambiguously positive. If anything, we should subsidizing and promoting increases in atmospheric CO2, exactly the opposite of the Executive Summary's opening claim that developments since AR4 "further strengthen the basis for human activities being the primary driver in the concerns about climate change." (Page 1-2, lines 4-5.) As someone who recognizes the scientific errors in this disastrous report, I can at least make sure that the issue is put properly before the authors of AR5. Thus I am documenting as concisely as possible the solar-magnetic omission and the errors it leads to. The discussion is substantial but I have kept it well under the character limit for a single comment. This comment is being submitted as a top-level comment on AR5 as a whole, and it is being submitted unaltered as a comment on three different sub-chapter headings where the omitted solar-magnetic evidence ought to be taken into account (on FAQ 5.2 starting on page 5-43, on section 7.4.7 starting on page 7-50, and on table 8.6 starting on page 8-45).	

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						A sample of the omitted evidence Listed below are a few of the most prominent and compelling studies that have found a high correlation between solar activity and climate, together with a semi-random collection of similar findings, totaling two dozen citations all together. It would be easy to list two dozen more, but the purpose here is just to show a sample of the omitted evidence, to document up-front the existence and validity of it. Included are brief descriptions of the findings for about ten of the studies. None of the observed correlations are reported anywhere in AR5. The first four are the ones I mentioned above: Bond et al. 2001, "Persistent Solar Influence on North Atlantic Climate During the Holocene," Science. Excerpt from Bond: "Over the last 12,000 years virtually every centennial time scale increase in drift ice documented in our North Atlantic records was tied to a distinct interval of variable and, overall, reduced solar output." Neff et al. 2001, "Strong coherence between solar variability and the monsoon in Oman between 9 and 6 kyr ago," Nature. Finding from Neff: Correlation coefficients of .55 and .60. Usoskin et. al. 2005, "Solar Activity Over the Last 1150 years: does it Correlate with Climate?" Proc. 13th Cool Stars Workshop. Excerpt from Usoskin: "The long term trends in solar data and in northern hemisphere temperatures have a correlation coefficient of about 0.7 — .8 at a 94% — 98% confidence level." Shaviv and Veizer, 2003, "Celestial driver of Phanerozoic climate?" GSA Today. Excerpt from Shaviv: "We find that at least 66% of the variance in the paleotemperature trend could be attributed to CRF [Cosmic Ray Flux] variations likely due to solar system passages through the spiral arms of the galaxy." [Not strictly due to solar activity, but implicating the GCR, or CRF, that solar activity modulates.] Plenty of anti-CO2 alarmists know about this stuff. Mike Lockwood and Claus Fröhlich, for instance, in their 2007 paper: "Recent oppositely directed trends in solar climate forcings and the global mean surface air temperature" (Proc. R. Soc. A), began by documenting how "[a] number of studies have indicated that solar variations had an effect on preindustrial climate throughout the Holocene." In support, they cited 17 papers: the Bond and Neff articles from above, plus Davis & Shafer 1992; Jirikowic et al. 1993; Davis 1994; vanGeel et al. 1998; Yu&Ito 1999; Hu et al. 2003; Sarnthein et al. 2003; Christla et al. 2004; Prasad et al. 2004; Wei & Wang 2004; Maasch et al. 2005; Mayewski et al. 2005; Wang et al. 2005a; Bard & Frank 2006; and Polissar et al. 2006. The correlations in a lot of these papers are not directly to temperature. They are to temperature proxies, some of which have a complex relationship with temperature, like Neff 2001, which found a correlation between solar activity and rainfall. Even so, the correlations tend to be strong, as if the whole gyre is somehow moving in broad synchrony with solar activity. Some studies do examine correlations between solar activity proxies and direct temperature proxies, like the ratio of Oxygen18 to Oxygen16 in geologic samples. One such study was highlighted in Kirkby 2007. Mangini et. al. 2005, "Reconstruction of temperature in the Central Alps during the past 2000 yr from a δ18O stalagmite record," found: Excerpt from Mangini: "... a high correlation between δ18O in SPA 12 and D14C (r =0.61). The maxima of δ18O coincide with solar minima (Dalton, Maunder, Sporer, Wolf, as well as with minima at around AD 700,	

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						500 and 300). This correlation indicates that the variability of δ18O is driven by solar changes, in agreement with previous results on Holocene stalagmites from Oman, and from Central Germany." And that's just old stuff. Want some new stuff? Here are four random recent papers. Ogurtsov et al, 2010, "Variations in tree ring stable isotope records from northern Finland and their possible connection to solar activity," JASTP. Excerpt from Ogurtsov: "Statistical analysis of the carbon and oxygen stable isotope records reveals variations in the periods around 100, 11 and 3 years. A century scale connection between the 13C/12C record and solar activity is most evident." Di Rita, 2011, "A possible solar pacemaker for Holocene fluctuations of a salt-marsh in southern Italy," Quaternary International. Excerpt from Di Rita: "The chronological correspondence between the ages of saltmarsh vegetation reductions and the minimum concentration values of 10Be in the GISP2 ice core supports the hypothesis that important fluctuations in the extent of the salt-marsh in the coastal Tavoliere plain are related to variations of solar activity." Raspopov et al, 2011, "Variations in climate parameters at time intervals from hundreds to tens of millions of years in the past and its relation to solar activity," JASTP. Excerpt from Raspopov: "Our analysis of 200-year climatic oscillations in modern times and also data of other researchers referred to above suggest that these climatic oscillations can be attributed to solar forcing. The results obtained in our study for climatic variations millions of years ago indicate, in our opinion, that the 200-year solar cycle exerted a strong influence on climate parameters at those time intervals as well." Tan et al, 2011, "Climate patterns in north central China during the last 1800 yr and their possible driving force," Clim. Past. Excerpt from Tan: "Solar activity may be the dominant force that drove the same-phase variations of the temperature and precipitation in north central China." Saltmarshes, precipitation, "oscillations." It's all so science-fair. How about something just plain scary? Solheim et al. 2011, "Temperature prognosis based on long sunspot cycle 23," (not sure if this has been published yet, but you can find it here: http://www.au.agwscam.com/pdf/SolheimSolarTemperature.pdf). Excerpt from Solheim: "We find that for the Norwegian local stations investigated that 30-90% of the temperature increase in this period may be attributed to the Sun. For the average of 60 European stations we find ≈ 60% and globally (HadCRUT3) ≈ 50%. The same relations predict a temperature decrease of ≈ 0.9°C globally and 1.1−1.7°C for the Norwegian stations investigated from solar cycle 23 to 24." First Chapter 5 error: omitting all solar variables besides TSI Chapter 5, the paleo observations chapter, is the right place for the evidence for a solar-magnetic climate driver to be introduced because most of this evidence is obtained from the deposition of cosmogenic isotopes in various paleologic strata: ice cores, geologic cores and tree rings. When solar activity is strong, less galactic cosmic radiation (GCR) is able to penetrate the solar wind and reach earth, so variation in cosmogenic isotopes found in time-dated strata serves as a proxy for solar activity. But when chapter 5 does get around to looking at cosmogenic records, it only looks at how they can be used to reconstruct total solar irradiance (TSI). It never even hints at the flood of studies that show a high degree of correlation between solar activity and	

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						various paleo proxies for climate and temperature! This occurs under the subheading "FAQ 5.2: Is the Sun a Major Driver of Climate Changes?" which is placed as an addendum to Chapter 5, starting on page 5-43. This FAQ mentions the long-period chang in TSI that come with orbital variation (Milankovitch cycles), a factor which hasn't changed enough since 1750 to account for any significant amount of the warming since that date. Neither can TSI be responsible for significant recent warming because, as solar activity jumps dramatically up and down over the roughly 11 year solar cycle, TSI is known to remain remarkably stable, varying only .1 to .2% (as noted on page 5-43, line 53). Thus, concludes FAQ 5.2, solar variation cannot be responsible for any significant amount of the warming since 1750. But it is only able to reach this conclusion by completely omitting any consideration those solar variables other than TSI that could be affecting global temperature. Unlike TSI, solar wind speed and pressure vary considerably over the solar cycle and between solar cycles. So do the Ap index and the F10.7cm radio flux progression. The GCR that the solar wind modulates, the neutron counts measured at Climax and Oulu and other locations, can vary by a full order of magnitude over the solar cycle. In contrast, TSI varies so little that it is called "the solar constant." If there is a mechanism by which solar variation is driving global temperature, it is most likely to work through those solar variables that actually vary significantly with solar activity. Yet the discussion in FAQ 5.2 pretends that these other solar variables do not even exist. So that's the first error in FAQ 5.2: pretending to have addressed the range of possible solar effects while studiously neglecting to mention that there are a bunch of solar variables that, unlike TSI, vary tremendously over the solar cycle and might affect our climate in ways that we do not yet understand. We in-effect live inside of the sun's "atmosphere," the extended corona created by the sun's magnetic field and the solar wind. AR5 simply assumes that this solar environment has no effect on global climate, and they do it by rank omission of the relevant variables. The omitted variable problems that result are not an accident. They are omitted variable fraud. Second Chapter 5 error: the highly irrational assumption that temperature would be driven by the trend in solar activity rather than the level Perhaps in an effort to justify ignoring all solar variables other than TSI, FAQ 5.2 ends with what it presents as a general reason to dismiss the possibility that solar variation made any significant contribution to late 20th century warming by ANY mechanism. Page 5-44, lines 25-28: "[The sun can't be] a major driver of the climate changes over the past 40 years because instrumental TSI and SSI records contain no significant trend; whereas records of global mean temperature and GHG concentrations contain significant trends of increasing values. This lack of agreement in trends demonstrates that the Sun did not play a role during this period." TSI peaks at the high point of the solar cycle, just as the other solar variables do, so no matter what solar variable you look at, it can't have been the cause of recent warming, because these variables showed no upward trend over this period, right? Wrong. That's like saying you can't heat a pot of water by turning the flame to maximum and leaving it there, that you have to turn up the flame slooooowly if you want the water to heat. It is incredible to see something so completely unscientific in AR5, passing as highly vetted science. And the "flame" DID stay on maximum. Again, there was an 80 year "grand maximum" of solar activity starting in the early 1920's (Usoskin 2007). AR5 is in-effect assuming that the oceans had already equilibrated to whatever temperature forcing effect this high level of solar activity might have. Otherwise the continued temperature forcing from the continued high level of solar activity would have caused continued warming. Claims of rapid ocean equilibration have been made (Schwartz 2007), but they don't stand up to scrutiny. In order to get his result, Schwartz used an energy balance model with the oceans represented by a single heat sink. That is, he assumed that the whole ocean changed temperature at once! Once you move to a 2 heat sink	

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						model where it takes time for heat to transfer from one ocean layer to another (Kirk-Davidoff 2009), it becomes clear that the rapid temperature adjustment of the ocean surface tells us next to nothing about how long it takes for the ocean to equilibrate to a long term forcing. The paleo-temperature record is typified by multi-century warming and cooling phases, suggesting that equilibration can easily take centuries, making it ludicrous to assume that the warming effect of a grand maximum that began in the 1920's must have been spent by 1970 or 1980 or by ANY particular date. So no, there is no way to save the utterly incompetent argument in FAQ 5.2 that a solar driver of temperature can only cause warming when it is on the increase. If solar wind pressure or GCR does in some way drive global temperature, there is every reason to believe that it would have continued to warm the planet for as long as solar activity remained at grand maximum levels. There is NO EXCUSE for the IPCC to be omitting these variables, which are much more likely than TSI to be responsible for the high observed degree of correlation between solar activity and climate. For chapter 5 to be tenable, all of the now massive evidence that there is SOME mechanism by which solar activity is driving MOST temperature change must be laid out in full. Technical note: misattribution is assigned manually in AR5, but the concept is the same as for purely statistical omitted variable fraud If TSI and the other solar variables all move roughly together, won't omitting the solar variables other than TSI cause any explanatory power they might have to be attributed to TSI rather than CO2, since they are more closely correlated with TSI? In a purely statistical estimation scheme yes, but the IPCC uses a combination of parameterized elements and estimated elements, and one of the elements that is parameterized is radiative forcings of CO2 and TSI, meaning that their relative warming effects are parameterized as well, with CO2 being assigned 40 times the warming effect of TSI over the 1750 to 2010 period. This parameterization means that the explanatory power of the omitted solar magnetic variables gets attributed forty parts to CO2 for every one part to TSI. This structure forces the misattribution onto CO2. You can think of it a manual assignment of the misattribution. The general concept of the omitted variable remains the same. There is only so much attribution for warming to go around (100%). If attribution is given to the solar-magnetic variables in accordance with the evidence from the historic and paleo records—at least 50%—then there less than 50% that can possibly be attributable to other causes. Which again beings the scientific competence of IPCC into question. If CO2 has 40 times the warming effect of the 50% driver of global temperature (total solar effects), that makes it what? The 2000% driver of global temperature? Chapter 7 inverts the scientific method, using theory to dismiss evidence Where chapter 5 simply pretends that no solar variable other than TSI exists, Chapter 7 doesn't have that option. It is tasked to address directly the possibility that variables like the solar wind and GCR could be affecting climate. But Chapter 7 still comes up with a way to avoid mentioning any of the massive evidence that there must be SOME mechanism by which solar activity is driving climate. Just as it starts to touch on the subject, it jumps instead to examining the tenability of PARTICULAR THEORIES about the mechanism by which solar activity might drive climate. This happens right at the beginning of section 7.4.7.1. "Correlations Between Cosmic Rays and Properties of	

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						Aerosols and Clouds." This is on page 7-50, lines 50-53: "Many empirical relationships or correlations have been reported between GCR or cosmogenic isotope archives and some aspects of the climate system, such as SSTs in the Pacific Ocean (Meehl et al., 2009), some reconstruction of past climate (Kirkby, 2007) or tree rings (Dengel et al., 2009). We focus here on observed relationships between GCR and aerosol- and cloud-properties." The first sentence of 7.4.7.1 is as close as AR5 comes to making any mention of overwhelming evidence that there is SOME mechanism by which solar activity drives global temperature. The Kirkby citation suggest some correlation between solar activity and climate, but what the correlation might be is completely obscured, and that's it. The second sentence effects the transition into looking at the evidence for particular theories of the mechanism involved. A short discussion later, the evidence for these particular mechanisms is asserted (quite tendentiously) to be "too weak" for the mechanisms to be "climatically-significant" (page 7-52, lines 33-35). This proclaimed weakness in turn becomes the rationale for omitting the mechanisms from the IPCC's general circulation models, and hence from the projections that are made with those models. What do the AR5 draft authors do with the overwhelming evidence that there is SOME mechanism at work that makes solar magnetic the primary driver of global temperature? So they don't like the particular theories offered. They have to still acknowledge that SOME such mechanism must be at work, don't they? Ahh, but readers don't know about that evidence, because it was skipped over with that single oblique reference to Kirkby 2007, and AR5 continues as if the evidence doesn't exist. They never use it. They never mention it. They never think about it. It is GONE. They declare their dissatisfaction with the available theories for how such a mechanism would work, and use this as an excuse to completely ignore the massive evidence that there is some such mechanism at work. This is an exact inversion of the scientific method, which says that evidence always trumps theory. The IPCC is throwing away the evidence for a solar-magnetic driver of climate because it isn't satisfied with the theories that have been proposed to account for it. This is the DEFINITION of anti-science: putting theory (or ideology, or ANYTHING) over evidence. Evidence has to be the trump card, or its not science. The IPCC is engaged in actual, definitional, anti-science, exactly inverting the scientific method. It is as if a pre-Newtonian "scientist" were to predict that a rock released into the air will waft away on the breeze, because we understand the force that the breeze imparts on the rock, but we have no good theory of the mechanism by which heavy objects are pulled to the ground. We should therefore ignore the overwhelming evidence that there is SOME mechanism that pulls heavy objects to the ground, and until such time as we can identify the mechanism, proceed as if no such mechanism existed. This is what the IPCC is actually doing with the solar-climate evidence. Y'all aren't scientists. You are pure, definitional, ANTI-SCIENTISTS. More anti-science: Chapter 7 repeats the second Chapter 5 error You know, that bit about thinking that a climate driver can only cause continued warming if its own level continues to increase? Chapter 7 says it again: just leaving a proposed climate driver on maximum can't possibly cause warming. From page 7-52, lines 35-37: Moreover it should be noted that one study infers no trend in cosmic ray intensity over the last 50 years (McCracken and Beer 2007). And that's the end of the section, AR5's punctuation mark on why solar activity and GCR should be dismissed as an explanation for late 20th century warming. This is anti-scientific in its own way. Scientists are supposed to be smart. They aren't supposed to think that you have to slowly turn up the flame under a pot of water in order to heat it. You could collect every imbecile in the world together and not a one of them would ever come up with the idea that they have to turn the heat up slowly. It's beyond stupid. It's like, insanely stupid. And	

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						multiple chapter-writing teams are proclaiming the same nonsense. Fruitcakes. Okay, I guess that means I'm ready to wrap up. Y'all have taken all these tens of billions of dollars of research money and used it perpetrate a fraud. As I have documented above, you have perpetrated the grandest and most blatant example of omitted variable fraud in history, but so far only the skeptic half the world knows it. You still have a shot, before global cooling is an established fact, to make a rapid turn around and save some shred of your reputations. But if AR5 comes out insisting that CO2 is a dominant warming influence just as global cooling is becoming an established fact, then you all are finished on the spot. You'll still have your filthy lucre, but the tap is going to turn off, and your reputations will be destroyed forever. Can you imagine a worse juxtoposition? And this is what the evidence says is going to happen, ALL of that evidence that you have been so studiously omitting. I'm eager for your embarrassment, but I would much rather see you save yourselves, so that the needed policy reversals can some that much sooner. The anti-CO2 policies that your fraudulent "science" has supported are right now destroying the world economy. You idiots are KILLING our future. Please wake up and try to save your own reputations before your lunatic anti-science ruins us all. End comment [Alec Rawls, United States]	
8-1142	8	46	0	46	0	3rd column for total aerosol indirect effect should be -1.2 W/m2 with a range from -0.2 to -2.3 W/m2 (from page 502, chapter 7, AR4) [Ulrike Lohmann, Switzerland]	Rejected, only RF values from AR4 were included.
8-1143	8	46	1	46	3	The convention for reporting uncertainties is not adhered to for AR5 RF. Some bracketed values show +/- uncertainties, while others show range [Larry Horowitz, USA]	Taken into account - table caption is modified with addition that uncertainties can either be given as +- or a range
8-1144	8	46	6	46	6	This section does not actually consider the impacts of climate change from emissions but rather the evaluation of emission metrics and their application to different activities. Suggest changing the title of this section to “Evaluation and Application of Emission Metrics” [Haroon Kheshgi, United States of America]	Taken into account. Structure and titles have been changed.
8-1145	8	46	6	56	7	These were nice sections on gwp,GTPs etc. but I found they repeated some of the earlier metric and chemistry sections. Some rationalising would be useful [Piers Forster, UK]	Taken into account. The structure of the chapter has now changed and repetitions are avoided
8-1146	8	46	6	56	7	More synthesis is needed for sections 8.5.2 and 8.5.3. As written now, it is hard to figure out what the major take-away points are. [Larry Horowitz, USA]	Taken into account. Sections have been rewritten, with more synthesis (and assessment).
8-1147	8	46	8	56	8	We are back at the problem of relating atmospheric concentratyions to emissions, something which cannot be done without your absurd models based on climate-excluing "greenhouse" assumptions (see my comment on pages 8.2.2 to 8.2.3. The models all depend on the naive belief that all these influences are uniformly distributed in the atmosphere when this is obviously untrue. [VINCENT GRAY, NEW ZEALAND]	Rejected. No scientific evidence to support changes suggested by the reviewer. The models are not based on the assumption that influeneces are uniformly distributed in the atmosphere
8-1148	8	46	18	46	24	It is important that the GWP values included in this assessment provide an internally consistent set of data. Many stakeholders use the GWP values published by IPCC to compare the potential climate impact due to the emission of different compounds. A valid comparison, however, requires an evaluation of GWPs that have been calculated on the same basis. This in turn requires that the atmospheric lifetime for these compounds be calculated by a consistent method. While the GWP values published in the IPCC Fourth Assessment Report have been determined using the same calculation method, the atmospheric lifetimes used in those calculations are not consistent. For compounds that are removed from the atmosphere primarily by reaction with OH, the atmospheric lifetimes for nearly all of the compounds cited in the Fourth Assessment Report were calculated by scaling the rate constant for that reaction relative to that for a well-defined reference compound. However, the atmospheric lifetimes for a few of the compounds were calculated directly using the globally averaged hydroxyl radical concentration. These two calculation methods result in estimated lifetimes which differ by a factor of 2. Examples of compounds for which the latter calculation [John Owens, United States of America]	Taken into account in the discussion of uncertainties and consistency although we could not discuss these issues at the level of details asked for here.
8-1149	8	46	18	46	24	method was used include HFE-43-10pccc124, HFE-236ca12, HFE-338pcc13 and possibly HFE-356pcc3 (although it is unclear for this last compound since the lifetime appears to be in error). Some of these errors	Taken into account in the discussion of uncertainties and consistency. The lifetimes, radiative efficiencies,

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						appear to have been corrected in WMO, 2010. Because of the importance placed upon the GWP values published in the IPCC Assessments it is vital that the lifetime and GWP data included in this chapter and the anticipated appendix provide a consistent set of data and attempt to correct the errors reported in the cited literature. [John Owens, United States of America]	and AGWPs are now taken from a recently submitted review paper.
8-1150	8	46	23	46	24	We have not included the ozone indirect effect in the ODS GWPs before in the main GWP table. Instead, we had the direct ODS GWPs in the main table and a separate table of indirect and net GWPs. See, for example, tables 5A-1 and 5A-4 or the 2011 ozone assessment. I believe there is benefit in having the direct, indirect, and perhaps net values rather than only a single value that includes the ozone depletion effect. [John Daniel, USA]	Taken into account. Indirect GWPs of some selected ODS are given in a separate table.
8-1151	8	46				Table 8.9 estimate on contrails: Lee et al (2010) estimate the value at 11.8 mW, in the table it says 0.02. There seems to be a mismatch somewhere, unless an even later paper is cited (but there is no citations). [Christian Azar, SWEDEN]	Taken into account - a summary of the assessment in Chapter 7 is given in section 8.3.4 and is the basis for the value given in the table.
8-1152	8	47	2			Again, worth mentioning the TH are "illustrative values" [Glen Peters, Norway]	Accepted.
8-1153	8	47	5	47	8	"this contribution was found to be larger than the direct CH4 effect." Is this effect in the numbers in Table 8.10 because the GTP100 numbers for CH4 fossil vs. non-fossil are similar? [Robert Portmann, United States of America]	Noted. We have moved this to the Supplementary Material now. We think the following sentence is clear: "They found that CO2 oxidation had a larger effect on GTP values and this effect was larger than the direct CH4 effect for time horizons beyond 100 years."
8-1154	8	47	6	47	6	State more clearly what you mean by the effect on CO2 (i.e., that CH4 oxidizes to CO2). [John Daniel, USA]	Accepted. Will be more clear.
8-1155	8	47	8	47	8	Add reference for CO2 effect being larger than direct CH4 effect in GTP. [John Daniel, USA]	Accepted. we have added reference: "Boucher et al. (2009) included the effect of CO2 from oxidation of methane from fossil sources and calculate a GWP100 higher than given in AR4 (27–28 versus 25). They found that CO2 oxidation had a larger effect on GTP values and this effect was larger than the direct CH4 effect for time horizons beyond 100 years"
8-1156	8	47	9			Perhaps worth indicating more clearly if there is a preference between putting the CO2 from CH4 in the CH4 inventory (metric) or in the CO2 metric. Likewise with CO, etc. Currently, I think most emission inventories would have all C that ends up as CO2 in the CO2 category. Thus, care needs to be taken including it in CH4 to avoid double counting. [Glen Peters, Norway]	Taken into account. This will be briefly discussed, with reference to e.g. Boucher et al., 2009, ERL.
8-1157	8	47	17	47	35	Can you consider providing uncertainty ranges for GWPs? You do it for the short-lived species, why not for well-mixed gases? Providing GWPs with 5 significant figures might be too much. [Olivier Boucher, France]	Taken into account. We have put more emphasis on uncertainties using ranges from the literature (Reisinger et al.; Boucher et al. 2009; Boucher, 2012; Olivie and Peters, 2012 etc). But we have not given uncertainty ranges for all listed GWP values. Number of significant digits will be reduced in next draft.
8-1158	8	47	17			Table 8.10 is very useful the way it distinguishes between lifetime and radiative efficiency with regards to the different GHGs. But it should include as many species as possible. Consider to make a table that is more in line with was is already reported for ozone depleting substances in the WMO Ozone Assessments. Such a table would be valuable for the work under UNFCCC. It is also good that both GWP and GTP are included with several different timespans. [Øyvind Christophersen, Norway]	Taken into account. The table has been expaned to include a long list of gases.
8-1159	8	47	17			I suggest that GTP NOT be given here (in this table and indeed in this chapter) as it is not a forcing. It assumes some astmospheric response. It is dealt with elsewhere in the document. Also Figure 8-26. [Stephen E Schwartz, USA]	Rejected. The inclusion of GTP is given by the scoping.
8-1160	8	47	17			I suggest instead of or in addition to GWP also present AGWP. GWP is ratio to CO2 which has uncertainty in forcing and time profile, which will certainly change in the future as they have in the past, necessitating change	Accepted. We will present bot AGWPs and GWPs. IRFs for fossil and biogenic CO2 has been briefly

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						in the GWP of a given substance for reasons that have to do with change in understanding of CO2, not the substance. The document even says the impulse profile used is provisional and will be updated. Then there is the cumbersome if not preposterous situation of a different impulse profile for biogenic and fossil CO2. CO2 is CO2 whatever the source; it has the same chemistry and physics. [Stephen E Schwartz, USA]	discussed.
8-1161	8	47	18			Define "radiative efficiency", it has not been defined since FAR as far as I could tell. [Glen Peters, Norway]	Taken into account. This is defined in the Supplementary Material.
8-1162	8	47	18			The definition of the radiative efficiency is also a little ambigous. It is worded as an marginal change and thus would be calculated as 5.35/Co (derivate of 5.35ln(C/Co)), also see Caldiera and Kasting 1993, Nature. It is worth being explicit on what it is meant to be [Glen Peters, Norway]	Taken into account. This is defined in the Supplementary Material.
8-1163	8	47	18			To use the radiative efficiency in a metric, it needs to be converted from a ppb to kg measure. It is worth mentioning this, and even the formula for doing this and parameters used (mass of atmosphere, molecular weights, etc) [Glen Peters, Norway]	Taken into account. This is given in the Supplementary Material.
8-1164	8	47	18			Why 20 and 100 years for GWP and 20, 50, 100 for GTP. The FAR uses the words "illustrative examples" and I think it would be useful to put that in the table caption and text. That is, state clearly something like "we use 20, 50, and 100 for consistency with previous assessment reports and do not place any particularly preference on any time horizon". However, in the case of GTP(50), I have seen justifications based on when 2degrees would be reached, and thus it is worth mentioning [Glen Peters, Norway]	Accepted. We write "Updated GWP and GTP values for WMGHG for some illustrative and tentative time horizons are given in 14 Table 8.A.1." But we have not discussed of time horizon can be chosen.
8-1165	8	47	19	47	20	“Boucher and Reddy (2007)” should be “Boucher and reddy (2008)”. [Dirk Olivié, Norway]	Taken into account.
8-1166	8	47	19			I think it is worthwhile to put in the equation for the temperature IRF, with the timescales and constants to save looking up in references. It is good to have all the input data shown. [Glen Peters, Norway]	Accepted. Will be given in supplementary material
8-1167	8	47	20	47	20	This is not the normal definition of "climate sensitivity", which is normally temperature change for a doubling of CO2 (9-64 L10). [Gareth S Jones, UK]	Noted. This has now been removed.
8-1168	8	47	23			I would put the equation and parameters of the CO2 IRF in here as well [Glen Peters, Norway]	Accepted. Will be given in supplementary material
8-1169	8	47	32			Does this mean the CO2 is not included in the CH4. The double counting issue should be emphasised. [Glen Peters, Norway]	Accepted. Has been clarified.
8-1170	8	47	38	47	43	Briefly define impulse response function before using it. [Robert Portmann, United States of America]	Accepted. Will be given in supplementary material
8-1171	8	47	38		44	The preposterous notion of an impulse function being dependent on the CO2 being from fossil or biogenic source should also put the stake through the heart of GWP's based on CO2 impulse profile. The whole concept of multiple impulse profiles for a given substance, including a negative impulse profile, should be exorcized. An impulse profile is fraction of emitted substance that remains in atm as fn of time. Try to explain to any rational individual how that fraction can be negative. [Stephen E Schwartz, USA]	Taken into account by adding more discussion of results in the literature on this issue.
8-1172	8	47	43			I suggest to delete from "which …" and include "…since C is taken up quickly in the surface ocean and outgassing is required later to ensure all C is sequestered as the biomass grows. Since the IRF becomes negative, then the RF will also become negative, and this will likely lead to a negative temperature. Thus, for some time horizons, it is expected the the GTPbio would be negative. The GWPbio also uses an experimental set up where the biomass is oxidised and then grows, but the GWPbio values will be different if the biomass grows before oxidation". [Glen Peters, Norway]	Taken into account. The text has been revised.
8-1173	8	47		47		Table 8.10: I expect that this table and it’s new evaluation of GWPs will be the most read item in this chapter and perhaps in the entire WG1 report. Unfortunately, the table is listed as preliminary limiting expert review. Suggest that the table 1) be clear about what mechanisms are included in the revised estimates in GWP (e.g. are effects on aerosols by methane included?), 2) give sound estimates of uncertainty particularly for LLGHGs where table 8.8 leads one to expect this is possible given the very high level of confidence listed in , and 3) break down the revision into components of the revision so that users of the information can decide which GWP is most appropriate for their use and what is the cause of the revision. Explanation is needed as to why GWPs continue to change seemingly beyond the uncertainty ranges implied. For example, why has methane	Taken into account to theextent possible given space restrictions.

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						GWP increased in each assessment from 21 in the SAR, while Table 8.8 indicates no major change and very high confidence for LLGHG RFs? [Haroon Kheshgi, United States of America]	
8-1174	8	47		47		Table 8.10: Unlike all previous IPCC evaluations of GWP, this table does not consider the 500 year time horizon. Suggest that the table either include this time horizon or explain why it is not included. [Haroon Kheshgi, United States of America]	Rejected. We have explained why in the Supplementary Material.
8-1175	8	47				Table 8.10: Be clear on whether methane includes the sulfate effect: I'd prefer that it not be included in the table because it isn't clear what the policy implications of including sulfate effects in a GWP is (eg, it isn't included when calculating the GWP effects of a coal-plant shutdown), but the effect should be detailed in the footnote because it is an interesting and important finding. [Marcus Sarofim, USA]	taken into account. We have explained which effects that are included.
8-1176	8	47				Table 8-10. GWPs/GTPs for a time horizon >100 years should be added. For consistency with previous reports I suggest a 500 year horizon would be appropriate. [Timothy Wallington, USA]	Rejected. The uncertainties in RF and IRF are very large on this timescale. But in the Supplementary Material we try to show the long term behaviour in other ways than by using ratios of integrated RF.
8-1177	8	48	1			The whole discussion of 500 yr time scale and why it is omitted is a consequence of resting the GWP on CO2. The point is trivial in AGWP world. For a gas like PFC14 that has a long lifetime, the AGWP (H) is simply equal to (normalized forcing) * H. Trivial. Done. No ratioing to the highly uncertain CO2 profile at long times. I am so vehement on this issue only because I am reasonably persuaded that this suggestion will be rejected, as it has been in the past. So I am mainly putting it in here for the record. Someday you will abandon GWP in favor of AGWP and I can remind you that I told you to do so back in 2012 (and 2006, and 2000, and 1995). [Stephen E Schwartz, USA]	Taken into account now that we give both AGWPs and GWPs. And the variation of GWP with H and how this is controlled by AGWP_CO2 is discussed and illustrated in a new figure added.
8-1178	8	48	7			Are these then the GTP values (1/40,000 and 1/23,000)? If so, say so. [Glen Peters, Norway]	Rejected. We don't think there is a suffiecient basis for giving that detailed information about effects after 500 years.
8-1179	8	48	8			Could drop "obviously" as it is obvious! But, since the uncertainties are so large, what is the reason for extending the calculation so long? [Glen Peters, Norway]	Accepted. "Obviously" is dropped.
8-1180	8	48	12	48	18	This distinction seems a value judgment to me. One could equally argue that the GTP500 gives misleading information compared with GWP500 since it ignores that shorter-lived gas ever had an effect on the climate. [Robert Portmann, United States of America]	Rejected. We disagree with the comment. We're not showing GTP500 anyway, and if we were, it would be correct (not misleading) that shorter-lived gases have little effect on 500 year climate
8-1181	8	48	12	48	18	There is nothing ambiguous regarding the meaning of GWP500 values. As discussed in Chapter 8, GWP values are mathematically defined as the ratios of the radiative forcing integrated over time. GWPs provide an unambiguous measure of the relative contribution of different gases over a certain time horizon to radiative forcing of climate change. [Timothy Wallington, USA]	Noted. We agree that GWP500 not ambiguous in terms of integrated RF, but in terms of what this means for climate change; and thus not very useful. This paragraph has been rewritten and moved to the Supplementary Material.
8-1182	8	48	12			"ambiguity regarding the meaning of GWP500", which by implication implies no ambiguity of GWP20 and GWP100. If so, can you state the meaning of GWP20 and GWP100. Otherwise, I suggest to reword so that GWP20, GWP100, GWP500 are all stated to have no meaning (or ambiguous meanings). [Glen Peters, Norway]	Taken into account. We will add to the text that uncertainties in RF and IRF get very uncertain on that timescale.
8-1183	8	48	12			I think it is quite okay to justify not including GWP500 as we have poor knowledge on the carbon cycle IRF over that time, and the radiative efficiency will not be valid over that period. I dont think the "ambiguity regarding the meaning" is required [Glen Peters, Norway]	Taken into account. We will add to the text that uncertainties in RF and IRF get very uncertain on that timescale.
8-1184	8	48	15	48	18	I'd argue that a GWP500 is only msileading if the GWP concept isn't understood: because it is an integral over the full time, it accounts, in a fashion, for effect both early and late. In some ways, this may explain the longevity of the GWP100: while it does nothing perfectly, it does a bit of everything... [Marcus Sarofim, USA]	Noted. Our argument is that it generally isn't properly understood. E.g. typical literature comments are "methane (which is 25 times more powerful as a greenhouse gas than CO2) ...".
8-1185	8	48	20	8	28	Nitric acid has been neglected as a GHG. Experimental measurements of the surface radiative forcing	Rejected, a RF (at the tropopause) of nitric acid is not

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						indicated fluxes of .08 W/m2, qualifying it as a significant GHG. W.F.J. Evans and E. Puckrin, The Wintertime Surface Radiation Forcing Associated with Nitric Acid, Atmos. Environment, 35, 71-77 (2001). [Wayne Evans, USA]	supported by the peer-reviewed published literature. Gases with very short short lifetime may have a small impact on the longwave radiative flux at surface but be negligible for longwave RF (at tropopause).
8-1186	8	48	26			"substantial challenges" arising for short lived substances. Not in AGWP world, where the AGWP is simply a constant after the material is no longer in the atmosphere. [Stephen E Schwartz, USA]	Noted. We do not agree that AGWPs would be without problems; there are still some challenges even if AGWP is constant.
8-1187	8	48	35		38	"Due to"; I grant that the native language of some of the authors may not be english; but please try to remember that "due to" introduces an adjectival phrase, not adverbial. The difficulty is due to the reactivity - adjectival; correct; due to to the reactivity, the problem is difficult - adverbial; incorrect. [Stephen E Schwartz, USA]	Editorial. Will be corrected
8-1188	8	48	41	48	43	Describe "methane-induced ozone forcing" and "methane forcing" here (or point to where in chapter it is discussed). [Robert Portmann, United States of America]	Accepted. We write "CH4-controlled O3 response". This is also discussed earlier in the chapter.
8-1189	8	48	46	48	46	Typo in reference: "Frohlich" should read "Fröhlich". [Georg Feulner, Potsdam]	No reference to this is given here
8-1190	8	48	50	48	50	Typo in reference: "Frohlich" should read "Fröhlich". [Georg Feulner, Potsdam]	No reference to this is given here
8-1191	8	48		61		CO2 has a long atmospheric lifetime and the influence of CO2 on climate is a long-term phenomenon. Assessing impacts over times scales >100 years is every bit as important as assessing impacts over this century. Intergenerational justice is an important concept which is easy to lose sight of from an individual human perspective. The discussion is focused on 20 and 100 year time horizons. There should be equal attention to effects on >100 year time scales. [Timothy Wallington, USA]	Taken into account. We agree that long term is important, but we have shown long term perspectives in other ways. The long term persistence of the CO2 perturbation is also shown in chapter 6.
8-1192	8	48				Fig 8-26. Replace by figure showing forcing vs time, per kg. Make it explicit that the forcing shown for CO2 is per kg (CO2) or Kg (C). Better kg(C) as emissions are generally presented in PgC yr-1. [Stephen E Schwartz, USA]	Taken into account. The figure is moved to the Supplementary Material and unit has been clarified.
8-1193	8	49	1	49	1	“that that this” should be “that this”. [Dirk Olivié, Norway]	editorial
8-1194	8	49	10	49	11	Explain further. What are the key uncertainties? [Larry Horowitz, USA]	Taken into account to the extent possible given space restrictions.
8-1195	8	49	14	49	16	Even globally, Table 8.11 suggests the sign of Nox GWP is uncertain, as evidenced by the Fuglestvedt et al. (2010) and Shindell et al. (2009) estimates [Larry Horowitz, USA]	Noted. Uncertainties are stressed.
8-1196	8	49	21	50	22	Derwent et al (2008) (Radiative forcing from surface NOx emissions: spatial and seasonal variations, Climatic Change DOI 10.1007/s10584-007-9383-8) may also be a useful source. [David Stevenson, UK]	Rejected due to space limitations. But references to similar studies from this group are given in several places.
8-1197	8	49	22		23	Let me get this straight. You are saying that on a 20 year horizon, a kg of NOx (whatever that is, because we don't know the molecular weight of NOx so it depends on the mix of NO and NO2 or needs to be particularized), emitted in east asia, which lasts in the atmosphere for maybe a week because of oxidation to nitrate and removal in precip (ok some remains longer if it goes to pan) can exert anywhere from 45 to -38 times as much as a kg of CO2 that lasts maybe 100 years, so certainly in its youth at 20 years. If that is what you are saying then you are saying we have no idea of the effect. But in the tropics you are confident that the sign is positive and that the effect is anywhere from 43 to 130 times as much on a 20 year time scale but then the whole thing changes sign at 100 years. This is astonishing, if important. Lets try to see if its important. Typical NOx emissions are 1-3 g /kg fuel; CO2 emissions are (if you really are referring to CO2 and not C) 3 kg/kg fuel. So a factor of 1000. If the GWP is 100, then the effect is 10%, so I guess its important. I would like to see the AGWP as a function of time. What's driving this? Is it NOx influencing the removal of methane, so if you emit NOx you kill methane? The AGWP of CO2 on this time frame is just proportional to time; so need to see what the AGWP of NOx is on this time frame. All that said, what is missing in the report is the sort of context I have just tried to provide. Some discussion of importance; some discussion of why these gwp's	Taken ito account. Several important points are brought up in this comment. We will try to include a more comprehensive discussion of these issues, to the extent possible given the space limitations. Figure 6 from the paper by Fuglestvedt et al., 2010 gives much of the explanations asked for here but due to space limtations we cannot include this. We dont agree that AGWP will show the various effects better than GTP or AGTP. Fig 8.28 (new fig 8.31) showing Emissions x Metric gives insight to importance of gases, and puts NOx into context. GTP will not be left out; this is given by scoping.

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						change sign; some discussion of why so much uncertainty. And skip the GTPs. And present the AGWPs. [Stephen E Schwartz, USA]	
8-1198	8	49	22		23	This whole discussion raises lots of questions but doesnt resolve any of them. But its astonishing. Finally, you put error bars on the numbers but are these based on uncertainties in parameters like rate constants that can be propagated, or are they more structural, depending on assumptions and the like. If the latter, how can you (or anyone) place confidence bounds on them and tell the world that these are 95% confidence. [Stephen E Schwartz, USA]	Taken into account. We will make it clearer that these are the uncertainties reported by the papers. In the Fry et al. it is mainly based on variation in response between model. In Fuglestvedt et al., 2010 it is from the range of values in the literature.
8-1199	8	49	27	49	29	Point out the need for more studies to confirm these (potentially very large) indirect chemical effects. [Larry Horowitz, USA]	Taken into account. We think this is clear from our discussion of indiect effects.
8-1200	8	49	33	49	34	Is this effect important? Is it estimated anywhere? [Robert Portmann, United States of America]	The effects of CO and VOC are significant; see new figure 8.31 and earlier sections with discussions of role of these gases.
8-1201	8	49	33	49	39	You should mention the forcing contribution of CO2 production from CH4 and CO as well (see, e.g., Daniel et al., JGR, 1998). [John Daniel, USA]	Accepted. The contributions to GWP are given (with references).
8-1202	8	49	34	49	34	Run-on sentence. Start new sentence with "By affecting …" [Larry Horowitz, USA]	Taken into account. Will be changed.
8-1203	8	49	34	49	34	Change "it" to "they" [Larry Horowitz, USA]	Accepted as suggested.
8-1204	8	49	35	8	40	In addition to these chemical effects on atmospheric chemistry, CO has a significant direct radiative greenhouse effect as demonstrated by surface radiative forcing measurements of fluxes of .06 to 0.11 W/m2. W.F.J. Evans, and E. Puckrin, An Observation of the Greenhouse Radiation Associated with Carbon Monoxide, Geophys. Res. Lett., 22, pp 925-928, (1995). Even though the lifetime is short, CO is continually destroyed it; is continually created and maintains a stable level which results in a continuous contribution to the total radiative forcing of about 0.1 W/m2. [Wayne Evans, USA]	Rejected. While the paper referred to studies the surface forcing there is no basis in the literature for a significant RF measured at the top of the atmosphere or at the tropopause. Thus we have not included this effect here. In addition, this section assesses estimates of GWP and GTP for CO in the literature, and none of these studies include any direct effect of CO.
8-1205	8	49	37	49	37	Presumably, the smaller variation across models results in part from the short-term and long-term effects being additive, unlike the case of NOx, in which the net effect is the residual between two larger terms of opposite sign [Larry Horowitz, USA]	Taken into account. This is made clear in the text.
8-1206	8	49		50		Similar discussion required for CO and VOC. What does it all mean? [Stephen E Schwartz, USA]	Taken into account. This is already illustrated in figure 8.28 (new 8.31)
8-1207	8	49				Table 8.11: This table is very useful, but it is important to make clear in the caption or notes the vertical extent of these emission changes - are they surface-only, or do they include elevated sources (e.g., aircraft). Given that the vertical emission profile sensitivity of the these metrics is likely to be much greater than the horizontal sensitivity, the variability shown here reflects differences in vertical transport as well as O3 formation and lifetime. [Oliver Wild, United Kingdom]	We have added "from surface sources" in the table caption. (The table has also been moved to the appendix).
8-1208	8	50	25	51	26	This section currently reads more like a review. There really should be a summary assessment paragraph. [John Daniel, USA]	Taken into account. We have tried to do more assessment.
8-1209	8	50	25			Section 8.5.2.1.3: There is a report draft made by US EPA on Black Carbon: Report to Congress on Black Carbon. Since they are discussing the possible sign of net effect (direct + indirect effects), this should be referred here. [Shigeki KOBAYASHI, Japan]	Rejected. We will not refer to a report, but rather scientiefic papers published after review or (at this stage) papers that are submitted.
8-1210	8	51	10	51	10	Vecchi et al. year is missing in the reference [HASIBUR RAHAMAN, India]	Unlcear which reference this is. And the comment refer to a blank line.
8-1211	8	51	11	51	16	"Since these metric values assume sustained emissions - in contrast to pulses - they cannot be compared directly to other estimates discussed here." This conclusion is not correct. The STREs in Jacobson (2010) include sustained emissions in both the numerator and denominator, so they are not much different from a	Taken into account. We agree that STRE is interesting and relevant since it includes other effects not accounted for in other metrics/studies. But the text

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						GTP or GWP. In fact, Jacobson (2010) shows in Table 4 that the STRE of methane is similar to the GWP of methane. These number should be included in Table 8.14 and it should be pointed out that they account for climate responses as well as forcing mechanisms not accounted for in the other studies, including the effects of BC as cloud inclusions within drops and crystals on cloudiness, the effects of BC on snow albedo, the effects of BC on water evaporation and the resulting feedback, in addition to semidirect, indirect, and direct effects.This explains why the other studies have lower magnitudes. [Mark Z. Jacobson, U.S.A.]	had to be shortened from FOD to SOD so we could not discuss this in more detail. We have however included new metric values from the Bond et al paper (submitted to JGR) and these metric values include several effects.
8-1212	8	51	11	51	16	Be careful with the STREs: my guess is that they are reported in carbon-equivalents rather than CO2 equivalents, and so may need to be multiplied by 12/44 to compare to sustained GTPs, for example. Also, they are calculated using Jacobson's exponential decay function for CO2 based on his "data constrained" lifetime. If those two issues are corrected for, then one can compare the STRE to a GWP in the same way that a GTP-sustained is similar to a GWP... [Marcus Sarofim, USA]	Taken into account. We have made it clear that STRE uses one single lifetime for CO2 which is inconsistent with the literature.
8-1213	8	51	12			I think it IS the GTP for a sustained emission (though check). If it is, then it is the same as an iGTP for a pulse, Peters et al 2011 ERL [Glen Peters, Norway]	Taken into account. We will add this when STRE is discussed
8-1214	8	51	23			Again the table demonstrates that the GWP is utterly obfuscating the fact that the BC and OC do their thing in a week and that the GWP just decays as CO2 continues to do its thing. [Stephen E Schwartz, USA]	Taken into account in table with metrics for the other gases, and this point is made clear in the text. But in the tables for the short lived species we will not give absolute values since these are taken from the literature. And these tables have been moved to Appendix.
8-1215	8	51	42	52	4	Perhaps some discussion that puts these SLCF into the overall forcing perspective would be helpful. Also, if you have insight into a path forward on some of the most important issues, that would be helpful to include. [John Daniel, USA]	Taken into account. This is illustrated in new figures 8.17 and 8.31. Due to page limits, we could not discuss path forward on important issues beyound discussing uncertainties.
8-1216	8	51				Much more impt to give context; what is the ratio of mass emissions of BC or of OC (does this include SOA?) to CO2. that is the beginning of context. Only then does it become possible to tell how impt this all is. [Stephen E Schwartz, USA]	Taken into account in the chapter's discussions of these effects.
8-1217	8	52	6	52	16	Will indirect effect be attributed by emitted component (unlike in the draft Figure 8.27)? This would be useful but presumably ery uncertain. [Larry Horowitz, USA]	Rejected - there is not sufficient peer-reviewed litterature to split the indirect aerosol effect by emitted species. In their assessment of the indirect aerosol effect Chapter 7 has only provide an estimate for the total and not by emitted specie. The Aerosol Indirect effect has been attributed to SO2 in the figures used in SOD.
8-1218	8	52	8	52	16	The diagram should indicate uncertainies and the Levels of Scientific Understanding as in the previous reports [VINCENT GRAY, NEW ZEALAND]	Taken into account - The useful suggestion to add uncertainties is included, but LOSU nor confidence level is included. This can be found in the FOD Table 8.8 that will be located close to this figure in the printed report.
8-1219	8	52	22			This is an interesting figure in terms of intepreting GWPs. In the FAR, they dont say directly but imply GWP100 is a proxy for temp (GWP20 is rates, GWP500 is like sea level, thus one could conclude GWP100 is like T). However, the figure shows that GWP20 is similar to GTP10 (or is that 20?). It suggests that GWP20 is more like temperature at 20 years! GWP100 is not like GTP100, suggesting that GWP100 is not a proxy for temperature. It might be worth elaborating on these points. [Glen Peters, Norway]	rejected due to space restrictions.
8-1220	8	52	54			contrails could be discussed here, see comment 51 [Ruth Doherty, UK]	Rejected. Good suggestion, but not possible due to space restrictions
8-1221	8	52		56		Sectoral. I think this could all be omitted. At best this is pertinent to Mitigation. This WG is supposed to be dealing with the physics and chemistry and climate. [Stephen E Schwartz, USA]	Rejected. Reflects results in the literature. Giving impacts by sector is an alternative to impacts by

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							source. It is also indicated in scoping that we should include this.
8-1222	8	52				fig 27; Need to put error bars on; probably multiple bars per substance reflecting different kinds of uncertainty: parametric, structural. [Stephen E Schwartz, USA]	Accepted. Bars added.
8-1223	8	52				Fig 28. the interesting thing about this figure is that on the 20 year horizon the SO2 almost negates the CO2; if you showed for 10 years it would more than negate. This has major implications for interp of climate change over the last decade not discussed. Any new SO2-emitting power plant that comes on line will be cooling for something over 10 years (depends on sulfur emissions). This needs to be discussed in context of new capacity coming on line especially in china. The theory for all this was worked out long ago: See Schwartz, S. E., Energy Internatl. J. 18, 1229-1248 (1993).Does fossil fuel combustion lead to global warming? (http://www.ecd.bnl.gov/steve/pubs/Fossil.pdf). [Stephen E Schwartz, USA]	Noted. The cancellation for 20 yrs applies if GWP is used. We think GTPs give a better picture of these opposing effects. We agree that there are many aspects of the results in this figure that could be discussed further. But new capacity, e.g. in China, is not a topic for this chapter. This could be covered by other chapters in WGI and by WGIII.
8-1224	8	52				Fig 28. Again get rid of GTP. This is a forcing chapter. And do it as AGWP and you will find it much more informative. [Stephen E Schwartz, USA]	Rejected. This is in line with scoping.
8-1225	8	52				Fig 29. Get rid of it. this is a forcing chapter, not a response chapter. [Stephen E Schwartz, USA]	Rejected. This is in line with scoping.
8-1226	8	53	13			I am not sure that the result is (in)dependent of the sector, but that sectors have a specific mix of pollutants and location. It is this that causes the differences, not that they are a sector. [Glen Peters, Norway]	Rejected. For LLGHGs it doesn't matter which sector that is causeing the emission. But we agree that the NET effect will depend on mix. We will consider rewording.
8-1227	8	53	22			Efficiency and Fig 30. This is a very different use of efficiency (per kg emission) as opposed to the more conventional, temp change per forcing. I am concerned over possible confusion. However the concept is interesting and arguably within the purview of this chapter on forcing since it deals with W m-2. It would seem that the concept is applied only to short lived species, so the issue of confounding what is going on with issues of the CO2 lifetime are not present. The question is what confidence can be placed in any of this given the uncertainties of forcing per emission indicated in tables 8.12, 8.13; and why not include NOx, which has even greater uncertainty, Table 8.11. Again context is required; need to speak to emissions to get a sense of W m-2; why not a second figure that multiplies by current emissions to give that context. [Stephen E Schwartz, USA]	Noted. The figure will not be included.
8-1228	8	54	10	54	19	The discussion in this paragraph is very important, but the issue on the uncertainty of net effect of certain component should be included, since if it is negative, reduction of such component will cause further warming. [Shigeki KOBAYASHI, Japan]	Rejected. Good points. But due to space restrictions we could not go very much into this.
8-1229	8	54	32			I assume "one year pulses" means an instantaneous pulse of one years emissions? [Glen Peters, Norway]	Yes. Should be clear as it is.
8-1230	8	54	32			Unger uses sustained emissions, where you talk about pulses. Maybe specify that to avoid confusion [Glen Peters, Norway]	Taken into account. This is clarified.
8-1231	8	54	37			This will be regionally dependent (not all coal will have cooling, but only coal with a lot of SO2 emissions and SO2 controls vary strongly with region) [Glen Peters, Norway]	Noted. Yes, there are regioal variations. We use global averages here.
8-1232	8	54				Figs 8-31; 8-32. Temperature. Excise. Not for forcing chapter. Strongly dependent on one forcing/response profile. Etc. [Stephen E Schwartz, USA]	Rejected. Given by scoping.
8-1233	8	55	5		5	Societal actions: Inappropriate for WG1: "Analysing climate change impacts by using the net effect of particular activities or sectors may – compared to other perspectives – provide clearer insight into how societal actions influence climate. " Likewise excise Table 8-15 [Stephen E Schwartz, USA]	Rejected. We think it is within the frame of WGI to consider the effects of various drivers; e.g. components, sources and sectors. This is also supported by the scoping.
8-1234	8	55	14	55	14	Table 8.15 should include the STREs from Jacobson (2010), Table 4 (similar to GWPs) for BC +POC from fossil fuel soot, BC from fossil fuel soot, BC+POC from biofuel soot and BC from biofuel soot. [Mark Z. Jacobson, U.S.A.]	Rejected. This is problematic since STRE is using a different CO2 IRF (with just one lifetime). Thus, numbers are not directly comparable. We also had to shorten the section on sectors.

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8-1235	8	55	14	55	15	Stevenson et al (2004) and Stevenson and Derwent (2009) may be useful sources for aviation NOx GWPs. Also Wild et al. 2001. [David Stevenson, UK]	Taken into account. The first and third paper suggested are included in the ranges given in table 8.15. The 2nd paper is referred to in the text.
8-1236	8	55	14	56	7	There is a superscript "y" used in several places (eg: BD dir + albedo) but I couldn't find what that referred to. The notes for the figure only go from (a) to (f). [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted and corrected.
8-1237	8	55	14			Table 8.15. An extremely useful table. Can information relating to land cover change and agriculture (including crops and livestock) be included? Agriculture and animals are included in figure 8.31 so it looks like the information is available for those sectors. However I think it would be important to include deforestation too (not just agriculture). [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted. Thanks. Good suggestion, but we don't think there is a basis for this in the literature. We will however discuss these effects in other parts of the chapter.
8-1238	8	56	10	56	34	This is an important section that needs to be fleshed out. Will ACCMIP provide enough forcing information from enough models to assess uncertainties? [Larry Horowitz, USA]	Taken into account - information from ACCMIP has been used for best estimates and uncertainties for this section.
8-1239	8	56	10	56	34	Mention discussion of spatial distributions of future forcings in 8.6.3 [Larry Horowitz, USA]	Taken into account - a reference to 8.6.3 has been included in this section.
8-1240	8	56	10			Section 8.5.4: This section should also include a discussion of uncertainties in SLCFs in the RCPs. [Twan Van Noije, Netherlands]	Taken into account - a reference to discussion of uncertainties in the RCPs is included.
8-1241	8	57	1	57	5	I think it's worth emphasising that it's not so much that the forcing is different at the surface than the intial "RF" at TOA. It's that rapid adjustments to the atmosphere oppose a portion of the forcing without changes in global mean temperature. The forcing experienced at the surface and TOA is actually the same in the global mean. There may be differences regionally, of course. [Francis Hugo Lambert, United Kingdom of Great Britain & Northern Ireland]	Rejected - this is not referring to rapid adjustments but to absorbing components directly reducing radiation reaching the surface
8-1242	8	57	1			Fig 8-33 is useful. However the analysis is thin. What would be valuable would be a detailed analysis of the reasons for agreement/disagreement among the models. How well are they getting aerosol optical depth, spatially. Extend Kinne ACP 06. Analyze extensive ppties such as optical depth; separated by compound if possible; Compare to satellite. Analyze intensive properties such as residence time as done by Textor ACP 06; forcing per optical depth; optical depth per emission.Textor found order of mag differences in residence times of given substances model to model, yet optical depths similar. So agreement (such as it is represented in the figure might (and in my opinion likely) represents convergence on an expected value for very different reasons, which can be determined only by examination of the intensive variables. The present analysis is weak in comparison to those studies. Another model is the Climate Change Science Program SAP 2.3. Atmospheric Aerosol Properties and Impacts on Climate. Climate Change Science Program (U. S.), Synthesis and Assessment Product 2.3. Chin M., Kahn R. A., and Schwartz S. E., Eds., Washington, DC, 2009. http://downloads.climatescience.gov/sap/sap2-3/sap2-3-final-report-all.pdf See table 1.1. No need to reinvent the wheel; the authors will be pleased to be quoted; alternatively update the analysis for the new model results. But be critical; dont just report averages and std deviations. Look for reasons behind agreement or disagreement. For another example of how to do such an assessment, see Bates et al Atmos. Chem. Phys., 6, 1657–1732, 2006, Figure 13, which clearly portrays model to model differences in a variety of measures. [Stephen E Schwartz, USA]	Rejected: This analysis is to be done by Ch 7
8-1243	8	57	18	57	20	Discuss (somewhere) the resulting strong sensitivity of BC forcing to BC vertical distribution relative to clouds [Larry Horowitz, USA]	Accepted: Discussion added.
8-1244	8	57	27	57	28	It looks to me like there is generally more model spread almost everywhere. It seems that your characterization is contradictory to this. [John Daniel, USA]	Accepted, this is corrected.
8-1245	8	57	32	57	33	Please also mention the comment from Quaas et al. regarding the Penner et al. 2011 study [Ulrike Lohmann, Switzerland]	Accepted, Done
8-1246	8	57	37	57	38	"and has been shown in the modelling study of Ming et al. (2007) to reduce precipitation in the NH as well as to cause a southward shift in the ITCZ." This result had been known for over a decade (Rotstayn et al., 2000;	Accepted, Done

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						Williams et al., 2001). Rotstayn et al. showed the southward shift of precipitation, and Williams et al. also showed the change in the Hadley circulation (meridional streamfunction). Also, this really belongs under the response (8.6.2.2), where, incidentally, Rotstayn et al. (2000) is cited. References: Rotstayn, L. D., B. F. Ryan, and J. E. Penner, 2000: Precipitation changes in a GCM resulting from the indirect effects of anthropogenic aerosols. Geophysical Research Letters, 27, 3045-3048. Williams, K. D., A. Jones, D. L. Roberts, C. A. Senior, and M. J. Woodage (2001), The response of the climate system to the indirect effects of anthropogenic sulfate aerosol, Clim. Dyn., 17, 845–856. [Leon Rotstayn, Australia]	
8-1247	8	57	38	57	38	The southward shift of the ITCZ has also been discussed in AR4 on page 566. I suggest to quote Denman et al. (2007) here as well. [Ulrike Lohmann, Switzerland]	Accepted, Done
8-1248	8	57	43	57	47	More explanation is needed for the regions of very large standard deviation in surface adjusted forcing in Fig. 8.33. [Larry Horowitz, USA]	Accepted, Done, was simple upon a more careful inspection.
8-1249	8	58	1			Same concerns over Figure 8.34 as on fig 8-33. Break it down by model; break it down by compound. Look under the hood (bonnet). [Stephen E Schwartz, USA]	Accepted. Other components have been added.
8-1250	8	58	41	58	42	I do not understand how the peak at mid-century is consistent with what sounds like a monotonic trend in the previous 3 lines. [John Daniel, USA]	Accepted. It is monotonic, with increase and leveling off toward end of century. This has been clarified.
8-1251	8	58	50	58	50	CMIP5 model may better represent global dimming. For one of those models, Haywood et al. [2011] show that modelled changes in surface radiation are consistent with observed changes over Europe and Asia. [Nicolas Bellouin, United Kingdom]	Rejected. This is addressed in Chapter 2 and is beyond scope of Ch 8
8-1252	8	58	50			"greatly underestimated"; how much? what are the implications of this if these are the models that are being used in the assessment of climate change over the twentieth century? [Stephen E Schwartz, USA]	Rejected. We have removed the discussion of global dimming due to space and scope limitation.
8-1253	8	59	1			I think this section needs some discussion of the response to LLGHG forcing near the start. Even though the forcing is quasi-uniform in space, there are many interesting responses of circulation, e.g. a lifting of the tropopause, poleward shift of midlatitude jets, weakening of Walker circulation etc. [Leon Rotstayn, Australia]	Rejected. The opening paragraphs apply to all forcers and the spatial response to LLGHG forcing is covered elsewhere (Ch 10).
8-1254	8	59	1			Avoid dealing with response; stick to forcing [Stephen E Schwartz, USA]	Rejected. Response as it is linked to forcing, is within the purview of this chapter
8-1255	8	59	3	61	20	Repeats some of section 8.1, though better here. Also shindell et al., 2011 paper [Piers Forster, UK]	Taken into account. This section has been shortened.
8-1256	8	59	6			Please check the Taylor ref; I briefly looked at it and did not find that it examined the effect of different spatial distrib of forcing. [Stephen E Schwartz, USA]	Rejected. Taylor et al do not consider different forcings but they do compare correlations of response to forcing and to feedbacks, so the reference is appropriate
8-1257	8	59	25	59	37	It might be good to clarify that many of the studies mentioned in this paragraph focus mainly or entirely on the Atlantic sector (Sahel/Brazil). The earliest studies (comment 20, above) showed the shift in AGCMs in general terms, and then there were several studies that discussed possible links to rainfall in the Sahel or Brazil, starting with Rotstayn and Lohmann (2002). The idea of the ITCZ shift might also be relevant to the Asian region, though I'm not sure if it has been quantified in that way. (It is suggested by Figs. 3 and S9 of Bollasina et al., 2011). Thank you for the Chang et al. reference, which I wasn't aware of. Two other recent papers that use coupled OAGCMs to add weight to the postulated link between aerosol forcing and the Sahelian droughts are Kawase et al. (2010) and Ackerley et al. (2011). The use of OAGCMs in the more recent studies is important, because it doesn't automatically follow from the earlier studies with slab oceans that a similar result will be obtained in a more realistic modelling framework; one way to look at this is to note that, with a slab ocean, the atmospheric circulation takes up all of the cross-equatorial energy transport that is induced by the inter-hemispheric forcing anomaly, whereas in an OAGCM the ocean is expected to take up a substantial fraction of it. Although (to my knowledge) it hasn't been explored systematically, this point is noted by Ming and Ramaswamy (2011) towards the end of their paper; compare also Cai et al. (2006), who looked at the oceanic heat transport. References: Cai, W., D. Bi, J. Church, T. Cowan, M. R. Dix, and L. D. Rotstayn, 2006: Pan-oceanic response to increasing anthropogenic aerosols: impacts on the Southern Hemisphere oceanic circulation. Geophys. Res. Lett., 33, L21707. Kawase, H., M. Abe, Y. Yamada, T. Takemura, T. Yokohata, and	Accepted. Many of these references are now included.

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						T. Nozawa (2010), Physical mechanism of long-term drying trend over tropical North Africa, Geophys. Res. Lett., 37, L09706, doi:10.1029/2010GL043038. . Ackerley, D., B. B. B. Booth, S. H. E. Knight, E. J. Highwood, D. J. Frame, M. R. Allen, D. P. Rowell, 2011: Sensitivity of Twentieth-Century Sahel Rainfall to Sulfate Aerosol and CO2 Forcing. J. Climate, 24, 4999–5014. doi: 10.1175/JCLI-D-11-00019.1. [Leon Rotstayn, Australia]	
8-1258	8	59	30	58	30	More generally, models seem to suggest that the ITCZ moves towards the warmer hemisphere, i.e. if aerosol cool the north hemisphere more than the south hemisphere, the ITCZ moves southward. See for example A. Jones et al., JGR, 2007. [Nicolas Bellouin, United Kingdom]	Accepted. This more general statement is now included
8-1259	8	59	55	59	55	After possible aerosol effects on the Asian monsoon are discussed, it might be worth mentioning the possibility of compensating effects on circulation and rainfall south of the Equator in the Indo-Pacific region. Motivated by an observed multi-decadal trend of increasing rainfall in north-western Australia, we have attempted to tackle this question, first using a low-resolution AOGCM (Rotstayn et al., 2007), and recently, using a CMIP5-class model (Rotstayn et al., 2012). The latter paper also goes into a deeper discussion of possible mechanisms, because we found that there may be important differences between summer and winter. Also, Bollasina et al. (2011) should probably be added for the South Asian monsoon, and you might want to mention the postulated "thermodynamic" response to aerosol forcing, which suggests that there is an aerosol-induced strengthening of the Walker circulation, independent of the pattern of forcing (Ming and Ramaswamy, 2011). References: Bollasina, M. A., Ming, Y., and Ramaswamy, V., 2011: Anthropogenic aerosols and the weakening of the South Asian summer monsoon, Science, 334, 502–505, doi:10.1126/science.1204994. Ming, Y. and Ramaswamy, V.: A model investigation of aerosol-induced changes in tropical circulation, 2011: J. Climate, 24, 5125–5133, doi:10.1175/2011JCLI4108.1. Rotstayn, L. D., et al. (2007), Have Australian rainfall and cloudiness increased due to the remote effects of Asian anthropogenic aerosols?, J. Geophys. Res., 112, D09202, doi:10.1029/2006JD007712. Rotstayn, L. D., S. J. Jeffrey, M. A. Collier, S. M. Dravitzki, A. C. Hirst, J. I. Syktus, and K. K. Wong (2012). Aerosol-induced changes in summer rainfall and circulation in the Australasian region: a study using single-forcing climate simulations. Atmos. Chem. Phys. Discuss., in press. [Leon Rotstayn, Australia]	Accepted partially. We include the Bollasina reference. However the detailed discussion of impacts on hydrology are beyond our scope.
8-1260	8	60	7			Give page number of figure 8.22 [Pieter Aucamp, South Africa]	Taken into account. A place-holder has been added, since page numbers will change
8-1261	8	60	13	60	16	It is useful to mention the CO2 physiological forcing in this section, but currently this is rather buried in the middle of a paragraph on land cover change. It is a separate issue from land cover change so needs its own paragraph. Also I think the potential CO2 physiological effects on surface albedo due to enhanced plant growth should be mentioned here. Increased vegetation cover due to CO2 effects is simulated to lead to decreased surface albedo for a 4*CO2 scenario (O'ishi et al, 2010, Geophys. Res. Lett., 36, L11706, doi: 10.1029/2009GL038217) and forest and shrub cover has been observed to increase in many areas of the world, eg; boreal regions, Esper and Schweingruber 2004, GRL, but also in tropical rangelands too ("woodland thickening"). This all seems relevant to this section. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Accepted. We have expanded this discussion, discussing these references and have added a distinct paragraph on CO2 effects.
8-1262	8	60	16	60	20	Same comment as for chap 8, p. 42, l. 25-35 [Pierre Bernier, Canada]. Copied here: It is important to be really up-to-date with the text on these issues so that public or private investments in mitigation programs that involve land cover changes are done on projects that really achieve their stated goal. Although global modelling is certainly not my field, this conclusion appears too broadly neutral and potentially not consistent with current evidence. For example, see the modelling work of Aurora and Montenegro (Arora, V.K. and A. Montenegro. 2011. Small temperature benefits provided by realistic afforestation efforts. Nature Geoscience V. 4 , p. 514. DOI: 10.1038/NGEO1182) and of Betts et al (Betts, R.A., P.D. Falloon, K.K. Goldewijk, and N. Ramankutty. 2007.Biogeophysical effects of land use on climate: Model simulations of radiative forcing and large-scale temperature change. Agricultural and Forest Meteorology 142: 216-233 DOI: 10.1016/j.agrformet.2006.08.021) that both simulate a strong decrease in afforestation effectiveness for lowering total radiative forcing at high latitudes on account of albedo changes, pointing to a significant climate effect of land cover changes at these higher latitudes. The recent and excellent paper by Lee et al (Lee, X., et al. 2011. Observed increase in local cooling effect of deforestation at higher latitudes. Nature. 479:384-387. doi: 10.1038/nature10588) actually measure the local net cooling effect of deforestation through direct site-level air temperature measurements. In addition, increases in atmospheric water vapour concentration do not	Accepted. This discussion has been expanded and the earlier references with regional relevance have been included.

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						replace increases in temperature or available energy, but actually respond to them (Lacis, A.A, G.A. Schmidt, D. Rind, and R. A. Ruedy. 2010. Atmospheric CO2: Principal Control Knob Governing Earth's Temperature. Science, Vol. 330, No. 6002, pp. 356-359; DOI: 10.1126/science.1190653), and may in addition alter regional or global precipitation regimes (Swann, A.L.S., I.Y. Fung, and J.C.H. Chiang, 2012: "Mid-latitude afforestation shifts general circulation and tropical precipitation." Proceedings of the National Academy of Sciences, v. 109, no. 3, pp. 712-716, doi: 10.1073/pnas.1116706108.). So, even if the effects on air temperature balance out globally, the resulting increases in atmospheric water vapour would alter climatic patterns, a rather negative consequence. [Pierre Bernier, Canada]	
8-1263	8	60	23	60	24	It is not clear if this is still referring to the Arctic or to the entire North American continent. A little more explanation would be helpful in this paragraph. [John Daniel, USA]	Accepted.This has been clarified and expanded slightly
8-1264	8	60	29	60	33	If this effect is very uncertain, as stated on line 33, then why give numbers in lines 29 and 30? Please rephrase to something more like "While some authors have tried to quantify the BC effect……" [Susan Solomon, USA]	Accepted.This has been rephrased to emphasize the uncertainty and qualify the model estimates
8-1265	8	60	29			"has been modeled to cause"; better "has been shown in a model calculation to cause". [Stephen E Schwartz, USA]	Accepted.This has been rephrased.
8-1266	8	60	35	60	35	It is certainly not true that solar forcing has 'increasing during much of the past century'. The evidence supports no increase since at least 1979. This is very important and needs to be fixed. [Susan Solomon, USA]	Accepted, this paragraph has been revised
8-1267	8	60	35	60	38	"Swingedouw 2011" is not in the list of references. [Gareth S Jones, UK]	Taken into account- Reference added.
8-1268	8	60	35	61	8	Much of this material about solar and volcanic effects is really about the forcing, not the response. It might fit better in 8.6.2.1. I realise that it isn't always easy to discuss the response without reference to the forcing. [Leon Rotstayn, Australia]	Taken into account. There is some discussion of response so we feel it belongs under the response section, and we also now point to the response discussion under Chapter 10 (D&A).
8-1269	8	60	35			"Solar forcing has increased during much of the past century"; do you really mean to say this? Much compared to what? greater than ghg forcing? A sentence like this out of context could be dynamite. Even in context I am having trouble finding out what the authors mean by it. Looking at Fig 8.25 I find it hard to reconcile with that statement. [Stephen E Schwartz, USA]	Accepted, paragraph has been revised: Solar forcing has changed about 0.08 Wm-2 between 1900 and the present, with a small downward trend of -0.04 W m-2 between 1986 and 2008 and a small upward trend between 1986 and 2011 of 0.01 W m-2
8-1270	8	60	36	60	48	Since the Hoyt and Schatten reconstruction is not considered up to date as far as I understand, why is this here? Again, please assess (i.e., say what the bottom line is and be critical) and don't review (i.e., don't spend space on studies that are no longer considered valid due to new information). [Susan Solomon, USA]	Taken into account- Text changed. The works cited here are those by Meehl et al. (2003 and 2008). In the first one the authors use de Hoyt and Schatten (1993) TSI reconstruction and in the second one they use the Hoyt and Schatten (1993) and the Lean et al. (1995) reconstructions. Meehl et al. (2003, 2008) study the period between ~1900 and 2000. Attending exclusively to the values, from 1900 on, the two TSI reconstructions are similar to others published more recently.Moreover, the authors themselves (Meehl et al., 2003) when commenting on the Hoyt and Schatten and Lean et al. reconstructions indicate that "Such differences between the solar forcing datasets, important for a detection/attribution study, are less crucial in the persent paper since we are focusing on forcing/response aspects. It is the mechanism of the response that are the focus of this paper, and these should be comparable with either solar forcing data set." This comment can apply to any other TSI reconstruction between 1900 and 2000.

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8-1271	8	60	38	60	38	Reference for Swingedouw et al., 2011 is missing. [Leon Rotstayn, Australia]	Taken into account- Reference added.
8-1272	8	60	39	60	40	What is "much larger"? Is it the change over the solar cycle, the trend, the absolute TSI, etc? [John Daniel, USA]	Taken into account- Text changed. We use a wrong phrase: Compared to other reconstructions, the Hoyt and Schatten (1993) reconstruction had a much larger increase in TSI between 1700 and 1800, but from 1900 on, it is similar to reconstructions published recently. As the Meehl et al. (2003 and 2008) studies referred to the years 1900 to 2000, the phrase "much larger" has been deleted. See also answer to comment by Susan Solomon above.
8-1273	8	60	54	60	55	It seems like you are comparing a regional surface absorption with a global average forcing here. Please clarify that these are two very different things and are not expected to agree. [John Daniel, USA]	Accepted. This has been clarified.
8-1274	8	60	54	60	55	If possible present the Meehl global average value, too. [John Daniel, USA]	Rejected. The global number was not provided in Meehl et al for the surface forcing.
8-1275	8	61	1		20	This is pretty elementary and not quantitative; how well is forcing known? [Stephen E Schwartz, USA]	Noted - There have been few eruptions over time, so little opportunity to measure the changes in radiation. Pinatubo is well-known, and we extrapolate from that.
8-1276	8	61	10	61	11	Please approximately quantify the relative importance of the shortwave and longwave terms. [John Daniel, USA]	Accepted - The shortwave effect is an order of magnitude larger than the longwave effect.
8-1277	8	61	16	61	17	Clarify. The heating itself does not destroy ozone. [John Daniel, USA]	Accepted - The reaction rates of ozone production and destruction reactions are temperature-dependent, resulting in net destruction of ozone.
8-1278	8	61	22			Avoid dealing with response; stick to forcing [Stephen E Schwartz, USA]	Rejected: Some response has been assigned to this section
8-1279	8	61	29			"significantly"; this is one of those weasel words. does it mean "significantly with respect to some statistical test"? or "importantly"? If the latter suggest "substantially" but then suggest go on and be quantitative. [Stephen E Schwartz, USA]	Taken into account. In the Second Order Draft, we put more assessment for the future aerosol loading quantitatively.
8-1280	8	61	33	61	41	It might be interesting to include similar figures for total RF as well. [John Daniel, USA]	Taken into account. Figure 8.36 is revised to one similar to Figure 8.33 for total aerosol and ozone RFs.
8-1281	8	61	39	61	41	These lines say the same thing as the immediately following lines 43 to 45. In case more references are needed, Bellouin et al. [2011] is relevant. [Nicolas Bellouin, United Kingdom]	Taken into account. The contents between lines 39 - 41 and 43-45 is combined. In this section, results from intercomparison projects have priority because they did along prescribed standard protocols with participation from many models for quantitative comparisons.
8-1282	8	61	47			I cannot see the northward shift of the ITCZ in Figure 8.36. [Robert Waterland, United States of America]	Taken into account. The northward shift of the ITCZ is not directly shown in Figure 8.36. The text is revised not to be misleading.
8-1283	8	61	49			Cox et al. 2008 - this is the result from one model from AR4, do the CMIP5 models project this extreme drying? [Ruth Doherty, UK]	Rejected. Cox et al. (2008) itself is a work after AR4. The similar referable papers from the CMIP5 have not been published yet.
8-1284	8	61	53	61	54	I presume that overall the Artic will continue to warm though. If that is correct, this sentence should be rephrased to make it clear the aerosol change has a cooling influence, or offsets parts of the warming, but does not lead to overall cooling. [John Daniel, USA]	Accepted. It makes it clear.

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8-1285	8	62	1			Fig 8-36. What accuracy can be ascribed? [Stephen E Schwartz, USA]	Taken into account. Information on the uncertainty is added based on the ACCMIP/CMIP5 in the Second Order Draft.
8-1286	8	62	1			The chapter ends with a whimper, not a bang. One would never infer from this chapter that the forcing is still uncertain by a factor of 2 (fig 8-25) and the implications that this has for interp of clim change over 20th century. Or that the present situation 1.4 to 2.9 W m-2 is a great improvement over AR4, 0.6 to 2.4 W m-2, factor of 4. What is responsible for the great reduction in uncertainty. what are the prospects for improving this in the future. are we to be stalled with this factor of 2 uncertainty or can we expect it to continue to decrease. Can we really believe this improvement. Is Fig 8-25 the bottom line that is being handed off to the community? Do we expect this to become the iconic forcing figure analogous to Fig spm-2 of AR4? Are all the components of Fig 8-27 included in Fig 8-25c? Will Fig 8-25C have LOSU qualifiers on it? Fig 8-27 should have error bars on it, and these should be propagated to a sum. Does it agree with 8-25c (just an expansion of it)? It seems as if the author committee just ran out of steam or time and didnt really finish the chapter or its implications for understanding and modeling clim change over the 20th century and first decade of 21st. In my judgment there is really still a lot of work to be done. Forcing is central to understanding; in AR4 it is clear that uncertainty in forcing limits understanding. Is that still the situation? [Stephen E Schwartz, USA]	Taken into account. The structure of the chapter has been strongly modified. The most important findings are summarized in the beginning of the chapter (Executive Summary) and not in the end of the chapter. The ES describe why the range in the forcing has been reduced since AR4 as well as why the magnitude is larger. The main cause for the reduced range in the RF is the smaller range in the estimates for aerosols and this is described in ES and the chapter and references are made to Chapter 7, where the details are discussed. A completely new version of figure 8.27 is generated including uncertainties. This figure is made consist with the main bar chart (Fig 8.25c in FOD). Fig 8.25c will not include LOSU since this is made in a separate figure for comparison with previous assessments.
8-1287	8	62	11	63	2	FAQ 8.1: The present first paragraph of this FAQ sets the scene rather than providing a high-level summary answer (which is the standard WG1 FAQ style). I suggest inserting a new italicised first paragraph which in 5-6 lines covers the main points from the full answer - in particular the key points from lines 49-57 on this page. [David Wratt, New Zealand]	Accepted. Paragraph added
8-1288	8	62	13	62	57	This section needs to be more carefully written. Water vapor is a messy issue and FAQs appeal to many not as well informed about climate. Some looking to at this FAQ will have heard confusing messages and it is important to ma,e the basics very clear. I recommend starting from scratch. Water vapor in the troposphere and water in the stratosphere are two different stories. It would be most instructive to lay out what the water vapor content was before 1750, what it is today, and how it got there. Then distinguish clearly throughout what is background and what is added by humans (directly and indirectly through the addition of anthro GHGs to the atmosphere). This section needs to clarify up front the issue with water and clearly deliver the message that it is a slave to other properties of the Earth system. That is better than waiting until the last sentence. [James Butler, United States of America]	Rejected. Although we will change the FAQ according to other comments, we do not agree with the recommandations as stated here
8-1289	8	62	14			FAQ 8.1 add a summary sentence as done for FAQ 8.2 [Ruth Doherty, UK]	Accepted. Paragraph added
8-1290	8	62	15	62	21	Need to note at the outset that the primary source of water vapor into the atmosphere is evaporation from the ocean, which is a response to surface water warming. It would also be good to show some quasi-quantitative amounts so that the "additional" amount put in by direct human activities is put in perspective. Order of magnitude estimates are fine for this. [James Butler, United States of America]	Rejected. FAQ already criticized as too technical. Puting numbers may increase the problem
8-1291	8	62	15	62	21	What a botch up! You will do anything to cover up the primary importance of water vapour [VINCENT GRAY, NEW ZEALAND]	Rejected. The importance of water vapor on climate is clearly stated "water vapor has the largest greenhouse effect".
8-1292	8	62	23	62	23	"to" should be "than" [James Butler, United States of America]	Editorial. Agreed
8-1293	8	62	23	62	30	I am not an expert on water vapour, but this justification seems unjust to me! Chapter 8 spends a lot of time discussing SLCF and even discusses contrails as short as a few hours. Thus, saying that the water vapour is in the atmopshere for one week is not really a justification to not quantify it (otherwise, the same arguement applies to many SLCFs). Is it posisble to estimate a radiative efficiency for water vapour and compare it to other SLCFs? [Glen Peters, Norway]	Accepted. Residence time is not the only factor. The other factor is that there is a "natural" flux that is much larger than the anthropogenic flux, so that the added contribution does not impact significantly the concentration.
8-1294	8	62	25	62	25	water droplets or ice particles [Gavin Schmidt, USA]	Editorial. Agreed

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8-1295	8	62	26	62	26	more like 10 days (25 mm column water vapour / 3mm/day precip) [Gavin Schmidt, USA]	Agreed. Added
8-1296	8	62	26	62	26	Replace "As a consequence, any additional water vapour injected into" with "As a consequence, on short time scales any additional water vapour human activities inject into". [Robert Waterland, United States of America]	Editorial
8-1297	8	62	26	62	28	I think the wording could be improved here. Emissions of water vapour do have an impact on atmospheric concentrations, otherwise there would be no water vapour feedback, but the control is not from the source term but rather from the sink term. [Olivier Boucher, France]	Agreed. Corrected
8-1298	8	62	32	62	37	Not only do the relative amounts in the stratosphere and troposphere need to be elucidated to show the order-of magnitude differences, but it should be briefly noted why water in the stratosphere is of concern to climate change (which it is, very much so). [James Butler, United States of America]	Agreed. Added
8-1299	8	62	35			Please expand the acronym "RF" for the benefit of the general reader. [David Wratt, New Zealand]	Editorial. Agreed
8-1300	8	62	42	62	44	Water vapor concentration in the atmosphere does not increase just with heating of the air. Only the potential of the air to contain water does, so we need to be careful how this is worded. There must be a source of the water vapor and that predominantly is the ocean. Of course, the story is different if one is considering stratospheric air alone and that needs to be better explained here. [James Butler, United States of America]	Agreed. Corrected
8-1301	8	62	49	62	57	What an implausible excuse! [VINCENT GRAY, NEW ZEALAND]	Rejected. There is ample evidence for what is written in this paragraph
8-1302	8	62	51	62	52	Make it clear you are talking about all GHGs, natural and anthropogenic. Otherwise, it could be interpreted that if we eliminate anthropogenic GHG emissions, we will "plunge the Earth into a frozen state". [John Daniel, USA]	Rejected. We do not think there is ambiguity here
8-1303	8	62	56			It says that water vapor "amplifies any initial forcing by a factor of typically 3". Here one should be more careful as to whether one refers to an amplification of the warming or the forcing. If we apply 4 W/m2 and (in the absence of feedbacks) get a temperature increase by 1 K, then water vapor will amplify the forcing by an additional 1-2 W/m2 (see AR4, or soden held 2006), i.e., by 25%-50%. If the amplification refers to temperature, the impact will be delta T= 0.25 * 4 /(1-0.25*2) = 2 K (assuming that the feedback is 2 W/K/m2. Thus, I propose that the sentence is rewritten into "amplifies any initial warming by a factor of typically 2". [Christian Azar, SWEDEN]	Rejected. This chapter is mostly about forcing, not warming, although the later is implciit. Besides, there is the assumption of a linear relationship between forcing and warming, at least to some extent.
8-1304	8	62				Do not use abreviations and acronyms at all in the FAQs. It just confuses the intended audience. [Pieter Aucamp, South Africa]	Editorial. Agreed
8-1305	8	62				faq 1 - metion irrigation explicitlty, say how much of earth's natural greenhouse effect water vapour and clouds account for? [Piers Forster, UK]	Rejected. Irrigation is already stated. We are reluctant to add numbers to an already technical FAQ
8-1306	8	62				The treatment of FAQ 8.1 on the Importance of Water Vapour for Climate Change is very much appreciated. Congratulations to the authors for that contribution. [Klaus Radunsky, Austria]	Noted
8-1307	8	62				FAQ 8.1: This FAQ should be revised to clarify anthropogenic vs. natural forcing, and provide quantitative numbers to illustrate the role of water vapour in the forcing as a feedback. A schematic figure could be used to help frame this FAQ. [Thomas Stocker/ WGI TSU, Switzerland]	Agreed. Corrected
8-1308	8	62				FAQ 8.1: 'Control knob' in final paragraph - needs to be clear that this is the main 'anthropogenic control knob'. [Thomas Stocker/ WGI TSU, Switzerland]	Agreed. Corrected
8-1309	8	62				FAQ 8.1: Overall we consider this FAQ to be rather technical. Technical terms need to be explained, possibly within (...). [Thomas Stocker/ WGI TSU, Switzerland]	Agreed. Corrected
8-1310	8	63	5	64	20	FAQ 8.2: This FAQ does a nice job of covering the relevant issues. However there are some places where wording could be clarified or expanded to assist the general reader. [David Wratt, New Zealand]	Noted: this will be clarified
8-1311	8	63	5			I suggest replacing "detrimental" by "warming" for clarity (and to keep away from wording which could be criticised as making a value judgement). [David Wratt, New Zealand]	Taken into account: text was changed

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8-1312	8	63	7	64	20	There are no references in the FAQs which seems like a conscious decision. However, there are some places where references could be very useful, as statements are made here that weren't made in the main text. Particularly for the statements of air pollution health impacts and studies showing health and climate co-benefits of methane and BC reductions. [Susan Anenberg, USA]	Rejected: no references in FAQs
8-1313	8	63	7	64	20	Would this FAQ be more appropriately placed in Chapter 11, where the impacts of air pollution controls on climate are discussed (p. 11-42 and 11-43)? [Susan Anenberg, USA]	Noted:because of the more extensive discussion of chemistry and ozone in Ch 8., it makes sens to have it here.
8-1314	8	63	7	64	20	Would this FAQ be more appropriately placed in Chapter 11, where the impacts of air pollution controls on climate are discussed (p. 11-42 and 11-43)? [Susan Anenberg, USA]	same comment
8-1315	8	63	16	63	17	Policies have been implemented also because of the concern for acidifcation impacts [Henning Rodhe, Sweden]	Taken into account: point was added
8-1316	8	63	20	63	20	The transition from "pollution controls" to "generated GHGs" is awkward. I would start by stating how GHG and aerosols differ in their climate impacts, then discuss the impact of policy-induced changes. [Nicolas Bellouin, United Kingdom]	Noted: the FAQ has been majorily reorganized to address such comment
8-1317	8	63	25	63	31	first and last sentence of this paragrpah seem to contradict each other, the first sentence could perhaps be clearer [Ruth Doherty, UK]	Noted: first sentence was made more to the point
8-1318	8	63	28	63	31	"… the impact of emission changes (e.g. transportation) can become quite complicated owing to atmospheric chemistry and couplings between all targeted emissions. For example, while reducing tropospheric ozone, nitrogen oxide emission controls…." This seems to suggest the transportation sector has high NOx emissions compared to other emissions in that sector and compared to other sectors and therefore it might not be worth reducing the emissions from this sector. Maybe just remove the "(e.g. transportation)". [Helen Worden, USA]	Noted: text was clarified
8-1319	8	63	33	63	34	Syntax problem here. Try this: "Because of the varying shape and composition of aerosols, the net effect of their interaction with radiation ranges from . . . . [James Butler, United States of America]	Taken into account: text was changed
8-1320	8	63	35	63	36	Replace "have a detrimental effect on" with "will warm the climate". [Robert Waterland, United States of America]	Taken into account: text was changed
8-1321	8	63	36	63	36	"important" is an ambiguous term. Try "the particle with the greatest warming influence" or something like that. [James Butler, United States of America]	Taken into account: text was changed
8-1322	8	63	36	63	38	The sentence "strong candidate for combining air quality and climate improvements" should be commented by the fact that net effect could be negative, and if it is a case, air quality and climate change issues are trade-off. [Shigeki KOBAYASHI, Japan]	Taken into account: such balance is now more explcitly indicated
8-1323	8	63	36	63	38	I suggest some rewording of this sentence, to make it clear to the general reader that reducing black carbon in the atmosphere would have a cooling effect on the climate (as well as improving air quality). [David Wratt, New Zealand]	Noted: the FAQ has been majorily reorganized to address such comment
8-1324	8	63	38			Please explain "hydrophilic" for the general reader. [David Wratt, New Zealand]	rejected: this is standard use of the term
8-1325	8	63	42			Please explain the meaning of "tropospheric", for the general reader - perhaps by adding a height range in brackets. [David Wratt, New Zealand]	rejected: this is standard use of the term
8-1326	8	63	49	63	49	Please use a more informative word than "domestic" here. [Robert Waterland, United States of America]	Taken into account: text was changed
8-1327	8	63	50	63	50	Replace "shown the increase" with "shown an increase". [Robert Waterland, United States of America]	Editorial: text is changed
8-1328	8	63	51	63	54	This is assumed in the RCPs, so perhaps this point needs to be made in the main text for context. One of the limitations of the RCPs is that air pollution emissions were assumed to occur beyond "current legislation" but as this statement points out, it is unclear whether that will actually happen. [Susan Anenberg, USA]	Rejected: too specific and technical for an FAQ

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8-1329	8	63	53	63	53	"product" should be plural. [James Butler, United States of America]	Rejected: text is removed
8-1330	8	63	53	63	53	Change "curb" to "curve" (or "trajectory") [Larry Horowitz, USA]	Taken into account: this typo is fixed
8-1331	8	63				Do not use abreviations and acronyms at all in the FAQs. It just confuses the intended audience. [Pieter Aucamp, South Africa]	Taken into account: acronyms are spelled out
8-1332	8	63				faq 2 is good [Piers Forster, UK]	Thanks !
8-1333	8	63				Faq on water vapor. Water vapor is not a forcing agent; it is not external to the climate system. It is part of the climate system; page 4 line 16 clearly states that forcing is external. Get rid of the faq. [Stephen E Schwartz, USA]	Rejected. Irrelevant comments
8-1334	8	63				FAQ 8.2: Please add full chemical names for the abbreviations used in the Figure. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: figure was redone accordingly
8-1335	8	64	2	64	2	I would add the following sentence. "Just as improvements in air quality can affect climate change, so can climate change influence air quality. [Robert Waterland, United States of America]	Noted: the FAQ has been majorily reorganized and this comment does not quite apply anymore
8-1336	8	64	2	64	10	It is not just meteorology that drives the climate impacts on air quality, but changes in temperature affecting reaction rates, and emissions (including both biogenic VOCs and occurrence of wildfires). [Susan Anenberg, USA]	Noted: a broader statement is now made
8-1337	8	64	4	64	4	More specifically, an active, or precipitating, front. For a less technical FAQ, "band of rain" may be a better term. [Nicolas Bellouin, United Kingdom]	Rejected: this is not just about rain but also of lifting pollutants out of the boundary layer
8-1338	8	64	4			clarify that hot conditions are only associated with high pressure systems occuring in summer (i.e. cold conditions in winter) [Ruth Doherty, UK]	Taken into account: point was added
8-1339	8	64	6			refer to chapter 11 section 11.4.3.2.1 (which does not use the terminology climate penalty unlike here). Outweigh or offset "many measures taken to improve air quality"? [Ruth Doherty, UK]	Rejected: the FAQ should be stand-alone.
8-1340	8	64	7	64	8	Replace "Based on model studies this impact of climate change (sometimes referred to as “climate penalty”) could in some regions outweigh many measures taken to improve air quality." with "Rising global temperatures may result in more frequent stagnant high pressure episodes. This would lead in turn to increases in surface ozone which may outweigh many measures taken to improve air quality. This phenomenon is commonly termed a "climate penalty"". [Robert Waterland, United States of America]	Noted: the FAQ has been majorily reorganized and this comment does not quite apply anymore
8-1341	8	64	10			Humidity changes will most likely have a greater effect on O3 vis loss by O1D (as outlined in chpater 11-section 11.4.3.2.1) rather than though wet deposition changes. [Ruth Doherty, UK]	Noted: the FAQ has been majorily reorganized and this comment does not quite apply anymore
8-1342	8	64	13	64	14	Isn't it also becoming clear that climate influences air quality? If the section is to be ended on this, both should be mentioned. [James Butler, United States of America]	Noted: the FAQ has been majorily reorganized and this comment does not quite apply anymore
8-1343	8	64	16			FAQ 8.2 Figure 1: Please consider explaining the chemical symbols and the acronyms in the figure caption for a general audience, e.g. by adding names in brackets (such as CH4 (methane), CO (carbon monoxide), ... [David Wratt, New Zealand]	Taken into account: figure was redone accordingly
8-1344	8	69	35	69	35	Incomplete reference. [Georg Feulner, Potsdam]	Accepted - reference corrected
8-1345	8	70	9	70	9	Reference? [Larry Horowitz, USA]	Accepted - reference corrected
8-1346	8	76	28	76	29	Use full initials of authors: Shindell, D.T., Schmidt, G.A., Mann, M.E., Rind, D., Waple, A., Solar forcing of regional climate change during the Maunder Minimum, Science, 294, 2149-2152, 2001. [Michael Mann, USA]	Accepted - reference corrected
8-1347	8	81				Figures general: For many figures, uncertainty from observations, or models are missing. Would be good if this could be provided. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - Several figures have been deleted and a better balance of figures between the various have been made. Substantially more information on the uncertainties have been included in

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							the figrues.
8-1348	8	82	1	82	8	Fig. 8.1: Is the net flux imbalance at TOA or the tropopause? In (b) text says TOA but figure suggests tropopause. [Helen Worden, USA]	Rejected. Because the stratosphere adjusts, the flux imbalance is the same throughout the stratosphere (ie at toa, tropopause, and between)
8-1349	8	82				Name axis and give broad indication of values [Pieter Aucamp, South Africa]	Editorial. Be more explicit in Fig legend
8-1350	8	82				Fig 1 - are all these different forcings need. I would just have RF, AF and response. Where does the regression method fit in? [Piers Forster, UK]	Rejected. Irrelevant
8-1351	8	83	1	83	2	Figure 8.2 This is the only part of this chapter which I think is a step backwards from AR4. It goes back to a rather out-of-date view of the cause-effect chain of climate influences and impacts with RF as the centrepiece, and so gives the misleading impression that non-radiative forcings do not matter. In AR4, figure 2.1 was more balanced an included non-radiative forcings (even though it used the cumbersome and confusing term "non-initial-radiative effects". I do agree that it is useful to show the forcing concept in the policy and metrics context, but the scientific concepts in the middle should not be over-simplified. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Rejected. We do acknowledge the importance of the non-radiative forcings, but this figure is meant to illustrate the chain from emissions to impacts and not the cause effect chain of man made perturbations of climate in general. Since the chapter will be re-structured the figure will be moved so it will be more associated with the emission metric issue than with forcing in general. But we agree that these effects are important. The effects are discussed in other parts of the chapter.
8-1352	8	83	1			Emissions have an uncertainty of about 5+% (see Chapter 6), and concentrations have a much lower uncertainty. Thus, the arrow for increasing incertainty is not quite correct. Concentrations are the most certain. [Glen Peters, Norway]	Rejected. We think uncertainty in modelled concentrations adds to emission uncertainty so it increases downward
8-1353	8	83		83		Figure 8.2 should also include adaptation since the impact of an emission cannot be calculated without its consideration. This is critical in the generation of a metric since metrics based only on climate (e.g. global temperature) omits consideration of the relation between climate and impacts. [Haroon Kheshgi, United States of America]	Rejected. Adaptation is beyond the scope of this paper and report. The comment is also unclear.
8-1354	8	83				Fig 2 - not sure if boxes on right and left are needed [Piers Forster, UK]	Rejected. The boxes explain what the metrics are used for.
8-1355	8	83				Figure 8.2: if GWP and GTP values are computed by using a new CO2 IRF which includes climate-carbon cycle feedbacks, an arrow going from "Climate Change" to "Atmospheric Concentrations" would be needed. [Katsumasa Tanaka, Switzerland]	Rejected. We agree with the comment but in order ro keep the figure simple and conceptual we did not include more arrows.
8-1356	8	84	1			Perhaps worth noting that the AGTP depends on RF (hence E). Due to the IRF in the AGTP equation, the response decays (hence the use of the memory or discounting analogy). [Glen Peters, Norway]	Taken into account. More equations are now given in supplemenrtary material.
8-1357	8	84	8	85		Figs 8.3 and 8.4 - too tutorial for IPCC. I would delete [Piers Forster, UK]	Rejected. We think (and have experienced that) these figures are needed to help the readers understand.
8-1358	8	84				In Fig 8,3 (a) it is impossible to see the effects of the different colours. Use lighter shades for the main colours. [Pieter Aucamp, South Africa]	Taken into account, The figure has been improved.
8-1359	8	84				It is not clear how the 3 curves have been normalised (or not). The i index for the AGTP iand delta Ts in the second equation should come right after the AGTP/delta Ts, not after the brackets. [Olivier Boucher, France]	Taken into account. The index will be corrected. Re normalization: we think it is clear as it is.
8-1360	8	84				Fig 8.3. I do not find the figures all that informative, as they are ratios. I suggest present plots of AGWP, AGTP (as well); if you preset both, at least it will pave the way for the eventual transition to AGWP. [Stephen E Schwartz, USA]	Rejected. These figures are not showing ratios; they show absolute values for integrated RF and delta T in response to pulses. The equations on the right hand side show ratios.
8-1361	8	84				Fig 8.3: Figure needs more explanatory information, eg, include a legend providing colors. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. The figure has been improved.
8-1362	8	85	4	85	6	It is unclear what is being plotted here. Clarify by adding to figure caption: "representing the temperature	Taken into account. We will explain better what the

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						change in year xxxx resulting from …". [Larry Horowitz, USA]	figure is showing and text on x-axis has been changed.
8-1363	8	85	4			I think the caption might need a little more explaination and also with reference to looking very similar to Manne and Richels [Glen Peters, Norway]	Taken into account. We will explain better what the figure is showing and text on x-axis has been changed.
8-1364	8	85				Put legend in box to make it clearer. [Pieter Aucamp, South Africa]	Rejected. We don't think a box will make a difference. But we have tried to improve the figure.
8-1365	8	85				The figure requires a better explanation [Henning Rodhe, Sweden]	Taken into account. We will explain better what the figure is showing and text on x-axis has been changed.
8-1366	8	86	0	86	0	I don't think this figure is needed. [Susan Solomon, USA]	Taken into account - only global mean emissions will be shown.
8-1367	8	86	1	86	8	Units? [Larry Horowitz, USA]	Taken into account - units are included
8-1368	8	86		92		Seems like too many figures for the chemistry section and ACCMIP. Makes chapter imbalanced - try to rationalize? [Piers Forster, UK]	Taken into account: fewer figures from ACCMIP are being used in the SOD
8-1369	8	86				Is it possible to add the continent Africa to the graphs? It has a large number of counties about which little is said in the report. [Pieter Aucamp, South Africa]	Noted: figure in SOD will only show global numbers. Additional figure in the supplement will have all regions, including Africa
8-1370	8	86				Figure 8.5 Very useful figure. How about also including the land cover change reconstructions& projections from the RCPs as well? [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Rejected: not the place for land-use discussion
8-1371	8	86				Provide the unit for what is shown. [Olivier Boucher, France]	Taken into account: units are added
8-1372	8	86				fig 8.5. is postage stamp figure necessary here, what are you trying to say.Expect that emissions vary with region? [Piers Forster, UK]	Taken into account: figure in SOD will only show global numbers.
8-1373	8	86				Fig 8.5 What are the units in this figure? [Gareth S Jones, UK]	Taken into account: units are added
8-1374	8	86				Figure 8.5: The resolution of this figure could be improved. [Fiona O'Connor, United Kingdom of Great Britain & Northern Ireland]	Taken into account: figure in SOD will only show global numbers
8-1375	8	86				Split into 2 so that the hoizontal scales are expanded [Henning Rodhe, Sweden]	Taken into account: figure in SOD will only show global numbers. Additional figure in the supplement will have all regions, including Africa
8-1376	8	87	0	87	0	I don't think this figure is needed. [Susan Solomon, USA]	Taken into account: figure is removed
8-1377	8	87	1	87	5	Units? [Larry Horowitz, USA]	Rejected: figure is removed
8-1378	8	87	4	87	5	Maybe add that the colour code is as in Figure 8.5. [Dirk Olivié, Norway]	Rejected: figure is removed
8-1379	8	87				This is a very confusing set of graphs. There is no legend for the colours and the dots add to the confusion. Rather replace them with different lines. [Pieter Aucamp, South Africa]	Rejected: figure is removed
8-1380	8	87				Unit? [Olivier Boucher, France]	Rejected: figure is removed
8-1381	8	87				Fig 8.6 What are the units in this figure? [Gareth S Jones, UK]	Rejected: figure is removed
8-1382	8	87				Figure 8.6: The resolution of this figure could be improved. [Fiona O'Connor, United Kingdom of Great Britain & Northern Ireland]	Rejected: figure is removed

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8-1383	8	87				The black dots are not connected, it is thus partly difficult to see how many other scenarios are valid. If the central line is thought to be more reliable, would it be useful to make the line bigger? [Michael Schulz, Norway]	Rejected: figure is removed
8-1384	8	87				Fig 8.6: Units are lacking, individual inventories should be identifiable [Thomas Stocker/ WGI TSU, Switzerland]	Rejected: figure is removed
8-1385	8	88	1	88	2	In Figure 8.7 cartoon of Sun (left) should appear fully. [Lokesh Kumar Sahu, India]	Rejected: figure is removed
8-1386	8	88				Replace the word "Deposition" with "Destruction". It is a more accurate term. [Pieter Aucamp, South Africa]	Rejected: figure is removed
8-1387	8	88				Fig. 8.7 This figure would be improved by adding 'ozone' or 'O3' in strategic places such as title (Tg/yr O3) and similiarly in production, sink, deposition and burden values. Also to add 'O3 production' to label left hand purple line. It is not clear what the right hand purple line is meant to represent. [David Fahey, USA]	Rejected: figure is removed
8-1388	8	89	1	89	5	Explain what the curve represent in plot. [Larry Horowitz, USA]	Rejected: figure is removed
8-1389	8	89	1	89	5	Fig. 8.8 needs a legend or text describing the lines and symbols [Helen Worden, USA]	Noted: color lines will be explained or replaced by single color for all models
8-1390	8	89				What do the colours describe? Need a legend. [Pieter Aucamp, South Africa]	Noted: color lines will be explained or replaced by single color for all models
8-1391	8	90	1	90	6	Figure 8.9. Not quite clear what the correlation is (last column). Is this the correlation of the observed and modelled seasonal cycle at each vertical level? This figure should perhaps guide which levels should be plotted in Figure 8.8. [David Stevenson, UK]	Rejected: figure is removed
8-1392	8	91	0	91	0	This figure could be made more useful if an additional y-axis were added showing the range of estimated RF (which, as is noted in the chapter, approximately scales with the tropospheric column). [Susan Solomon, USA]	Taken into account: right axis will have the equivalent RF
8-1393	8	91	1	91	6	Fig. 8.10 black dot in the legend should be an error bar symbol for TES O3. [Helen Worden, USA]	Taken into account: this is corrected
8-1394	8	91	1	91	7	Figure 8.10. I'm not sure where the 1850-2005 change in O3 column/DU for ACCENT-AR4 comes from, but it is not Stevenson et al (2006) as indicated in the figure. Probably should be Gauss et al (2006) - and NB this was ACCENT PhotoComp Experiment 1, and was [David Stevenson, UK]	Rejected: this number indeed comes from Stevenson et al
8-1395	8	91				Figure 8.10: Results from recent simulations carried out for the ACCMIP O3/CH4 RF experiment can also be included. [Twan Van Noije, Netherlands]	Rejected: this would make the plot more complicated and does not convey the targeted information
8-1396	8	91				Figure 8.10: For reference, the HTAP multimodel study gave a mean 2000 burden on 30.1 +/- 3.7 DU based on 13 models, but I believe that these numbers remain unpublished at the present time. [Oliver Wild, United Kingdom]	Taken into account: this number will be added in the SOD figure
8-1397	8	92	0	92	0	I am not sure this figure is useful. If it is kept, it needs a legend - but it would be better to discuss in text and drop. [Susan Solomon, USA]	Noted: we find this figure useful but it will be redrawn to address comment
8-1398	8	92	1	92	5	Which models are which lines? At least indicate that each line represents one model (if that is indeed the case). [Susan Anenberg, USA]	Noted:this figure will be redrawn to address comment
8-1399	8	92	4	92	5	Change to "Time evolution of methane lifetime (with respect to loss by tropospheric OH) …" [Larry Horowitz, USA]	Noted::this figure willl be redrawn to address comment
8-1400	8	92	4			Figure 8.11: Need to label the individual coloured lines or provide a key. [Robert Waterland, United States of America]	Noted::this figure will be redrawn to address comment
8-1401	8	92				What do the colours describe? Need a legend. [Pieter Aucamp, South Africa]	Noted::this figure willl be redrawn to address comment
8-1402	8	92				Figure 8.11: The resolution of this figure could be improved. [Fiona O'Connor, United Kingdom of Great Britain & Northern Ireland]	Noted: higher resolution will be used in the SOD
8-1403	8	92				Fig 8.11. Please, give a legend for the curves. [Ilkka Savolainen, Finland]	Noted: :this figure will be redrawn to address

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							comment
8-1404	8	92				Figure 8.11: Please indicate model names. [Twan Van Noije, Netherlands]	Noted::this figure willl be redrawn to address comment
8-1405	8	93				Put legend in box to make it clearer. [Pieter Aucamp, South Africa]	Taken into account- There are specific instructions to prepare figures for the report and boxes are not allowed.
8-1406	8	93				Comment on Figure 8.12: I have having difficulty seeing where the (low) absolute numbers for the ACRIM curve come from, which is not consistent with e.g., Figure 2a of Willson and Mordvinov 2003. I had thought that PMOD/ACRIM all showed values many W/m2 higher than the SORCE results (recommended that this be shown too). (Note also that the Willson and Mordvinov study is cited twice in the bibliography, the second one with errors) [Chris Colose, United States]	Taken into account- The TSI composites are now revised and updated, the TIM/SORCE results are included in Fig. 8.13 (previously Fig. 8.12) and the composites are matched to TIM at the year 2003, the initial year of TIM measurements. The references are corrected.
8-1407	8	93				Fig.8.12 caption: Need to update references which are all older than the end of the time series. Also note somewhere recalibration of ACRIM curve (since last IPCC) to match SORCE/TIM. [Joanna Haigh, UK]	Taken into account- The TSI composites are now revised and updated, the TIM/SORCE results are included in Fig. 8.13 (previously Fig. 8.12) and the composites are matched to TIM at the year 2003, the initial year of TIM measurements. The references are corrected.
8-1408	8	93				Fig 8.12: Is the large gap between ACRIM and IRMB/PMOD and potential implications for radiative forcing and climate change covered in sufficient detail in the text? [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. We think that we have covered this point in sufficient detail in the text.
8-1409	8	94				Figugre 8.13 Does this include all up-to-date TSI reconstructions? Is there not one by Lief Svalgaard as well? Maybe it has not been published yet - I have only seen it in a powerpoint presentation. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Fig. 814 (previously Fig. 8.13) has the available up-dated TSI reconstructions. However, as more reconstructions will probably be appearing we shall be up-dating the figure accordingly.
8-1410	8	94				Fig.8.13: Colours in legend don't match curves (Krivova at least) [Joanna Haigh, UK]	Taken into account. We have corrected this
8-1411	8	94				Fig 8.13 Judith Lean's TSI reconstruction that has been recommended for use in the CMIP5 simulations should be included in this figure (Lean JL, calculations of solar irradiance: available at http://sparcsolaris.gfz-potsdam.de/input_data.php). [Gareth S Jones, UK]	Taken into account- We have include the JL calculations in Fig. 8.14 (previously Fig. 8.13). They are the Wang et al. Reconstructions.
8-1412	8	95	1	95	8	Unreadable Figure 8.14 even with high resolution. Nothing to review. [Ruprecht Jaenicke, Germany]	Accepted - new figure with higher resolution is generated.
8-1413	8	95				Graph unreadable [Pieter Aucamp, South Africa]	Noted - see 8-1412
8-1414	8	95				Quality is poor on my printed version. It would be good to include The Vernier et al GRP 2011 study here as well. [Olivier Boucher, France]	Noted - see 8-1412
8-1415	8	95				Wow, these figures are in bad shape [Larry Thomason, United States of America]	Noted - see 8-1412
8-1416	8	96				Symbols on graph (a) unreadable. [Pieter Aucamp, South Africa]	Accepted. An updated figure with better symbols is included.
8-1417	8	96				(confession of ownership of this figure) A better version of this figure can be obtained from Vernier and/or Thomason [Larry Thomason, United States of America]	Accepted. Vernier has made a new updated figure and is a CA.
8-1418	8	97	1	97	7	It will be hard to show so many curves in (b) while still allowing the reader to distinguish among them. As currently plotted, it is impossible to tell which curve is which. [Larry Horowitz, USA]	Accepted: This figure will need to be redesigned
8-1419	8	97	1	97	8	It would be nice to see references for the observations on which these forcing values depend. [John Daniel, USA]	Accepted:Reference will be made to the appropriate table in Ch.2.

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8-1420	8	97	1	97	8	By the next version it would be helpful to make the curves and legends more distinguishable. [John Daniel, USA]	Accepted: This figure will need to be redesigned
8-1421	8	97	4	97	7	Unreadable Figure 8.16 even with high resolution. [Ruprecht Jaenicke, Germany]	Accepted: This figure will need to be redesigned
8-1422	8	97				Graph confusing - especially (b). Use less compounds and use colours that have a greater contrast. Shades of the same colours do not work. [Pieter Aucamp, South Africa]	Accepted: This figure will need to be redesigned
8-1423	8	97				Fig. 8.16 - As discussed in details in the comment about Chapter 2 Page 44, the values plotted in Fig 8.16 consider all observed CO2 increases, irrespective of their anthropogenic (~ 0.5 ppm per year, viz. ~ 0.01 W/m2) or natural (the remaining which essentially follows temperature as seen for the 1998 peak which is a consequence of an exceptional El Niño) origins. Anthropogenic emissions do not change by a factor 4 from one year to the next and are not alone to contribute to data fluctuating from 0.01 to 0.04 W/m2. Only the anthropogenic residual fraction, viz ~ 0.01 W/m2, has to be considered if I have well understood the mission of IPCC. The same is true for other greenhouse gases which show strong fluctuations from year to year like CH4 in Fig. 6.17, N2O in Fig 6.18... [François GERVAIS, France]	Rejected: The well-mixed gases section deals with observed concentrations. However we will add discussion of the variation in sinks.
8-1424	8	98	1	98	6	Clarify what is meant by "short-lived gases" here. What does "total" mean in this context? [Larry Horowitz, USA]	Accepted:Short-lived will be defined in the text.
8-1425	8	98	1	98	7	Consider adding a curve that shows the impact of the strat H2O change not caused by CH4 on Earth's energy balance. Even though it is being considered a feedback, it could be useful to readers. [John Daniel, USA]	Rejected: We have decided to remove the stratospheric water vapour curve.
8-1426	8	98				Somehow the caption should specify that this is about anthropogenic short-lived trace gases, not all short-lived components. [Olivier Boucher, France]	Accepted:This will be changed
8-1427	8	99	0			Figure 8:18 seems to me a repetition of figure 7.12 in chapter 7. But its not totally the same. Probably not needed in chapter 8. Also what is the basis for this figure? [Michael Schulz, Norway]	Taken into account, figure deleted
8-1428	8	99	1	99	2	Figure 8.18: I guess BB stands for biomass burning though not defined in this chapter. If this is true then what is “BB” as component in radiative forcing. [Lokesh Kumar Sahu, India]	Noted, figure deleted
8-1429	8	99	4			Figure 8.18: Name the components in the caption e.g. Black Coebon from Fossil Fuel burning (BC FF); change the colour of the black boxes so we can see the uncertainty ranges for all components; is the RF computed for changes in aerosols since 1750? [Robert Waterland, United States of America]	Noted, figure deleted
8-1430	8	99				Fig 8.18 Ideally this figure would become a subpanel of Fig. 2.25. If not, then reorientat to match format of Fig. 2.25c. [David Fahey, USA]	Rejected, figure deleted since a similar figure will be shown in Chapter 7
8-1431	8	99				will this figure be in Chapter 7? [Piers Forster, UK]	Accepted, figure deleted
8-1432	8	99				Figure 8.18: this is essentially same with Fig.7.12, but uncertainty ranges are slightly different. [Shigeki KOBAYASHI, Japan]	Taken into account, figure deleted
8-1433	8	99				Include both red and blue parts of the BB bar [Henning Rodhe, Sweden]	Noted, figure deleted
8-1434	8	99				Fig 8.18/8.20: We note inconsistency in the way in which forcing/ feedback figures are plotted in the different chapters (i.e., swapping of the X and Y axis) See for example chapters 6 and 7. We suggest chapters coordinate and use a consistent layout. [Thomas Stocker/ WGI TSU, Switzerland]	Noted, Figure 8.18 deleted
8-1435	8	100	1	100	2	Figure 8.19: Uniformity should be mentioned regarding the use of abbreviation example: in this figure its “Soa” while in other figures “SOA”. Another example: one of these “Sul”, “Sulphate”, “sulfate”,” SO4”. This should be done throughout the chapter and also for other species. [Lokesh Kumar Sahu, India]	Taken into account, abbrevations have been made consistent.
8-1436	8	100	1	100	7	Can dotted lines be placed around the black line for the total as well? [Susan Anenberg, USA]	Taken into account, the uncertainty range is shown in a different way and included also for the total
8-1437	8	100	1	100	7	It would be useful to also include aerosol AF in this figure (if possible) [Larry Horowitz, USA]	Rejected, not sufficient information available to be included in the figure

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8-1438	8	100				Leave out the standard deviation. It makes the graph too complex. [Pieter Aucamp, South Africa]	Taken into account, the uncertainty range is shown in a different way.
8-1439	8	101	1	101	11	Clarify that the SO4 AF/RF are for direct forcing only (as currently plotted) [Larry Horowitz, USA]	Accepted - included as suggested
8-1440	8	101				Fig. 8.20 This figure would perhaps be more easily interpreted if it were separated into left/right panels that showed RF and AF, respectively. [David Fahey, USA]	Taken into account - the AF bars has been modified and made consistent with the main bar chart and thus hatch bar for AF.
8-1441	8	101				Figuew 20. Are RF and AF values comparable [Piers Forster, UK]	Taken into account - figure deleted
8-1442	8	102				Can you correct the Skeie study for the difference in reference year? This would make it more of an assessment, and less of the compilation. [Olivier Boucher, France]	Taken into account - more studies from ACCMIP is included in the figure.
8-1443	8	103	4			Figure 8.22: What is the reference year for the change in the TOA SW flux; you should call this the Change in the net downward TOA SW flux. If these pictures show the change in the TOA SW flux since 1750, why is the 1750 picture not zero everywhere? [Robert Waterland, United States of America]	Editorial. Be more explicit in Fig legend
8-1444	8	104	5			Figure 8.23; Agreement with what? [Robert Waterland, United States of America]	Taken into account figure moved to Chapter 1 where more information is given.
8-1445	8	104				Figure 8.23 why put this in this chapter in particular? I think it is more generally applicable. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Taken into account, figure moved to Chapter 1
8-1446	8	104				Fig 23, Isn't this a generic ipcc figure for TS, rather than chapter specific? [Piers Forster, UK]	Taken into account, figure moved to Chapter 1
8-1447	8	105	5	105	5	"confidence" rather than "confindence" [Olivier Boucher, France]	Accepted, text revised as suggested
8-1448	8	105				fig 24 - I like this [Piers Forster, UK]	Noted
8-1449	8	105				Fig 8.24: Why is the 1st assessment report not included? [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - the LOSU was not available in FAR. A sentence in the caption is included: 'LOSU for the RF mechanisms was not available in IPCC (1990).'
8-1450	8	106	1	106	3	I don't find that the third dimension adds much to these plots. If anything, it makes the line plot harder to read. [Larry Horowitz, USA]	Taken into account, time evolution figures made as separate figures
8-1451	8	106	2	106	2	Figure b should first explicitly state that it is radiative forcing from 1980, not 1750, and second, it looks like the sign of volcanic and solar forcing should be swapped in the bar chart: the temporal evolution shows negative solar and positive volcanic, but the bar chart for 2010 makes it look like positive solar and negative volcanic (also, 2000 should be positive volcanic, not positive solar). However, I was also under the impression that recent research suggests volcanic aerosol cooling during the 2000 decade: if this is true, then the temporal evolution chart showing what I presume is a Pinatubo rebound effect is not quite correct... [Marcus Sarofim, USA]	Taken into account, the figure is totally redrawn and with updated data. For solar and volcanic it is explicitely stated the reference year.
8-1452	8	106				These graphs are too complex and confusing. Simplify. [Pieter Aucamp, South Africa]	Taken into account, time evolution figures made as separate figures
8-1453	8	106				I am unsure as to what information the third dimension actually brings to the graphic, except the breakdown for the aerosol RF. [Olivier Boucher, France]	Taken into account, time evolution figures made as separate figures
8-1454	8	106				Fig.8.25a,b This figure is not very effective, particularly the 3-D aspect. It would be more effective if the time lines were shown in an upper panel and the forcing terms show for each time marker arrayed below. The forcing terms would best be shown in a vertical configuration as in Fig. 8.25c. [David Fahey, USA]	Taken into account, time evolution figures made as separate figures
8-1455	8	106				Fig. 8.25a,b The display of a volcanic term is confusing and inconsistent with Fig. 8.25c and Table 8.9. If it is distinguished as natural term in a separate bar or a component of a bar in 8.25a,b, then it would seem important to do this in 8.25c. Is the volcanic term only direct? I didn't see this noted in the text. [David	Taken into account, the volcanic RF is shown in time evolution figures (now as separate figures) and it is noted that it is not taken into account in the main bar

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						Fahey, USA]	chart figure since this is for 1750-2010.
8-1456	8	106				fig 25 - panel a) is cool but maybe overly complicated. I generally like the 4 frames though. Again, Andrews anf Forster showed an appreciable AF for CO2 - it also has a physilogical forcing - so can you really assume AF and RF are the same for the LLGHGs? [Piers Forster, UK]	Taken into account, time evolution figures made as separate figures. AF and RF for CO2 is now different.
8-1457	8	106				Panels (a) and (b) of Fig 8.25 are really hard to figure out. I question whether the 3-D presentation is really all that useful. [JOHN OGREN, USA]	Taken into account, time evolution figures made as separate figures
8-1458	8	106				Fig. 8.25, a) and b) are too complicated [Henning Rodhe, Sweden]	Taken into account, time evolution figures made as separate figures
8-1459	8	106				Much too complicated figure [Henning Rodhe, Sweden]	Taken into account, time evolution figures made as separate figures
8-1460	8	107				Nice plot [Olivier Boucher, France]	Noted
8-1461	8	107				Fig. 8.25c. This is the leading figure in this chapter and one that will be shown extensively by users of AR5 (based on AR4 usage experience). As such it should be as clear and as useful as possible. First, I recommend that BC forcing be shown explicitly. As the 2nd or 3rd largest anthropogenic agent, BC is conspicuous by its absence (as it was in AR4). Further, BC is one of 3 leading SLCFs (+ O3 and CH4) that have been a substantial focus of scientific and policy efforts since AR4 because of potential mitigation actions. Hence, it would be good if BC could be 'found' on the master forcing 'bar chart' as are the other two SLCF agents. Graphically, the direct bar could be split into a left hand blue component and a right hand red component on the same line and labelled 'non-BC' and 'BC'. The AF bar could remain as a single bar for simplicity. Second, this is the first display of AF in a master bar chart in an IPCC assessment. As noted on p5 ln 45 'AF provides a good indication of the eventual climate response.', implying better than RF. Hence, AF is in an important sense more important than RF in evaluating the anthropogenic influence on climate. As a consequence, it is perhaps more consistent graphically to make the AF bars solid and the corresponding RF bars hatched. Similarly, in 8.25d, RF lines should become dashed and AF lines solid. [David Fahey, USA]	Taken into account - the main summary figures have been strongly modified and reorganized. BC is not included in the bar chart Figure 8.25c so to some extent rejected is the response to this comment. However, the FOD Figures 8.25c, 8.25d and 8.27 have been merged to a multi-panel figure and BC will be explicitely shown in the new version of the FOD Figure 8.27. Since RF is shown in FOD Figure 8.27 we continue to show RF in solid bars.
8-1462	8	107				Fig. 8.25 - Same as comment about Fig. 8.16 [François GERVAIS, France]	Taken into account - text is included in section 8.1 with a further description on how RF is calculated based on abundance changes.
8-1463	8	107				Figure 8.25: Part b) of this figure is showing the time evolution of radiative forcing from 1980 to 2010 but it isn't clear from the figure caption that differences in RF relative to that in 1980 are being shown. [Fiona O'Connor, United Kingdom of Great Britain & Northern Ireland]	Taken into account, the figure is totally redrawn and with updated data. For solar and volcanic it is explicitely stated the reference year.
8-1464	8	107				Fig 8.25: Suggest to split panels (a) and (b) into two figures. Convert to several 2D representations, thereby making interpretation easier. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account, time evolution figures made as separate figures
8-1465	8	107				Figs 8.25 and 8.27: Ensure consistency between the values given in these two figures. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account, new consistent figures for 8.25 and 8.27 are generated.
8-1466	8	108	1			I am in mixed minds about whether this figure is needed. Another idea is to use a log scale (or log-log) and take the time longer. This will strengthen the point you wanted to make in the text [Glen Peters, Norway]	Rejected. We think it is needed to show the long term behaviour of the gases. A log scale is too complicated. The figure is moved to Supplementary Material
8-1467	8	108				Put legend in box to make it clearer. [Pieter Aucamp, South Africa]	Editorial. Figure is improved (and has been moved to Supplementary Material)
8-1468	8	108				Use colours that have a greater contrast. [Pieter Aucamp, South Africa]	Editorial. Figure is improved (and has been moved to Supplementary Material)
8-1469	8	108				Do not use the E notation in the graphs. Using a number of zeros makes it clearer. [Pieter Aucamp, South Africa]	Taken into account.

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8-1470	8	108				"Temperature" rather than "Temparature" [Olivier Boucher, France]	Editorial. Corrected
8-1471	8	108				Make a separate figure for CO2 to emphasize its long-term behaviour [Henning Rodhe, Sweden]	Taken into account in the text. We have added references to chapter 6 where the long-term behaviour or CO2 is illustrated and discussed.
8-1472	8	108				Figure 8-26. Choose different line types (dotted, dashed, etc.), add arrows connecting species with lines, or add text to figure to avoid confusion. [Timothy Wallington, USA]	Editorial. Figure is improved (and has been moved to Supplementary Material)
8-1473	8	109	1			CO2 is included in CH4, CO, and NMVOC, but was CO2 reduced correspondingly in the CO2 bar? It may be woth showing somehow, how much is reallocated? [Glen Peters, Norway]	Taken into account - a new figure is generated and the CO2 contribution from CH4, CO, and NMVOC is reduced correspondingly in the CO2 bar.
8-1474	8	109	1			It might be worth spelling out what the T and the S represent. [Glen Peters, Norway]	Taken into account -T and S are removed from the figure.
8-1475	8	109	4	109	6	You should show uncertainties and also estimates of the Level of Scientific Understanding as in the preevious reports [VINCENT GRAY, NEW ZEALAND]	Taken into account - The useful suggestion to add uncertainties is included, but LOSU nor confidence level is included. This can be find in the FOD Table 8.8 that will be located close to this figure in the printed report.
8-1476	8	109	4			In the Halocarbons bar can you split the CFCs, HCFCs, and halon contributions out in the same way you are showing CH4, O3(T), H2O(S), CO2 in the CH4 bar [Robert Waterland, United States of America]	Taken into account - the suggested split in the halocarbon RF is included
8-1477	8	109				You may want to add a caveat about co-emitted species in the caption. [Olivier Boucher, France]	Rejected - discussion included in the text.
8-1478	8	109				Fig. 8.27 - Same as comment about Fig. 8.16 [François GERVAIS, France]	Taken into account - see comment 1462
8-1479	8	109				Figure 8.27: Please include a table with the numeric values for this (and Figure 8.26), for easy access, since it is useful to be able to calculate sub-parts of the forcing… [Marcus Sarofim, USA]	Taken into account - numbers included in a supplementary.
8-1480	8	110	1			Is this GTP-10 or GTP-20 with a typo? If GTP-10, why? [Glen Peters, Norway]	Noted. No, not a typo. The time horizons are selected as examples
8-1481	8	110				Figure 8-28. Assessing impacts on a 10 year time scale (GTP-10) is not meaningful in the context of discussions of climate change. [Timothy Wallington, USA]	Noted. We think a time horizon of 10 years also conveys important information about the behaviour of the various components.
8-1482	8	111				Put legend in box to make it clearer. [Pieter Aucamp, South Africa]	Taken into account. This figure is now replaced with a new one (which has legends in a box)
8-1483	8	111				Use colours that have a greater contrast. [Pieter Aucamp, South Africa]	Taken into account. This figure is now replaced with a new one.
8-1484	8	111				Fig. 8.29 - Same as comment about Fig. 8.16 [François GERVAIS, France]	Noted. Fig 8.29 shows contributions to warming from various anthropogenic emissions
8-1485	8	111				Make the labeling of the graphs clearer [Henning Rodhe, Sweden]	Taken into account. This figure is now replaced with a new one.
8-1486	8	111				Figure 8-29. Assessing impacts of emissions on time scales less than 20 years is not meaningful in the context of discussions of climate change. I suggest making the 0-20 year portion of the curves dotted and adding text to clarify the fact that discussions of impacts on such short time scales are not consistent with the accepted definition of climate change but they are added for transparency. This is not to say that short-term impacts are not important. The impact of short term effects is captured in the data for time scales longer than 20 years in the figure. [Timothy Wallington, USA]	Rejected. We think time horizons shorter than 20 years convey important information about the behaviour of the various components.
8-1487	8	111				Figs 8.29, 8.31, and 8.32: Unclear why these figures are expressed as temperature change as opposed to	Taken into account. Figures are improved and we

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						radiative forcing. Consider to reduce number of colors and use variety of dashing/stippling instead. [Thomas Stocker/ WGI TSU, Switzerland]	have written that the AGTP concept can be used to study the effects of the various components over time.
8-1488	8	112	1	112	6	It probably makes more sense to plot the forcing efficiency as (W/m2)/(kg/y), so that you are comparing the effect of equal rates of mass emissions across different regions. [Larry Horowitz, USA]	The figure has been removed.
8-1489	8	112				Figure 8.30: It is not very obvious that the SO2 forcing appears in the stratocumulus low cloud regions. I also think that the word radiative forcing efficiency is misleading here, since that term has been used to express the local relation between burden and forcing, or the global relation of burden to forcing. Here it seems to me that the local emission is related to global forcing. Maybe that can be expressed differently? [Michael Schulz, Norway]	The figure has been removed.
8-1490	8	113	1	113	2	Fig. 8.31(a) It is difficult to distinguish the separate colors – maybe use different line types as well. [Helen Worden, USA]	Taken into account. This figure is now replaced with a new one.
8-1491	8	113	2	113	9	Fig. 8.31(b) make a zero reference line in the bottom panel (b) [Helen Worden, USA]	Taken into account. This figure is now replaced with a new one.
8-1492	8	113				Use colours that have a greater contrast. [Pieter Aucamp, South Africa]	Taken into account. This figure is now replaced with a new one.
8-1493	8	113				Do not use the E notation in the graph (b). Using a number of zeros makes it clearer as in (a). [Pieter Aucamp, South Africa]	Editorial. Taken into account.
8-1494	8	113				Use the same scale notation for both graphs. [Pieter Aucamp, South Africa]	Taken into account. These figures are now replaced with new ones.
8-1495	8	113				Figure 8.31. Extremely useful figure. I think it should also include emissions from forestry (deforestation / afforestation). Ideally would also be good to include albedo, but the information may not be available for that. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted. Thanks. The figure has been replaced by a figure from a recently submitted paper. Available metrics and emission data were used, and we could not include the suggested factors.
8-1496	8	113				Fig. 8.31 - Same as comment about Fig. 8.16 [François GERVAIS, France]	Noted. Fig 8.31 shows anthropogenic contributions
8-1497	8	113				Figure 8-31. Same comment as for Figure 8-29. In panel (b), why focus on the temperature change in the short-term (t = 20 years)? It would be more balanced to give temperature changes in the short- medium- and long-term (i.e., perhaps 20, 100, 500 years). If space constraints allow just one figure I suggest a medium-term (say 100 year) time point would be more appropriate. [Timothy Wallington, USA]	Rejected. We think time horizons shorter than 20 years convey important information about the behaviour of the various components.
8-1498	8	114	1	114	6	Fig. 8.32 It is difficult to distinguish the separate colors – maybe use different line types as well. [Helen Worden, USA]	Taken into account. This figure is now replaced with a new one.
8-1499	8	114	1	114	7	Make curves more distinct from each other. [John Daniel, USA]	Taken into account. This figure is now replaced with a new one.
8-1500	8	114	1	114	7	Define "household FF and BF". [John Daniel, USA]	Taken into account. This figure is now replaced with a new one (which has different sector names)
8-1501	8	114			16	I would find it interesting if you could state in the text how large the total temperature change will be after 100 years (or some other time horizon) of constant "current" emissions. [Borgar Aamaas, Norway]	Rejected. The total effect (of the particlular "scenario") is not the main focus. It is rather to show the relative contributions and different temporal behaviours.
8-1502	8	114				Use colours that have a greater contrast. [Pieter Aucamp, South Africa]	Taken into account. This figure is now replaced with a new one.
8-1503	8	114				Put legend in box to make it clearer. [Pieter Aucamp, South Africa]	Taken into account. This figure is now replaced with a new one (which has legends in a box)
8-1504	8	114				Maybe it will help to put the legend on the line. [Pieter Aucamp, South Africa]	Taken into account. This figure is now replaced with a new one.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8-1505	8	114				Colors are somewhat difficult to distinguish [Olivier Boucher, France]	Taken into account. This figure is now replaced with a new one.
8-1506	8	114				Fig. 8.32 - Same as comment about Fig. 8.16 [François GERVAIS, France]	Noted. Fig 8.32 shows anthropogenic contributions
8-1507	8	114				Figure 8-32. Same comment as for Figure 8-29. Also, some comment in the text and figure caption is needed regarding whether it is reasonable to assume constant emissions for some sectors such as road transport where it is well established that the emissions of criteria air pollutants are very likely to decline rapidly over the coming decades (see Wallington et al., Science, 327, 268, 2010). [Timothy Wallington, USA]	Rejected. We think time horizons shorter than 20 years convey important information about the behaviour of the various components. We agree that constant emissions is not a likely scenario, but this is chosen to illustrate the behaviour of the various components.
8-1508	8	115	4			The values given for the global area-weighted means given in the upper right for the three lower plots in the right column seem inconsistent with the scales shown. [Robert Waterland, United States of America]	Taken into account - new figures with correct global mean values have been included.
8-1509	8	115				Figure 8.33 Is it possible to also add the AeroCom DRF to this panel? These comprise more and different models, and it is used for the aerosol DRF estimate. [Michael Schulz, Norway]	Rejected. This will appear in Ch 7
8-1510	8	117	1	117	7	Also include a panel for aerosol AF in this figure. [Larry Horowitz, USA]	Noted. This will be added before the SOD
8-1511	8	117				Use colours that have a greater contrast. [Pieter Aucamp, South Africa]	Noted. This will be added before the SOD
8-1512	8	117				Put legend in box to make it clearer. [Pieter Aucamp, South Africa]	Noted. This will be added before the SOD
8-1513	8	118				I miss a similar map for forcing by tropospheric ozone [Henning Rodhe, Sweden]	Noted. This will be added before the SOD
8-1514	8	119	1	119	5	NOx can also be targeted for PM2.5 controls. [Susan Anenberg, USA]	Taken into account: a dashed arrow is included
8-1515	8	119				Figure FAQ 8-2. The figure indicates (mistakenly) that controls of methane emissions target local ozone formation. [Timothy Wallington, USA]	Rejected: CH4 control has first of all a global impact but this also affects ozone at the regional ozone level (West et al. paper).
8-1516	8	120	3	120	3	Please include the newer references: Kirkevåg, A., T. Iversen, J. E. Kristjánsson, , Ø. Seland, and J. B. Debernard. (2008) On the additivity of climate response to anthropogenic aerosols and CO2, and the enhancement of future global warming by carbonaceous aerosols. Tellus 60A, 513-427. DOI: 10.1111/j.1600-0870.2008.00308.x and Kirkevåg, A., T. Iversen, Ø. Seland, J. B. Debernard, J. E. Kristjánsson, T. Storelvmo (2008) Aerosol-cloud-climate interactions in the climate model CAM-Oslo. Tellus 60A, 492-512. DOI: 10.1111/j.1600-0870.2008.00313.x [Trond Iversen, United Kingdom of Great Britain & Northern Ireland]	Rejected, no specific section is requested for incorporation of these references. This is an assessment and not a review so only the most important references since AR4 are included.