Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8-1	8	0	1			Criticism of Chapter 8 [Jack Barrett, United Kingdom]	Noted
8-2	8	0	1			A general criticism is the use of ppm as the unit of concentration of greenhouse gases. It is fairly meaningless unless modified to ppmv, parts per million by volume, in which case the concentration is equivalent to the mole fraction of the gas; its concentration in terms of molecules of gas per molecules of the atmosphere in unit volume. The use of ppm alone could be envisaged as meaning ppmv or ppmm, the latter parts per million by mass being a different number to that expressed as ppmv. Example 390 ppmv of CO2 is equivalent to 390 × 44/28.96 = 592.5 ppmm. The relative molar mass of CO2 is 44 and the mean value for the atmosphere is 28.96. With concentrations measured as ppmv the mass of the molecule is incorporated and the concentrations of two or more gases can be compared in molecular terms without any further calculations. The same criticism applies to ppb which should be ppbv to obviate similar confusion. [Jack Barrett, United Kingdom]	Rejected, the standard unit for GHG is ppm.
8-3	8	0	1			Emission levels [Jack Barrett, United Kingdom]	Noted
8-4	8	0	1			Nowhere in the document have I been able to find a discussion of emission level of any greenhouse gas and this seems to be a serious omission. [Jack Barrett, United Kingdom]	Rejected, this chapter is an assessment of changes in the understanding of forcing since AR4 and there has been no changes related to this comment since AR4.
8-5	8	0	1			There are general treatments in textbooks of mean emission levels and how they might change if greenhouse gas concentrations change. I find these discussions unhelpful and wish to propose a different strategy that will be understandable and meaningful. [Jack Barrett, United Kingdom]	Noted.
8-6	8	0	1			It is important to explain that the concept of emission level, i.e., the altitude where emission to space is likely, applies strictly and separately to each and every frequency at which any given greenhouse gas absorbs terrestrial radiation. There is no one level for any gas. This should be obvious from the absorption spectrum of the gas with its very varied absorption coefficient as a function of frequency. [Jack Barrett, United Kingdom]	Rejected, see comment 8-4.
8-7	8	0	1			The best basis for defining an emission level is the optical path or optical density of the atmosphere at any given frequency with the zero taken to be the top of the atmosphere, wherever that might be. In practice 70 km is satisfactory, but it does not matter too much as there is very little absorption by any molecules at higher altitudes. [Jack Barrett, United Kingdom]	Rejected, see comment 8-4.
8-8	8	0	1			One basis for the definition of the emission level is to adopt the critical optical density for radiation of a given frequency escaping to space as 0.67. That is, if the optical density, measured from the top of the atmosphere to the emission level is 0.67 for radiation of a given frequency travelling from the emission level to space in all directions, the transmission of the atmosphere would be 1/e0.67 = 0.51. There would be a 51% probability of such photons reaching space and a 49% probability that they would be absorbed before reaching space. [Jack Barrett, United Kingdom]	Rejected, see comment 8-4.
8-9	8	0	1			With such a definition there could follow two cases of the greenhouse effect. [Jack Barrett, United Kingdom]	Rejected, see comment 8-4.
8-10	8	0	1			Case 1 would be if the emission level was somewhere in the troposphere. An increase of greenhouse gas concentration at the given frequency would increase the emission level to a higher altitude where the atmosphere would be cooler because of the negative value of the lapse rate. Consequently the emission intensity would be lower at the higher concentration and the system would have to warm up to restore radiation balance with space. [Jack Barrett, United Kingdom]	Rejected, see comment 8-4.
8-11	8	0	1			Case 2 would be if the emission level was somewhere in the stratosphere. An increase of greenhouse gas concentration at the given frequency would again increase the emission level to a higher altitude, but the atmosphere would be warmer because of the positive value of the lapse rate. Consequently the emission intensity would be higher at the higher concentration and the system would have to cool down to restore radiation balance with space. [Jack Barrett, United Kingdom]	Rejected, see comment 8-4.
8-12	8	0	1			I suggest this consideration would be very helpful for people attempting to understand the physics and would be impossible to be ignored by extreme sceptics! [Jack Barrett, United Kingdom]	Rejected, see comment 8-4.
8-13	8	0	1			Consistency in assessment numbers: Because chapter assessments continue to be refined, please check	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. [Thomas Stocker/ WGI TSU, Switzerland]	
8-14	8	0	2			Treatment of Uncertainty: please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted, we follow the IPCC guidance note on uncertainties.
8-15	8	0	3			Format of Executive Summary (ES): As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted, our SOD ES was on the agreed format, but we have shortened the ES and several of the paragraphs.
8-16	8	0	4			Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
8-17	8	0	5			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted, full references to AR4 chapters are included.
8-18	8	0	6			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
8-19	8	0	7			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
8-20	8	0	8			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II - Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted, all our main results will be included in Annex II.
8-21	8	0				The correct citation is: WMO (World Meteorological Organization), Scientific Assessment of Ozone Depletion: 2010, Global Ozone Research and Monitoring Project-Report No. 52, 516 pp., Geneva, Switzerland, 2011. [Pieter Aucamp, South Africa]	Accepted, reference corrected.
8-22	8	0				Chapter 8 should contain at least a comment on the validity of the concept of using a radiative forcing index (RFI) to upscale the climate effect of aircraft emissions. RFI values in the range of 2.5-3 often used in the public discussion to calculate CO2 emissions of aircraft travel (e.g. in the context of emission trading schemes). However, the whole concept has been criticized as unfair when only applied to aviation: Piers M. de F. Forster, Keith P. Shine, Nicola Stuber, It is premature to include non-CO2 effects of aviation in emission trading schemes, Atmospheric Environment, Volume 40, Issue 6, February 2006, Pages 1117-1121, ISSN 1352-2310, 10.1016/j.atmosenv.2005.11.005. [Dietrich Feist, Germany]	Rejected, it is beyond the scope of the chapter to go into detail about the RFI for each sector.The chapter does discuss the effect of emissions by sector to some degree in section 8.7 however.
8-23	8	0				I have read the whole chapter carefullly. It is concisely written and rather technical with detailed and somewhat complex figures. A very impressive job of conveying so much information in a short space. I think the level of technical detail is not a problem as policy makers understand RF and GWP etc. quite well. I don't think it needs changing too much. I would especially resist adding detailed explaination and a whole lot more	Noted.

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						references - as the current levels are consistent across sections and, in my view, about right [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	
8-24	8	0				In spite of my explicit recommendation in ROFOD, the direct measurements of the earth albedo performed at the Big Bear Solar Observatory by Pallé, E., P.R. Goode, P. Montanes-Rodroguez, J. Geophys. Res. 114 (2008) 1029, and its time dependence is still ignored in SOD. [François Gervais, France]	Rejected, not sufficiently scientifc evidence how this information could be included to the various forcing mechanisms.
8-25	8	0				Over all this is a solid and well-written chapter. The text makes clear the developments in the science since AR4. I learnt a lot from reading the chapter. [Nathan Gillett, Canada]	Noted
8-26	8	0				Many uncertainty ranges are quoted in the chapter, including in the ES, but I don't think it says anywhere what the uncertainty ranges correspond to (I think 5-95%). This should be stated up front in the ES. [Nathan Gillett, Canada]	Taken into account, it was already given in footnote 3 in ES.
8-27	8	0				In some parts of the chapter there is a tendency to quote radiative forcing ranges without a clear indication of where the estimates come from. Are they based on expert judgment? Are they based on results discussed elsewhere in the chapter? Are they based on results in other chapters (chapter 7)? For example pg 26, ln 31-32, and pg 25, ln 6-7, If the restults are summaries of the following sections, then they should come after the section, with some indication that they are a synthesis of the results just presented. Otherwise give references to sources, or to subsections of the report. [Nathan Gillett, Canada]	Taken into account, it is stated more clearly in start of each section how the numbers are derived.
8-28	8	0				general comment: There is no mention of NOAA's Annual Greenhouse Gas Index (AGGI). This is a metric included in the annual State of the Climate reports produced by the Bulletin of the American Meteorological Society. Is there a reason why it is not included or even mentioned here? [European Union]	Rejected, not of sufficiently importance to be used in this chapter.
8-29	8	0				There are more figures using the GTP than the GWP. This shows the bias toward the GTP in this chapter. Corresponding figures for GWP should be presented. [Government of Germany]	Taken into account, by a more balance between GWP and GTP with regard to figures and text.
8-30	8	0				One concern with the current chapter is that key messages tend to get lost in the details. There are many important facts and new scientific insights on climate change, yet the role that humans play in climate change does not come out directly and clearly – it is often implicit, as described through activities and sectors. However, the role of humans in developing fossil fuel-intensive energy and transport systems, land use changes, etc. seems to be taken for granted. Direct reference to individual and collective responsibility (humans and certain societies as the drivers of climate change) is missing, although there is an extensive scientific literature on this. This leaves the audience without a clear message on how humans and societies influence changes in average climate and in extremes. [Government of NORWAY]	Rejected, beyond the scope of forcing chapter in general. Some linkage to human activities is provided in the analysis of the forcing due to emissions by sector in section 8.7, however.
8-31	8	0				The role of humans (and society) as drivers of change does not come out very strongly in this chapter. There are a number of opportunities to integrate humans and society in this chapter. Sections 8,5.1, 8,5,2 and 8,5,3 would benefit from broadening the perspectives, and this would in turn also give context to the main conclusions presented in the executive summary. [Government of NORWAY]	See comment 8-30.
8-32	8	0				This chapter is mainly focused on CMIP3 evaluation studies. Nothing is said about CMIP5 evaluation in some subsections. This seriously difficults comparisons. [Government of Spain]	Rejected, the CMIP3 was not mentioned in the text in SOD, while CMIP5 results are used extensively.
8-33	8	0				The Likelihood Table (Table 1.1) and Confidence figure (1.12) should be repeated in the SPM, TS and each Chapter and the terminology should be applied consistently. As an alternative to repeating the complete table/figure the material should be restated briefly in the SPM, TS, and each chapter. [Government of United States of America]	Accepted, included in footnotes in ES.
8-34	8	0				There's a lot overlap between chapters 7 and 8. The authors shold consider a concerted cross-chapter coordination effort. [Government of United States of America]	Taken into account, discussion between Chapter 7 and 8 on the balance between overlap and that each chapter can be read as a stand alone chapter have been made. Section 8.3.4 is reduced. Note that Chapter 7 has no information on the time evolution and this needs to description in Chapter 8.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8-35	8	0				rates of change in radiative forcing should also be put in the context of the paleo record (e.g. Joos and Spahni, PNAS, 105, 2008) [Fortunat Joos, Switzerland]	Rejected, beyond the scope of this chapter and belong to Chapter 5.
8-36	8	0				Having read this chapter from beginning to end I have to say that it is for the most part based on unvalidated climate models that do not encompass all climate forces with 100% accuracy. There's no way that the models could be accurate when the level of understanding of so many forces is far from satisfactory. Climate models failed to predict the absence of warming over the last 16 years (Jan 1997 to July 2012 .. and maybe longer) so they logically have no credibility when it comes to predicting anything. Incredibly the authors of this chapter seem to think that models produce evidence, but that's a naive. Models only make predictions based on the input data they are given and the algorithms by which that data is processed. The primary driver of temperature since at least 1960 has been the ENSO, and the warming that began in 1977 and ended about 1996 is due to the dominance of ENSO conditions on the El Nino side of absolutely neutral (SOI = zero). McLean et al (2009) showed the sustained relationship since 1960 and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures" - discussed the Pacific Climate Shift that started the move to higher temperatures. The emphasis on climate models in this and other chapters are therefore without merit. [John McLean, Australia]	Rejected, this chapter is about forcing and estimated based on simulated or observed abundance changes and not about attribution of the recent temperature record.
8-37	8	0				I read not only the parts on solar forcing which is my main expertise but the entire chapter and I appreciate a lot the dicussion on metrics as it makes the link with the decision support aspect of the report. [Christian Muller, Belgium]	Noted.
8-38	8	0				Annex II. Chapter 8 has a wealth of data that is plotted or discussed that is now included in Annex II tables. Some are listed below. Please check on overlap and source of info for the tables. Please also refer forward to the Annex II tables in the figure captions or text where appropriate. Table AII.1.1: Historical abundances of the Kyoto greenhouse gases Table AII.1.2: Historical climate forcing as RF or AF (W m–2) Table AII.2.16: Anthropogenic CO emissions (Tg yr–1) Table AII.2.17: Anthropogenic NMVOC emissions (Tg yr–1) Table AII.2.18: Anthropogenic NOX emissions (TgN yr–1) Table AII.2.19: Anthropogenic NH3 emissions (TgN yr–1) Table AII.2.20: Anthropogenic SOX emissions (TgS yr–1) Table AII.2.21: Anthropogenic OC aerosols emissions (Tg yr–1) Table AII.2.22: Anthropogenic BC aerosols emissions (Tg yr–1) Table AII.4.1: CO2 abundance (ppm) Table AII.4.2: CH4 abundance (ppb) Table AII.4.3: N2O abundance (ppb) Table AII.4.16: Montreal Protocol greenhouse gas abundances (ppt) Table AII.5.1: Stratospheric O3 column changes (DU) Table AII.5.2: Tropospheric O3 column changes (DU) Table AII.5.3: Total aerosol optical depth Table AII.5.4: Absorbing aerosol optical depth Table AII.5.5: Sulphate aerosol loading (TgS) Table AII.5.6: OC loading (Tg) Table AII.5.7: BC loading (Tg) Table AII.5.8: CH4 atmospheric lifetime (yr) against loss by tropospheric OH Table AII.5.9: N2O atmospheric lifetime (yr) Table AII.6.1: RF from CO2 (W m–2) Table AII.6.2: RF from CH4 (W m–2) Table AII.6.3: RF from N2O (W m–2) Table AII.6.4: RF from all HFCs (W m–2) Table AII.6.5: RF from all PFCs plus SF6 (W m–2) Table AII.6.6: RF from Montreal Protocol greenhouse gases (W m–2) (single scenario) Table AII.6.7: RF from stratospheric O3 changes since 1850 (W m–2) Table AII.6.8: RF from tropospheric O3 changes since 1850 (W m–2) Table AII.6.9: Anthropogenic RF and AF components relative to 1850 (W m–2) from ACCMIP	Taken into account, the link with data and text with Annex II is highly improved.

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						Table AII.6.10: Total (anthropogenic plus natural) adjusted forcing relative to 1850 (W m–2) from CMIP5 [Michael Prather, United States of America]	
8-39	8	0				The RF estimates pick final years without volcanoes and thus present a biased view with regard to the RF impact of volcanoes. It would be good to look at the RF averaged over the 1990s and the 2000s, this avoids the singularity of picking a biased year (likewise it would not be good to pick 1992 for RF). [Michael Prather, United States of America]	Taken into account by using several approaches with regard to time periods.
8-40	8	0				The current draft is excellent. Relatively minor comments below. [Robert Waterland, United States of America]	Noted, thanks.
8-41	8	1	1	124	1	It is a bit unclear to me where the uncertainties in the forcing calculations are discussed. For aerosol, this is touched on in chapter 7 but it would be good if either of the chapters could reflect recent advances on uncertainties in radiative transfer (Randles et al., 2012) and host model parameters (Stier et al., 2012). [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Taken into account by describing clearer how the uncertainties are derived. Some of the uncertainties are described in the Supplementary Material and more reference to Chapter 7 is made.
8-42	8	1		124		Please decide in the entire text: aerosol or aerosols. This should be uniform throughout the text [Government of Poland]	taken into account by using aerosols
8-43	8	1		124		The use of AF (adjusted forcing) as an acronym of undesirable, since it is to be confused with the adjusted forcing concept in Ar4. One should use RFP (radiative flux perturbation) or similar (such as Radiative Flux Change) [M Daniel Schwarzkopf, United States of America]	Taken into account by using ERF (effective radiative forcing)
8-44	8	1		124		Overall this chapter is well written, and asseses the importance of new metrics such GTP and adjusted radiative forcing. There are sevral typos in the chapter. Figure 8.9 y axis should be 0.0 and not -0.0. Please correct the same. [Ramachandran Srikanthan, India]	Noted and axis in Figure 8.9 is corrected.
8-45	8	1		200		15. This paragraph refers to the entire Chapter 8. Chapter 8 reviews some of the published information on the topic "Anthropogenic and Natural Radiative Forcing". However, the motivation for the reviewed research effort and the logic behind it is more often fraudulent than not, as the respective research frequently follows the pseudo-scientific reasoning that "more corroborating evidence produces a stronger case for the AGW hypothesis". In fact, nothing can be further from the truth, as shown in my Paragraph 3. Indeed, no amount of corroborating evidence can prove a hypothesis, while a single piece of contradictory evidence is sufficient to reject a hypothesis. In effect, the only (dubiously) useful result of this research effort is the "general progress of science", resulting from wasteful usage of public money on climate studies, where no real problem requiring study may be found. Even the PhD degrees earned as a result of such research are of dubious (in the very least) value, as we are producing more pseudo-scientists certified as scientists, in addition to the already existing pseudo-scientists. Research based on the AGW hypothesis, known to be wrong, may provide no valid scientific results, as its conclusions are already known before the research even began - these conclusions being "AGW is happening, and we are to blame for it". Additionally, the data interpretation in the publications is frequently done based on the same climate models, which are demonstrably wrong (as shown in my Paragraphs 2 to 8), and therefore constitutes a fraud. [Igor Khmelinskii, Portugal]	Rejected, no scientific evidence to support changes suggested by the reviewer.
8-46	8	3	1	8	6	I thought this was the clearest written ES of all. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Noted, thanks.
8-47	8	3	3	3	4	The concept of Adjusted Forcing is introduced in this chapter. The opening explanation is different to that used in the executive summary of chapter 7 and chapter 1. AF is a complex concept - a consistent definition throughout would be useful. A further question is what value is there for policy makers in introducing this new concept particularly when at p.3 line 19-20 it states 'The total AF value is weaker than total RF and has greater uncertainty due to its inclusion of additional impacts on clouds'? while it is an interesting scientific concept does it actually add value to decision-makers understanding? if it does, then when and how policy-makers should use AF should be clearly identified. [Government of Australia]	Taken into account by homogeneous definitions in Chapter 1, 7, 8. A Box 8.1 is included to emphasis stronger the benefit of ERF.
8-48	8	3	3		14	It would be useful to note that RF already included some aspects of rapid adjustment (stratospheric temperature & ozone?) and that AF carries this concept forward to tropospheric non-climate responses. In terms of calculating the RF response from chemical pulses like CH4, we have always included the	Taken into account by adding further description in section 8.1

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						"adjustment" in atmospheric composition that involves water and ozone - have not we? [Michael Prather, United States of America]	
8-49	8	3	4	3	8	Here it is stated that an essential knowledge of Radiative Forcing is necessary. Please state that an explanation of Radiative Forcing will come up in the text. Please place an appropriate reference to page 7 line18 - 32 here (The Radiative Forcing Concept & Defining Radiative Forcing). [Government of Germany]	Taken into account, by reference to section 8.1 and a new box.
8-50	8	3	4			I don't think RF "is essentail" or this sentence "is essential"! [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Taken into account by removing the sentence.
8-51	8	3	10	3	10	Diagnosis of AF by the method of Gregory et al (2004) allows sea ice and SST to change as well, although one would expect such changes to be small. The essential distinction, I think, between AF and RF is that of timescale, as stated at line 13, but it is not precisely defined. I would say that the timescale of adjustment is much less than a year e.g. a month or two, long enough for stratospheric adjustment, and also allowing some changes in the upper ocean. Footnote 2 allows some surface conditions to change ("all or portion of surface conditions unchanged"), and I agree with that. In footnotes 1 and 2, I would suggest "net downward radiative flux" instead of "net irradiance". "Irradiance" may be a technically correct term, but is more often used to refer to incoming solar radiation. [Jonathan Gregory, United Kingdom]	Taken into account, we have revised the description of ERF to apply genearlly to the two primary methods in use in the literature. This acknowledges that all the surface conditions can respond in the 'Gregory' method, though surface temperature is effectively fixed as changes are removed by the regression, while SST and sea-ice are fixed in the 'fixed-SST' method. Virtually all the available ERF analyses from the new CMIP5/ACCMIP studies assessed here used the fixed-SST method, so that must be emphasized here.
8-52	8	3	10			To clarify the text, the authors may want to revise it to read: "ocean surface temperatures" (or some other measurable quantity) and not just "the ocean," because the ocean is not a variable. [Government of United States of America]	Accepted, revised.
8-53	8	3	10			"all variables" should be "all physical variables"? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted
8-54	8	3	10			"all variables except the ocean and sea ice". I thought the surface temperature, at least global mean, was fixed in the definition of adjusted forcing; if not, then you are admitting feedback into the adjusted forcing. [Stephen E Schwartz, United States of America]	Noted. This issue is discussed in detail in 8.1.1.2.
8-55	8	3	12	3	12	Not all changes to clouds by aerosols are rapid adjustements, there may also be changes due to standard feedback mechanisms [Terje Berntsen, Norway]	Agreed. This was meant to refer to the cloud responses discussed in the previous paragraph. Revised 'The' to 'These' to clarify this.
8-56	8	3	12	3	13	The sentence starting "The changes to clouds … forcing" can be deleted without much loss for the paragraph. [Olivier Boucher, France]	Rejected. While there is no strict separation between the timescales for the responses and climate feedbacks, we feel its useful for the reader to know that for the large adjustments that occur via clouds these do in fact take place much more rapidly. See also comment 51 from J Gregory about importance of timescale.
8-57	8	3	13	3	13	See comment on page 3 line 10. [Jonathan Gregory, United Kingdom]	Taken into account, see response to 51.
8-58	8	3	14		14	Can you note whether the use of AF instead of instant RF gives improved consistency in the efficacy or the climate sensitivity to a given forcing? [Michael Prather, United States of America]	Rejected, with introduction of ERF the efficacy is of minor importance and this is only briefly discussed in section 8.1. It is not of sufficiently importance to be mentioned in ES.
8-59	8	3	16	3	42	These lines contain an lot of acronyms and numerical results of changes from radiative forcing. Reduce the numbers of acronyms and use relative results (preferential in %) in addition to numerical data to show the changes of radiative forcing. This would help the reader to understand the executive summary. [Government of Germany]	Taken into account by including more relative changes.
8-60	8	3	16			Replace 'has a best estimate of' with 'is'. A best estimate and an uncertainty range are given. [Nathan Gillett,	Accepted, modified as suggested.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Canada]	
8-61	8	3	16			There is a potential inconsistency between the estimated total forcing, climate sensitivity and the observed ocean and surface warming. A large part of this is related to the aerosol total forcing estimated to be much smaller than in AR4. If all the forcing numbers are taken at face value, the observed warming implies a climate sensitivity of 2K or less, which is inconsistent with CMIP5, and which would imply much smaller warming in the future. This potential inconsistency needs to be checked, and if real its implications need to be discussed in one of the chapters. [Reto Knutti, Switzerland]	Taken into account. The estimate of total aerosol ERF from Chapter 7 is slightly revised compared to SOD. Implication of forcing estimates related to climate sensitivity from historical warming is beyond the scope of this chapter and this part of the comment is rejected.
8-62	8	3	16			Only if you accept the low magnitude of aerosol forcing from the aerosol chapter. As indicated there I think that the aerosol forcing is underestimated. [Stephen E Schwartz, United States of America]	Taken into account by using an updated estimate from Chapter 7 with a lrager uncertainty range than in SOD.
8-63	8	3	17	3	17	It should "total anthropogenic RF" in the middle of the line. [Olivier Boucher, France]	Taken into account by removing anthropogenic RF and only provide total anthropogenic ERF.
8-64	8	3	17	3	17	It seems rather silly to be saying it is "virtually certain" that human influences are positive instead of simply saying it is positive--there is just no other alternative that is plausible. Take the step and be really clear to decisionmakers. Fine to have a range, but this sounds silly. We say the greenhouse gases are well-mixed when that is not exactly the case, so take the leap. [Michael MacCracken, United States of America]	Taken into account by removing the sentence on human influence is positive.
8-65	8	3	17	3	19	“The total anthropogenic RF is 50% higher compared to AR4 (2005) due primarily to reductions in estimated aerosol RF but also to continued growth in greenhouse gas RF.” The time period this statement relates to should be made clearer. [Government of Australia]	Taken into account, by including both growth in WMGHG and reduction in magnitude of aeroslo forcing.
8-66	8	3	17			To clarify the text, the authors should consider revising the text to read: "the RF estimate is 50% greater than in AR4." In other words, the authors could include the word "estimate" or give a range of 46%--54% increase. [Government of United States of America]	Taken into account, 'estimate' is included, see also comment8-65
8-67	8	3	22	3	25	The effect of updating the calculations alone should ideally be quantified by giving the difference in RF for the same time for AR4 and AR5 methods, I think. On the other hand, since it is so small, it may not be worth mentioning in the Exec Summ. The date of the AR4 was 2007, not 2005. I assume you mean the date for which the AR4 made the assessment, but this is potentially confusing. [Jonathan Gregory, United Kingdom]	Accepted: We will remove this comment from the ES
8-68	8	3	22		32	Make sure that these RF uncertainties include the uncertainty in the PI abundances of CO2, CH4, and N2O. [Michael Prather, United States of America]	Accepted: These will be included in the final draft.
8-69	8	3	22			I would urge the authors of this chapter to drop the reference to "well-mixed" greenhouse gases and to refer to "long-lived" greenhouse gases instead. Reasons are given in a set of separate comments below. [Adrian Simmons, United Kingdom]	Rejected: For the discussions in this chapter, the property of most relevance is whether a gas is well-mixed or not. We do not consider the lifetimes except when calculating the climate metrics. We will clarify that these gases are only well-mixed within the troposphere.
8-70	8	3	22			In the first place, these gases are referred to as "long-lived" in chapters two, six and twelve (and briefly in chapters nine and eleven also, though chapter ten has a "well mixed"), and one would look for uniformity of terminology across the WG1 assessment. Please also see later comments on the SPM. [Adrian Simmons, United Kingdom]	Taken into account: "Well mixed" will be used throughout
8-71	8	3	22			There is even inconsistency within Chapter 8 itself. In FAQ 8.2 there is reference (page 8-64, line7) to "carbon dioxide and other long-lived greenhouse gases". [Adrian Simmons, United Kingdom]	Taken into account, FAQ8.2 is corrected.
8-72	8	3	22			The major long-lived greenhouse gases are in fact not well mixed. This is most certainly the case in the stratosphere, where methane varies very substantially due to oxidation in the upper stratosphere and where variation in the carbon dioxide distribution is used as an important indicator of "age of air". In the troposphere there are marked variations not only near sources, but also where the flow advects values away from source regions. Methane has a large source localised over Northern India, but this methane is transported upwards in summer by monsoon convection and large scale flow, and it accumulates in the large-scale Tibetan	Noted: While we agree that there are local variations in the concentrations of the WMGHGs, for the purposes of radiative forcing these variations are generally unimportant. The exception may be variations in the stratosphere and we will add a comment in section 8.3 to reflect this.

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						anticyclone, from which it is likely that values are peeled off in streamers that extend over large distances, as this is seen in models of the methane distribution and is a known feature of the humidity and potential vorticity fields. Gases and aerosols emitted from regions of wildfires also can be identified far from their sources. [Adrian Simmons, United Kingdom]	
8-73	8	3	22			Observations of the long-lived greenhouse gases from only a few remote sites may well be suitable for some climate purposes, such as RF calculations (page 8-10, lines 6 and 7) but comprehensive observation is needed for other climate purposes, for top-down estimation of surface fluxes, for example. Utilization of "well mixed" to characterize these gases does not help the case that some have to advance for both in-situ networks and satellite- and aircraft-based measurements. [Adrian Simmons, United Kingdom]	Noted: This chapter is concerned only with the RF calculations and has no remit to help justify cases for new measurement campaigns.
8-74	8	3	23	3	25	Change "0.01 W m–2 is due to updates in the RF calculations and the rest" to "nearly all is" as the extra detail is just distracting here. [Robert Portmann, United States of America]	Accepted
8-75	8	3	23			10% error in wmghg forcing. I think this estimate is greatly optimistic. CO2 forcings and climate response of 15 atmosphere-ocean general circulation models (GCMs) that participated in round 5 of the Coupled Model Intercomparison Project (CMIP-5) were compared by Andrews et al (2012). Forcing and temperature response coefficient were inferred from the output of the model runs respectively as intercept and slope of a graph of net top-of-atmosphere energy flux versus global mean temperature anomaly subsequent to a step-function quadrupling of atmospheric CO2. (Because the model experiments examined response to a quadrupling of CO2, rather than a doubling, the intercept had to be divided by 2 to obtain the forcing pertinent to doubled CO2). The forcing is interpreted as an "adjusted forcing" that includes rapid adjustments, mainly of atmospheric structure, that modify the TOA radiative flux on time scales shorter than a year or so. A key finding of Andrews et al. was the spread of values of forcing exhibited by the different GCMs, 16%, 1-sigma. The spread in forcing is a consequence of differing treatments of the radiation transfer in the several models as well as different treatments of clouds that interact with radiation. As the forcing inferred from the analysis of Andrews et al. is an adjusted forcing, it appropriately reflects differences among the models in rapid (<~ 1 yr) response of atmospheric structure to the imposed forcing. This spread in forcings inferred from the climate model runs is substantially greater than the uncertainty specified in the Figure. That there is such a range of forcing as inferred from GCM runs should not come as much of a surprise. For example, although the Radiative Transfer Model Intercomparison Project (Collins et al., 2006) reported a 1-sigma spread in longwave forcing at 200 hPa among the GCMs compared of only 8.5%, that study was restricted to cloud-free atmospheres, with the reason given that "the introduction of clouds would greatly complicate the intercomparison exercise," from which one infers that the spread of forcing in a model with clouds would greatly exceed that in an idealized cloud-free model. Hence the finding of a 1-sigma spread of ± 16% in the forcings (i.e., 5-95% range ± 26%, well greater than the ± 10% shown in the figure) is likely as accurate an assessment of the maximum level of confidence as can be placed at the present time in forcing by LLGHGs. Andrews, T., Gregory, J. M., Webb, M. J. and Taylor, K. E. 2012. Forcing, feedbacks and climate sensitivity in CMIP5 coupled atmosphere-ocean climate models. Geophys. Res. Lett. 39, L09712. Collins, W. D., Ramaswamy, V., Schwarzkopf, M. D., Sun, Y., Portmann, R. W., Fu, Q. et al. 2006. Radiative forcing by well-mixed greenhouse gases: Estimates from climate models in the IPCC AR4. J. Geophys. Res. 111, D14317. [Stephen E Schwartz, United States of America]	Rejected: Here we are assessing the uncertainty in RF from line-by-line calculations which we assess to be +/- 10%. We agree that the radiation schemes used in GCMs may well vary by more than this, but we regard that as an uncertainty in the climate modelling, not an uncertainty in our knowledge of the radiative effects of the WMGHGs.
8-76	8	3	25			What does "industrial era RF" exactly mean, please be more precise as to the years. [Michael Prather, United States of America]	Rejected: The year (1750) is already mentioned in this sentence.
8-77	8	3	26			The statements about largest etc. apply only in the global mean. Local forcings may exceed that of CO2. [Reto Knutti, Switzerland]	Noted: This bullet point deals solely with the global RF and we do not wish to add further detail on regional effects here
8-78	8	3	28	3	32	The use of % change since AR4 is good, but must be done for CO2 in parallel, then I calculate 9% (1.66-1.82), which is not that different from N2O - I think you need to reword these comparisons. [Michael Prather, United States of America]	Accepted: We will add a % change in CO2. However the absolute RF change (which is what drives the climate) is still dominated by CO2.

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8-79	8	3	30	3	32	The date of AR4 was 2007, not 2005. I assume you mean the date for which the AR4 made the assessment, but this is potentially confusing. [Jonathan Gregory, United Kingdom]	Accepted: This will be clarified
8-80	8	3	30			WMGHGs [Stephen E Schwartz, United States of America]	Accepted
8-81	8	3	30			"weak increase"; relative to their increase in previous years?; the comparison needs to be explicit and on a percent per year basis. [Stephen E Schwartz, United States of America]	Taken into account: This phrase will be reworded to explicitly refer to the absolute RF changes.
8-82	8	3	32			Does the 6% increase refer to the concentration of N2O or its radiative forcing? It appears to refer to the concentration, but the previous sentence describes a percentage increase in the forcing of CH4. [Nathan Gillett, Canada]	Taken into account: This will be rephrased to explicitly refer to the % change in RF
8-83	8	3	34	3	34	Is it just the Montreal Protocol, or also subsequent amendments? [Jonathan Gregory, United Kingdom]	Taken into account: "and amendments" added.
8-84	8	3	39	3	40	It is ambiguous whether the 0.02 W/m2 is the increase in RF since AR4 or the RF since pre-industrial times. [Philip Cameron-Smith, U.S.A.]	Taken into account: Clarified that this is the total RF since pre-industrial
8-85	8	3	39			Not sure whether that should be HFC or HCFC? [Government of United States of America]	Noted: This should be HFC as written.
8-86	8	3	40	3	40	I suggest replacing "and has an RF of" with "and now amounts to" . [Jonathan Gregory, United Kingdom]	Accepted
8-87	8	3	40	3	42	"There is high confidence that the growth rate in RF from all WMGHG is weaker over the last decade than in the 1970s and 1980s owing to a reduced rate of increase in the non-CO2 RF." This statement could be made clearer - is it all non-co2's that have seen a reduced rate of increase and just co2 that has increased? Is it also due to changes in calculating the RF for some WMGHGs? [Government of Australia]	Noted: It is the total of the non-co2 RFs that has shown a reduced rate of increase
8-88	8	3	40			An increase of 78% from a number that was almost zero (HFCs = 0.01 W/m2 in AR4?) is a bit absurd. It is less than 2% of the total, why not mention the real numbers to avoid appearing biased. If you want to be fair: " has nearly doubled but represents a small fraction of the total, 0.02 W/m2" [Michael Prather, United States of America]	Accepted
8-89	8	3	40			For greater clarity the following wording is suggested: There is high confidence that the overall growth rate in RF ... [Klaus Radunsky, Austria]	Accepted
8-90	8	3	44	3	44	It would be nice if this paragraph included an estimate of the total RF due to all short-lived GHG (even if uncertainty is relatively high) [George Ban-Weiss, United States of America]	Rejected: We will not consider all short-lived GHGs in this section, only ozone and stratospheric water vapour
8-91	8	3	44	3	44	The title to this section talks about short-lived GHGs, but then this talks only about ozone, except a very minor nod in the last sentence to other species. It would seem appropriate to specifically mention the other short-lived gases here, and I would think that methane should also be covered in this paragraph given the international effort to address methane as ashort-lived species. [Michael MacCracken, United States of America]	Taken into account: The title will be changed to ozone and stratospheric water vapour
8-92	8	3	44	4	6	Here the radiative forcing from changes in ozone is quantified. Can you clarify whether this radiative forcing is because of reduced/enhanced tropospheric/stratosphere ozone since pre-industrial? Also, is there any opposing forcing from the Antarctic verses the rest of the globe as Figure 8.24 shows the Antarctic forcing to be anomalous relative to the rest of the globe. [European Union]	Rejected: The different changes in tropospheric and stratospheric are already described. We do not want to add to this paragraph on global changes by descibing regional forcing.
8-93	8	3	46	3	46	the semicolon ; should be a comma , [George Ban-Weiss, United States of America]	Editorial
8-94	8	3	47	3	47	I would list volatile organics and nitrogen oxides before methane since they are more important for tropospheric ozone [George Ban-Weiss, United States of America]	accepted
8-95	8	3	48			volatile organic compounds [David Stevenson, United Kingdom]	accepted
8-96	8	3	49	4	3	All of this is OK when explained in the chapter, but in the ES it is confusing and not really important as clearly seen with the confidence levels given. In fact, the ozone statement is very confusing as it could be misread that there has been no damage to vegetation and agriculture from the 30% increase in O3 since PI. Drop these statements from ES. [Michael Prather, United States of America]	Taken into account

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8-97	8	3	50	4	1	In addition to the statement on then RF of ODSs, there should be a sentence on the large latitudinal differences in the RF. [Guus Velders, Netherlands]	Rejected: We don't want to discuss regional changes here, as the extra text may detract from the central point of the bullet.
8-98	8	3	50		51	How can ODS's have caused a greater RF today (-0.2) than that attributed to all O3 depletion today? Either way, this statement and the following are not really ES material. [Michael Prather, United States of America]	Rejected: ODSs have also decreased the tropospheric ozone forcing by ~0.1
8-100	8	3	51			How come the ozone RF attributed to ODSs is twice as large as the stratospheric ozone RF itself on line 45? Most of the change in stratospheric ozone is attributable to ODSs. Climate change has probably only played a small role in historical ozone changes. See e g. WMO (2007). Or is this an effect of stratospheric ozone depletion on tropospheric ozone? [Nathan Gillett, Canada]	Taken into account: ODSs have also decreased the tropospheric ozone forcing by ~0.1
8-101	8	3		3		The executive summary includes all chapters of the chapter 08 about radiative forcing. But the listing and the headings are not so easily readable and are not congruent with the following text. This should be reworked, e.g. page 5 line 24 should have a similar heading like 8.6 Geographic Distribution of Radiative Forcing (see Page 1 line 49). [Government of Germany]	Taken into account by improving the first sentences in the paragraphs.
8-102	8	3		3		Exec summary went from nice set of compact bullets in FOD to a set of turgid paragraphs. Suggest revert. In exec summary one is looking for findings, not dissertations. [Stephen E Schwartz, United States of America]	Rejected, the style of ES in the SOD is decided by TSU. Several of the paragraphs are shortened to highligth the most important material.
8-103	8	3		6		It seems that the executive summary relies on global mean RF values whereas the underlying chapter makes ample reference to the regional variation of anthropogenic RF changes. I would think that this fact could warrant mentioning in the Executive Summary of this chapter. [Jochen Harnisch, Germany]	Rejected, we feel the balance between global and regional forcing is sufficiently taken into account. The WMGHG is based on global numbers.
8-104	8	3		6		The title of the chapter is "Anthropogenic and Natural Radiative Forcing". However, the Executive Summary (and the underlying chapter) seems to mainly relate to anthropogenic RF. Shouldn't there be some quantitative/qualitiative framing of anthropogenic RF vis-a-vis natural also in the executive summary? This may be very basic to climatologists, but would add credibility. [Jochen Harnisch, Germany]	Rejected, in SOD it is one paragraph on anthropogenic versus natural forcing.
8-105	8	3				Footnote 2: In the rest of the chapter AF seems to be defined with SSTs and sea ice fixed but not land temperatures. Say this here. [Nathan Gillett, Canada]	Accepted, added.
8-106	8	3				The radiative forcing discussed in Chapter 8 refers to the period from 1750 to 2011. However, most CMIP5 historical simulations start from 1850 to 2005. Should the text be consistent with the CMIP5 period. [Government of United States of America]	Rejected, to be consistent with previous IPCC reports and account for the changes over the industrial era we use 1750-2011.
8-107	8	3				Table 8.SM-1: Showing Table 8.SM-1 with two and a half pages of numbers does not communicate well or efficiently. I recommend replacing this table with an expanded graph of the variations. e.g. with a vertical scale from 1360 to 1362 W/m2. Then post the table itself at a public access site and provide details in the text as to how to access that table. [David L. Hagen, United States of America]	Rejected, the SM is purely electronic and will simply be used as a documentation of the data.
8-108	8	3				Table 8.SM-1 Showing 9 significant figures in TSI when there is very high uncertainty in the 6th figure and high uncertainty in the 5th figure is unprofessional. Recommend rounding the calculated TSI to 6 figures maximum. [David L. Hagen, United States of America]	Accepted
8-109	8	3				Table 8.SM-1 More helpful would be to add another graph to show the variations in UV vs VIS and highlight their inverse variations and the corresponding uncertainties involved. [David L. Hagen, United States of America]	Taken into account: This comment seems to be related to the paper by Harder et al. (2009) where they found that the Spectral Irradiance Monitor (SIM) on board of the Solar Radiation and Climate Experiment (SORCE) measurements suggested that over SC 23 declining phase, the 200–400 nm UV flux decreased by 10 times more than expected from prior observations and model calculations and in phase with the TSI trend, while surprisingly the visible presents an opposite trend. However, SIM's solar spectral irradiance measurements from April 2004 to December 2008 and inferences of their climatic

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							implications are incompatible with the historical solar UV irradiance database, coincident solar proxy data, current understanding of the sources of solar irradiance changes, and empirical climate change attribution results, but are consistent with known effects of instrument sensitivity drifts; thus what seems to be needed is improved characterization of the SIM/SORCE observations and extreme caution in studies of climate and atmospheric change (Haigh et al., 2010) until additional validation and uncertainty estimates are available (DeLand and Cebula, 2012; Lean and DeLand, 2012). As there are serious issues about the UV measurements having an inverse variation with the visible, we do not think appropriate to include the suggested figure.
8-110	8	3				Ch. 8 Executive summary and following sections. The multiplication of “radiative forcings”, “warming potentials”, feedbacks and time scales, is undoubtedly a factor of confusion among non-specialists and aging specialists like me. Bravo for this largely successful attempt to illuminate the issues! [Robert Kandel, France]	Noted.
8-111	8	4	1			It is very surprising that there is only "medium evidence" on tropospheric ozone's detrimental impacts on vegetation. There is a robust and long history of literature on this topic (e.g., Fishman and many others) showing a negative impact on soy beans and other crops, as well as broad leaf trees. [Government of United States of America]	Accepted, changed to robust evidence and low confidence in quantification.
8-112	8	4	5	4	5	To justify excluding stratospheric water vapour decrease as a forcing, it would be helpful to state that it is thought to be due to internally generated climate variability, or climate feedback, if that is what you mean. [Jonathan Gregory, United Kingdom]	Noted: The sentence on natural changes has been removed
8-113	8	4	6	4	6	Are they similar because there has not been much real-world change between 2005 and 2011, or because scientific understanding has not changed, or both? The former would be a useful thing to state. If the latter, it's probably not worth mentioning in the Exec Summ. [Jonathan Gregory, United Kingdom]	Noted: This is probably not worth mentioning in the Exec Summ.
8-114	8	4	8			I would say if the numbers hold up, substantially reduced. [Stephen E Schwartz, United States of America]	Taken into account, uncertainties in aerosols have been increased since SOD.
8-115	8	4	9	4	9	I suspect it's still referred to as the direct effect in some literature and perhaps in some AR5 chapters, so "sometimes" would be fairer than "formerly", or simply omit this phrase. [Jonathan Gregory, United Kingdom]	Accepted.
8-116	8	4	10			The uncertainty range for aerosol cloud interaction is too narrow. [Henning Rodhe, Sweden]	Taken into account, uncertainties in aerosols have been increased since SOD.
8-117	8	4	12	4	13	What is the uncertainty in the Total Aerosol RF? Replace "The new RF estimates of aerosol are weaker than in AR4" with "The new estimates of total aerosol forcing are smaller than those reported in AR4". [Robert Waterland, United States of America]	Taken into account, part of the sentence is rewritten and some removed.
8-118	8	4	13			Uncertainty range missing on total aerosol RF. [Nathan Gillett, Canada]	Taken into account, total aerosol RF removed.
8-119	8	4	13			"weaker" => "smaller" - weak is an odd usage. [Michael Prather, United States of America]	Taken into account, and sentence rewritten and weaker not used. sentence
8-120	8	4	19	4	19	The influence of land use change is narrowed down to the global effect on radiative forcing. This approach is far too narrow. In Pielke, Roger A., Pitman, Andy, Niyogi, Dev, Mahmood, Rezaul, McAlpine, Clive, Hossain, Faisal, Goldewijk, Kees Klein, Nair, Udaysankar, Betts, Richard, Fall, Souleymane, Reichstein, Markus, Kabat, Pavel and de Noblet, Nathalie (2011) Land use/land cover changes and climate: modeling analysis and observational evidence. Wiley Interdisciplinary Reviews-Climate Change, 2 6: 828-850 the authors write: "We conclude that existing climate assessments have not yet adequately factored in this climate forcing. For those regions that have undergone intensive human landscape	Rejected, outside the scope of the this chapter. This chapter discuss forcing and mention shortly non-radiative forcing mechanisms is slightly mentioned.

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						change, or would undergo intensive change in the future, we conclude that the failure to factor in this forcing risks a misalignment of investment in climate mitigation and adaptation." In the paper: "In terms of an effect on the global average radiative imbalance, Forster et al.18 suggest that this direct biogeophysical radiative impact of LULCC since preindustrial times is a reduction in the global average radiative forcing of 0.2 ± 0.2 W m−2 which is small relative to other global climate forcings. Reasoning of this kind has led to the role of LULCC being mostly omitted from the climate models used in previous Intergovernmental Panel on Climate Change (IPCC) assessments of climate projections and historical reconstructions (although deforestation is included via emission scenarios of CO2). The role of climate science, however, extends beyond forming future emission mitigation policies. The role of LULCC is not limited to radiative forcing of climate and is not adequately assessed as a globally averaged forcing. LULCC is a highly regionalized phenomenon18,19 with regional-scale climate impacts that can vary in the sign of the change. In terms of an average flux, in regions of significant LULCC, a major perturbation occurs to the net radiation, to the partitioning of this net radiation between the two turbulent energy fluxes (sensible and latent heat), as well as changes in the aerodynamic roughness of the land surface.20,21 LULCC also fundamentally changes the biogeochemistry, including the terrestrial carbon exchange, and fluxes of trace gases (such as nitrous oxide), biological volatile organic compounds, and aerosols (including dust). Urban landscapes add additional direct heating of the lower atmosphere. The biogeography is also changed as flora and fauna are altered by deliberate and inadvertent land management and the introduction of invasive species.22" [Marcel Crok, The Netherlands]	
8-121	8	4	19	4	25	Can you briefly summarise the anthropogenic land use changes that have caused an increase in the land surface albedo here? Is there a region of the world that dominates in these changes? [European Union]	Rejected, too detailed for ES.
8-122	8	4	19	4	25	There are a number of land use forcings related to urban areas that I would think should be mentioned specifically--hopefully they are covered. So, there is the change in the land surface to buildings and pavement, but a term not really being discussed but which is important in major metropolitan areas, and that is the heat of combustion from fossil and other fuels (can be order 10 W/m2 or more over megalopolis sized domains). When model grids were five degrees or so this term could be dismissed as small, but with finer grids in global models and then nested models, these terms just can't be ignored (or if they are, mention this so that by the next assessment they get covered for they can make a difference). [Michael MacCracken, United States of America]	Rejected, too detailed for ES.
8-123	8	4	20	4	20	Please give the same number of decimal places in the uncertainty and the central value. [Jonathan Gregory, United Kingdom]	Accepted.
8-124	8	4	22			Say what these other effects are in brief. I think this is referring to physical effects not emissions from land use change, but this is not clear. [Nathan Gillett, Canada]	taken into account, by adding 'in particular to the hydrologic cycle.'
8-125	8	4	27	4	28	This needs to say the exact period considered, and whether the forcing quoted is a linear trend or a difference between specific years. Because of the variability in TSI, I think this will make a big difference (and this is shown in the chapter). [Nathan Gillett, Canada]	Taken into account: This is a difference between two years: 1986 and 2008. We have specified this.
8-126	8	4	27	4	28	I am not sure what this means. Because solar cycles are mentioned, I wonder if it is an amplitude of the solar cycle. If not, it might be the difference between 2011 and 1978 exactly, but in the case, the phase of the cycle is relevant. Do they correspond? Is that why you choose 1978, which should be explained? Or maybe it's the result of using a fitted trend to estimate the underlying change between these dates. [Jonathan Gregory, United Kingdom]	Taken into account: We have changed the text. Now we specify that the RF is obtained as the difference between two solar minima years: 1986 and 2008. We deleted the part of RF between 1986 and 2011.
8-127	8	4	27	4	36	In the executive report, natural solar forcing is reflected only by the TSI, it ignores the spectral variations especially in the UV, this approach excludes possible UV triggers and non linearity. Stronger wording should be used to state that only the observations of the satellite ages have real metrological value as three solar cycles of observation are insufficient to validate the values obtained from the different proxies (sunspot numbers, solar indexes...) [Christian Muller, Belgium]	Taken into account: We will change the text to emphasize that the RF for the satellite era has the greatest confidence. We mention the UV in sections 8.4.1.4. Particularly in 8.4.1.4 we say that "although metrics based only on TSI are not appropriate, UV measurements present several controversial issues and modeling is not yet robust ".

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8-128	8	4	30	4	31	On the basis of very unceratin and diverging results this number is fundamentallly uncertain and has become more so with the later results. Inaddition the reduction of the impact to 78% is based on a single publication that inaddition states that there is a built in uncertainty of at least 5% due to cycle variation. Additionally there is the incrinsic averging effct around 1750 that may give a too small variation from preindustrial times. I would propose to rewirite the two lines with: " ......rely on a number of indirect proxies that inherently do not give consistent results,For the time around 1750 there is a variation 0,75 Wm-2. This large uncertainty makes i difficilt to give both an average value and an uncertainty around this.With there provisos the best estimate of the RF from TSI over the industrial era os in the region 0-0,11 Wm-2 with a mean around 0,05 Wm-2. [Bo Andersen, Norway]	Taken into account: We will revise this part of the text. Also we will rewrite section 8.4.1 .2 to include these remarks.
8-129	8	4	32	4	33	The AR4 was only about half a cycle ago, not a full cycle, as this appears to imply. [Jonathan Gregory, United Kingdom]	Taken into account: We have changed the text and now it is clear that it is due to the addition of the last solar cycle descending phase and minimum.
8-130	8	4	34	4	34	“The recent solar minimum was unusually low and long-lasting”. This is only true for the last century. Minima preceding cycles 14 and 15 (1901 and 1913, resp.) were comparable to the minimum in 2008. There were more examples in the previous centuries. [Natalie Krivova, Germany]	Taken into account: Agree with your comment. However, we have changed the text and this phrase does not appear anymore.
8-131	8	4	35	4	36	What specifically limits the ability to project solar forcings? Satellite observations? Modelling capability? Historical observations? [European Union]	Taken into account: In the first instance is modelling. However, most modelling techniques use past data which include satellite and historical observations.
8-132	8	4	36	4	36	"[8.4.1; Figure 8.13; Figure 8.14]". I think these should be Figures 8.12 and 8.13. 8.14 shows volcanic reconstructions [Natalie Krivova, Germany]	Taken into account: The figures are now 8.10 and 8.11.
8-133	8	4	38	4	39	I wonder why you mention a timescale for climate impact in this case, which you do not do for any other RF agent. Any radiative forcing has a timescale of impact as long as the memory of the climate system. [Jonathan Gregory, United Kingdom]	Taken into account by modifying the first line of the paragraph.
8-134	8	4	38	4	39	The RF from stratospheric aerosols can certainly have a large impact on the climate for a year or two after eruptions of the strength of that of Mt Pinatubo in 1991, but is there evidence to justify the words "to decades after volcanic eruptions"? Maybe for the supereruptions that have occurred in the more distant past, but the decadal or multidecadal timescale is not discussed in section 8.4.2.3 where there is mention of extreme eruptions. Multi-decadal variations in the frequency or intensity of volcanic activity could impart multi decadal variability on climate, but this is not as far as I could see mentioned here (though it is in chapters ten and eleven). There is some modelling evidence of a decadal ocean response discussed in 8.4.2.3, but as there is a lack of observational evidence and the mechanism is described as not well understood, one might query whether it justifies the words "large impact" that appears on line 38 of page 8-4. [Adrian Simmons, United Kingdom]	Taken into account by using 'greatest for a short period(~2 years) following volcanic eruptions'.
8-135	8	4	38	4	44	Stratospheric aerosol was mentioned only in the context of volcanoes. Are the authors saying that SO2 emissions have no effect on stratospheric aerosol? [Government of United States of America]	Rejected, There is little evidence of anthropogenic change to the stratospheric aerosols over the decades, although the Hofmann et al. paper suggested it.
8-136	8	4	39			Isn't decades an overestimate? Admittedly I know long timescale effects have been shown for the ocean, but I think this is longer than demonstrated responses in surface temperature. [Nathan Gillett, Canada]	Taken into account, see comment 6-134.
8-137	8	4	40			Replace 'a RF' with 'a mean RF'. [Nathan Gillett, Canada]	Taken into account, sentence a periods modified.
8-138	8	4	40			Please put in an estimate for the decadal average RF 1991-2000 for Pinatubo as you did for the smaller ones in the following decade. [Michael Prather, United States of America]	Rejected, the focus is on changes since AR4. Such values can be found in section 8.5.
8-139	8	4	41	4	42	I don't think the climate impacts of these post-2000 eruptions have been identified in the observations. So how have these eruptions helped understanding of the depedence of climate impacts on the amount of material etc? This must be just through modelling studies, which could have been done with or without the eruprtions themselves happening. [Nathan Gillett, Canada]	Taken into account, sentence deleted.

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8-140	8	4	42	4	44	The comparison between emissions of CO2 from volcanic eruptions and anthropogenic emissions should be precised. What is the underlying time horizon? [Government of Germany]	Taken into account by stating that this is since 1750.
8-141	8	4	54	4	54	This statement is false. There's very high confidence that natural forces caused virtually all of the warming since 1960. This was demonstrated in McLean et al (2009) where Figure 7 showed the very clear relationship between ENSO and average global lower tropospheric temperature. (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal but conside rthe paper on its merits rather than ignore it because of a phony criticism?) [John McLean, Australia]	Rejected, no scientific evidence to support that anthropogenic forcing is small compared to natural forcing.
8-142	8	4	54			This claim is contradicted by various sets of data pointing towards considerable natural variability of climate discussed for example by Humlum et al (2012), Akasofu (2010), Loehle and Scafetta (2011), brilliantly also in the recent book of Bob Tisdale « Who turned on the heat ? The unsuspected Global Warming Culprit, El Niño-Southern Oscillation », and by the observed deceleration of sea level rise discussed in comments 3 0 and 3 77. [François Gervais, France]	Rejected, no scientific evidence to support that anthropogenic forcing is small compared to natural forcing.
8-143	8	4	54			need to say forcing over a specific time period. [Stephen E Schwartz, United States of America]	Taken into account by adding time periods.
8-144	8	4	55	4	57	The Second Order Draft acknowledges strong evidence of solar forcing beyond TSI but still needs to take account of the implications In a huge improvement over the First Order Draft, the SOD acknowledges strong evidence for a solar forcing more powerful than the slight variance in Total Solar Insolation. Both drafts cite a few papers that find correlations between solar activity and climate, but the SOD now adds the following sentence (p. 7-43, lines 2-4): "The forcing from changes in total solar irradiance alone does not seem to account for these observations, implying the existence of an amplifying mechanism such as the hypothesized GCR-cloud link." This important acknowledgment requires corresponding changes throughout the report that have still not been made. The main conclusion of the entire report, stated in the first line of the Executive Summary, is that advances since AR4 "further strengthen the basis for human activities being the primary driver in climate change" (p.1-2, lines 3-5, unchanged from the FOD). This conclusion is a direct result of the Chapter 8 assertion that: "There is very high confidence that natural forcing is a small fraction of the anthropogenic forcing." (Page 8-4, line 54.) As the next three lines in Chapter 8 explain, this assertion is arrived at by comparing anthropogenic forcings only to TSI and volcanic aerosols (p. 8-4, line 55-57): "In particular, over the past three decades (since 1980), robust evidence from satellite observations of the TSI and volcanic aerosols demonstrate a near-zero (–0.04 W m–2) change in the natural forcing compared to the anthropogenic AF increase of ~1.0 ± 0.3 W m–2." But as the SOD now acknowledges, there is strong evidence for solar forcing more powerful than TSI. That evidence invalidates any comparison between natural and anthropogenic forcings that does not include any solar effects but TSI. Thus the chapter 8 premise needs to be altered: there can be no "high confidence" that natural forcing is a small fraction of the anthropogenic forcing. And the Executive Summary conclusion must be similarly altered: the accumulation of evidence for a solar forcing beyond TSI does not "further strengthen the basis for human activities being the primary driver in climate change," but weakens it considerably. How much? As I noted in my FOD comments, dozens of studies have found correlations of .4 to .7 between solar activity and various measures of climate (temperature, rainfall, etcetera). That is, solar activity "explains" in the statistical sense something like half of all past temperature change and there is no reason to think that the last century was any different. Solar activity was persistently high and the planet did a modest amount of	Rejected, we state that it is high confidence (medium evidence and high agreement) that the GCR effect is too weak to influence CCN over the last century.

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						warming. Now that the sun has gone quiet, warming seems to have stopped. The rough outlines fit well with a solar explanation. If this temperature history is half due to the sun, that cuts in half the amount of warming that could be attributable to CO2, diminishing the implied climate sensitivity accordingly, and in the absence of a hypersensitive climate there is absolute nothing to fear from CO2. Any modest amount of warming is good. The only actual danger would be runaway warming, driven by a high climate sensitivity, and any substantial solar-warming effect takes that possibility completely off the table. The real danger is that solar variation might well turn out to be a MORE powerful climate driver than the human contributions to atmospheric CO2, in which case our now quiescent sun portends a period of global cooling, which we know from the planet's history of glaciation really can feed on itself in runaway fashion and really is something to worry about. What has become clear since AR4 is 1) that there has been a cessation of warming (whether temporary or long term) and 2) the growing mountain of evidence for some mechanism of solar forcing far more powerful than the slight variation in TSI. AR5 recognizes that evidence, now it needs to also recognize the implications: we can have NO confidence that anthropogenic forcings are greater than solar forcing and hence no confidence that human activity is the primary driver in climate change. If the report is to be honest, this needs to be stated right in the first line of the Executive Summary. [Alec Rawls, United States of America]	
8-145	8	4	55		57	This is misleading, the -0.04 is based on TSI I believe, but the Pinatuba RF was larger (negative). For these comparisons, what is needed is the average RF, not the instantaneous at present, which underestimates the climate impact of volcanos. [Michael Prather, United States of America]	Taken into account by rewriting of the text and more explicit mentioning of the time periods.
8-146	8	4	56			It's key to say how the change in natural forcing is calculated here. If a linear trend were fitted to the natural forcing since 1980 I'm sure this would be a postiive forcing, due to the two large volcanic eruprtions in the first half of the period. This must be a difference of end points. This needs to be stated. [Nathan Gillett, Canada]	Taken into account by explicit given in the text how this is calculated.
8-147	8	4	57	3	57	It might not be appropriate here but this seems like a good place for a sentence on natural forcing from natural aerosols (dust or sea salt) [George Ban-Weiss, United States of America]	Rejected, beyond the scope of Chapter 8.
8-148	8	5	2	5	6	Either list the forcings with high, medium and low coinfidence, or just give a summary statement without giving numbers of forcings. [Nathan Gillett, Canada]	Taken into account, paragraph deleted.
8-149	8	5	3	5	5	I tend to think the sentence "Five forcing agents …" is not particularly informative and could be omitted. It is implied by the preceding sentence that the others are all less than "very high". The following sentence is useful, however. [Jonathan Gregory, United Kingdom]	Taken into account, see comment 8-148
8-150	8	5	4	5	5	Might be worth stating which two forcing agents only have 'very low' confidence in the level of understanding. It is useful to know where uncertainty is largest and where more research/monitoring is needed. [European Union]	Taken into account, see comment 8-148
8-151	8	5	8	5	8	I suggest an alternative phrasing of this summary statement: "Forcing is usually attributed to changes in concentration, but can alternatively be attributed to emissions." [Jonathan Gregory, United Kingdom]	Taken into account:This paragraph has been reworded
8-152	8	5	8			"rather than" should read "in addition to" [Stephen E Schwartz, United States of America]	Editorial: This paragraph has been reworded
8-153	8	5	9	5	10	This comparison is not clear, and I don't think it really makes sense to compare the two approaches in this way. Just say that in this framework, CO2 emissions give rise to the largest radiative forcing. Earlier in the ES it has already been stated that CO2 concentration changes are the largest contribution to radiative forcing. [Nathan Gillett, Canada]	Taken into account: This paragraph has been reworded
8-154	8	5	9	8	16	I think the title of this section is confusing. Would it be better as "Indirect forcing effects" or something similar? [Robert Waterland, United States of America]	Taken into account:This paragraph has been reworded
8-155	8	5	10			The following wording is suggested: .. rr the impact of net emissions. [Klaus Radunsky, Austria]	Noted: This paragraph has been reworded

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8-156	8	5	11			This statement seems contrary to fact. I recommend that the section be removed from the ES (and possibly the chapter if need be). The analysis of more RF but which is counteracted by chemical effects is illogical. If the emissions did not increase the abundances as much as expected, then how can those emissions have had a larger climate effect. This is all part of a hypothetical case (i.e.. NOx and O3 and climate did NOT change, but they did, so you cannot propose that CH4 emissions had a larger impact on climate). You can separate them in terms of a GWP-like metric, but not in terms of past climate change. This is just a different (arbitrary?) method of accounting, which could be done another way. What might be more important is the integrated RF from those emissions. Is this about future emissions or past RF. Dividing past RF by emissions is possible, but may be ambiguous - and this paragraph is certainly confusing. [Michael Prather, United States of America]	Rejected: This paragraph has been reworded
8-157	8	5	13	5	13	Omit "clear", which is not necessary. [Jonathan Gregory, United Kingdom]	Noted: This paragraph has been reworded
8-158	8	5	14	5	14	Nitrogen oxides also includes N2O and then the statement is not true. [Terje Berntsen, Norway]	Taken into account: This paragraph has been reworded
8-159	8	5	14	5	14	Omit "but uncertainties are large". This is implied by "likely". If you put this caveat in as well, I wonder whether "likely" is justified. [Jonathan Gregory, United Kingdom]	Taken I;nto account: This paragraph has been reworded
8-160	8	5	14	5	14	Note that the sign of the forcing for NOx is dependent on the location of emissions (as shown in Tables 8.A.3 and 8.A.7). The statement that NOx emissions likely have a net negative forcing hides this and is thus potentially misleading. An additional phrase acknowledging that the RF is variable (in space and time) as well as uncertain would be useful. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Noted: This paragraph has been reworded
8-161	8	5	14			N2O is also a nitrogen oxide, although not meant here, I presume. Best exclude N2O explicitely. [Michael Schulz, Norway]	Taken into account: This paragraph has been reworded
8-162	8	5	15	8	16	Perhaps it would be nicer to say, "their own positive radiative forcing outweighs the negative radiative forcing due to the stratospheric …". [Jonathan Gregory, United Kingdom]	Taken into account:This paragraph has been reworded
8-163	8	5	18	5	18	This is the first occurrence of the acronym RCP in the Chapter. It needs to be defined or breifly introduced in the following section. [Robert Waterland, United States of America]	Taken into account, RCP defined in a footnote with a reference to Chapter 1.
8-164	8	5	18		22	Not sure this is a finding; this is more a concensus opinion by the drafters of the RCP's. More generally, I would not include results for RCP's in this chapter these seem more pertinent to the modeling chapter. [Stephen E Schwartz, United States of America]	Rejected, the future forcing presented in this chapter is stated to be based on the RCP scenarios. Future forcing is calculated differently in the modelling chapter and the forcing chapter and the future forcing calculated in this chapter is consistent with rest of forcing estimates, rejected also to this comment.
8-165	8	5	18			This is the first occurrence of RCP in this chapter. As a result, an expanded discussion of what it means, etc. might be warranted. [Government of United States of America]	Taken into account, see comment 8-163.
8-166	8	5	18			The following wording is suggested: Differences in RF between RCP scenarios .. [Klaus Radunsky, Austria]	Taken into account, sentence rewritten.
8-167	8	5	21			"robust feature" => its worth mentioning that this is linked to the emission scenario which is rather harmonized [Michael Schulz, Norway]	Taken into account by adding 'for these scenarios'
8-168	8	5	24			The key gases in question are not "well mixed in the atmosphere". The case may perhaps be argued for the troposphere (though see comments 238 and 239), but certainly not the stratosphere. The stratosphere is part of the atmosphere. [Adrian Simmons, United Kingdom]	Taken into account, by not using 'well-mixed' in this paragraph.
8-169	8	5	26	5	26	"showed a maximum" should be "showed maximum". [George Ban-Weiss, United States of America]	Taken into account, paragraph rewritten as the following: 'Forcing agents such as aerosols, ozone and land albedo changes are highly heterogeneous spatially and temporally. These patterns generally track economic development; strong negative aerosol forcing appeared in eastern North America and Europe during the early 20th century, extending to

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							Asia, South America and central Africa by 1980. Emission controls have since reduced aerosol pollution, particularly in North America and Europe. Ozone forcing increased throughout the 20th century, with peak positive amplitudes around 15°N–30°N due to tropospheric pollution but negative values over Antarctica due to stratospheric loss late in the century. [8.6.2; Figure 8.24].'
8-170	8	5	26	5	26	This sentence does not make it clear that maximum values are actually maximum NEGATIVE values. I would change "maximum values over eastern…" to "maximum negative values over eastern…" [George Ban-Weiss, United States of America]	Taken into account, sentence rewritten, see comment 8-169.
8-171	8	5	26	5	26	"aerosol-radiation interactions", no need for a second hyphen [Olivier Boucher, France]	Taken into account, sentence rewritten, see comment 8-169..
8-172	8	5	26	5	26	remove "a" in "showed a maximum values" [Räisänen Petri, Finland]	Taken into account, sentence rewritten, see comment 8-169..
8-173	8	5	26	5	27	Replace "Industrial era RF initiated by aerosol-radiation-interactions showed a maximum values over eastern North America and Europe during the early 20th century," with "RF due to aerosol-radiation-interactions was highest in the early 20th century over eastern North America and Europe,". [Robert Waterland, United States of America]	Taken into account, sentence rewritten, see comment 8-169..
8-174	8	5	26	5	34	"Industrial era RF initiated by"??; "the magnitude has decreased" ?; "peak forcing"?; "shows similar behaviour" ? "In contrast, whole atmospger ozone forcing"? => the whole paragraph is not very precise. Do you describe the total aerosol+ozone+land-use forcing evolution, temporally and spatially? It's may be worth to detail the regional pattern of forcing and response? more. [Michael Schulz, Norway]	Taken into account, sentence rewritten, see comment 8-169..
8-175	8	5	26			Industrial era RF is vague - do you mean 2010? Also I think you mean "minimum" or "maximum negative" values ? [Michael Prather, United States of America]	Taken into account, sentence rewritten, see comment 8-169..
8-176	8	5	26			The following wording is suggested: .. showed maximum values over .. (delete "a") [Klaus Radunsky, Austria]	Taken into account, sentence rewritten, see comment 8-169..
8-177	8	5	26			a maximum values [David Stevenson, United Kingdom]	Taken into account, sentence rewritten, see comment 8-169..
8-178	8	5	32	5	32	replace "whole atmosphere ozone" with stratospheric and tropospheric ozone [Katharine Law, France]	Taken into account, sentence rewritten, see comment 8-169..
8-179	8	5	32	5	34	This sentence could be written more clearly. Do you mean that the negative forcing over the Antarctic became more negative and the positive forcing over the rest of the globe became more positive? Do you mean that the global average (troposphere and stratosphere) became more positive despite Antarctica becoming more negative? [European Union]	Taken into account, sentence rewritten, see comment 8-169..
8-180	8	5	36	5	38	Objection! GWP is not attributing sectoral emissions to climate change. It is not,e.g., an answer to the question what is the current change in climate from the air traffic sector. [Volker Grewe, Germany]	Taken into account, 'future' added before climate change
8-181	8	5	36	5	38	From industry contacts, I know that there is a large irritation about the interpretation of GWP, e.g. in the air traffic sector. [Volker Grewe, Germany]	Rejected, this is beyond the scope of the chapter since this is related to how GWP is adopted by policymakers.
8-182	8	5	36	5	38	The headline suggests that this addresses questions like "How much can we reduce warming by 2010 if we change to biofuels" GWP and GTP do NOT answer this question! What is needed here are a base case scenarion and a change in the emission scenarion, which includes a fuel replacement. The the temperature change (AGTP) in the year 2100 might be the right quantity to look at! [Volker Grewe, Germany]	Rejected. We do not feel this is implied, any more than showing forcing due to a compound like CO2 is equivalent to examining a CO2-emissions reduction scenario.We fully agree with the reviewer's comment, however, and try to make this same point in section 8.7.

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8-183	8	5	36	6	2	What are the uncertainties for Global Temperature change Potential GTP? Are these much lower than for Global Warming Potential GWP or have they not been assessed with the same level of scrutiny yet? This is a very useful discussion - perhaps a little lengthy for the Executive Summary but maybe that's necessary. [European Union]	Taken into account, by stating that uncertainties are larger for GTP than GWP. Specific uncertainty levels cannot be given here as they vary with time and across the emitted gas, so are too detailed for the ES.
8-184	8	5	36	6	2	As written, the bullet is quite long. The authors should consider revising it to something along the line of what appears in the SPM. Also, the authors might consider modifying the first sentence to read "Different metric have been proposed to …" [Government of United States of America]	Taken into account, by shortening the text.
8-185	8	5	36	6	2	This paragraph seems too long for the Exec Summ, and contains some repetition and jargon. I have some specific suggestions in following comments. [Jonathan Gregory, United Kingdom]	Taken into account, see comment 8-185.
8-186	8	5	36	6	2	Excellent text - which should be maintained. [Jochen Harnisch, Germany]	Noted, thanks.
8-187	8	5	36	6	2	This is a very long point - what is the main message here? Currently it is very unclear what the point is. [Katharine Law, France]	Taken into account, see comment 8-185.
8-188	8	5	36	6	2	This bullet should be greatly shortened. It is far too rambling and not to the point. [Robert Portmann, United States of America]	Taken into account, see comment 8-185.
8-189	8	5	36	6	2	Delete; doesnt belong in the chapter. [Stephen E Schwartz, United States of America]	Rejected, GTP belongs to Chapter 8 as decided in the material from the chapter outline.
8-190	8	5	36	6	8	The text expresses a strong preference for GTP, but I am not aware of new science that could justify that. To the contrary, I have seen recognition that both short-lived and long-lived climate forcing agents are important, while the use of (pulse-) GTP would result in almost ignoring short-lived forcers. If GTP is used to add forcers within emission inventories, for example with the current time horizon of 100 Y, forcings such as contrails or black carbon will appear very little - in the real world, they have an impact, even if only in the short therm, so it is not an option to look at the climate ONLY 100 Y after the pulse (even knowing that there is some inertia which creates a residual long-term impact). In addition, this ES does not follow the content of the chapter, which rightly explains that uncertainties that affect GWP also affect GTP, and there is additional uncertainty when calculating GTPs. Finally, this ES is in disagreement with the conclusions of the IPCC Expert Meeting on metrics. This paragraph needs a complete revision. [Philippe Marbaix, Belgium]	Taken into account by a more balance between GWP and GTP. It is stated that GTP has larger uncertainty than GWP, and we do not express a preference for either metric nor for a particular time horizon.
8-191	8	5	39	5	39	What is "radiative efficiency"? Rather than being more detailed, maybe this sentence could be omitted. [Jonathan Gregory, United Kingdom]	Taken into account, 'radiative efficiency' removed.
8-192	8	5	40			up through AR4 [David Stevenson, United Kingdom]	Taken into account, sentence rewritten.
8-193	8	5	41			Define GWP here, or in a footnote. Otherwise this is not clear to a non-specialist. [Nathan Gillett, Canada]	Taken into account, GWP is given in the glossary.
8-194	8	5	42	5	42	"alternative" should be "alternatives" [George Ban-Weiss, United States of America]	Taken into account, corrected as suggested.
8-195	8	5	42	5	42	Replace "alternative" with "alternatives". [Robert Waterland, United States of America]	Taken into account, see comment 8-194
8-196	8	5	42			The following wording is suggested: .. Several alternatives are available .. [Klaus Radunsky, Austria]	Taken into account, see comment 8-194
8-197	8	5	42			alternatives [David Stevenson, United Kingdom]	Taken into account, see comment 8-194
8-198	8	5	44	5	44	"highly subjective" or just "subjective" [Olivier Boucher, France]	Taken into account, sentence rewritten.
8-199	8	5	44	5	44	Calling it "highly subjective" is not helpful. It might imply that there is a "true answer" but it's very uncertain and is obtained by expert elicitation and gut-feeling, for instance. In fact there is not a true answer; it depends on the purpose. This is what you go on to explain. Hence I would replace this sentence with something like, "The results depend strongly on the time horizon adopted." [Jonathan Gregory, United Kingdom]	Taken into account, sentence rewritten.
8-200	8	5	44			Please, instead of the words "which is highly subjective" use the words "which depends on the objective and time horizon of the policy." The choice of the time horizon depends on the objectives of the policy and the time horizon of the policy. These are mentioned in 8-51 referring to the UNFCCC Article 2 which mentions both the level goal (it can be interpreted as long term concentration or temperature target, e.g. 50-100 years time scale)	Taken into account, sentence rewritten.

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						and rate goal (medium term warming rate target, 10-30 years scale) . [Ilkka Savolainen, Finland]	
8-201	8	5	46	5	46	Their impact does not become less. Their relative importance does. [Jonathan Gregory, United Kingdom]	Taken into account, sentence rewritten.
8-202	8	5	47	5	48	This sentence "One may …" is not informative enough for the Exec Summ. What is the "driver-response-impact chain"? I would omit this sentence. The next sentence expresses the ideas better, I feel. [Jonathan Gregory, United Kingdom]	Taken into account, sentence rewritten.
8-203	8	5	51			The following wording is suggested: .. .. metrics does not define the policies ... [Klaus Radunsky, Austria]	Editorial. Sentence revised for brevity and clarity.
8-204	8	5	52	6	1	It upsets the flow to return to talking about GWP at this point. It would be more logical to put this part at line 42. Then maybe you could start a new para altogether for GTP. [Jonathan Gregory, United Kingdom]	Taken into account, paragraph shortened and rewritten.
8-205	8	5	53	5	54	This statement on the uncertainty of the GWP seems far to vague. Certainly it cannot be universally true? [Robert Portmann, United States of America]	Taken into account, sentence removed.
8-206	8	5	54			GWP_100 of what? (Methane?) The uncertainty is not the same for all gases. [Nathan Gillett, Canada]	Taken into account, sentence removed.
8-207	8	5	54			The following wording is suggested: .. increases with the time horizon ... [Klaus Radunsky, Austria]	Taken into account, sentence removed.
8-208	8	5	54			Please, add a sentence like "The uncertainty of GTP is even larger due to uncertaity accumulation in the longer calculation chain." The text on the top of the page 8- 55 says that the uncertainty for GTP is even larger than that of GTP. In order to give a balanced picture on GWP/GTP issue this should be also stated here. [Ilkka Savolainen, Finland]	Taken into account, by stating that uncertainties are larger for GTP than GWP.
8-209	8	6	2	6	2	"value judgements" about what? I am not convinced that this sentence adds any further information to the para. [Jonathan Gregory, United Kingdom]	Taken into account, sentence deleted.
8-210	8	6	2		8	In discussion metrics of climate change, you should also includes metrics of climate change from historical emissions and this integrates over emissions and time and is similar to those proposed above. Effectively this is attribution to nations and belong in here. The major 'MATCH' papers are by den Elzen and Hoehne (Hoehne et al 2011 Climatic Change 106, 359-391; den Elzen etal 2005 Environ Sci Policy 8:614–636). [Michael Prather, United States of America]	Rejected, not sufficiently of scientific importance for the ES.
8-211	8	6	4	6	8	The authors should consider deleting this bullet. The results are from Figure 8.34. The text in the chapter does not justify the conclusion. Therefore, the authors should consider either deleting the bullet or adding text to the effect that in spite of the uncertainty and lack of assessment for the GTP values, the emissions associated with industry and power generation is so much larger that the conclusion is valid. [Government of United States of America]	Noted. We have expanded the discussion of the robustness of the method used in calculating the sector-based relative impacts, and indeed find that the conclusions support the points made in the revised version of this bullet.
8-212	8	6	4	6	8	There is obvious value in this sort of classification, but I am not used to it, so I would find it helpful to know what is left (the sectors which aren't important), after you've listed the important ones. [Jonathan Gregory, United Kingdom]	Rejected, this information is given in section 8.7.2 but is too detailed for the ES.
8-213	8	6	4	6	8	Please either delete this section or add some more substance. In it's current laconic form it is virtually incomprehensible and hardly policy relevant. [Jochen Harnisch, Germany]	Rejected. In the ES there is not space to provide depth beyond the summary conclusion. Further analysis is given in the underlying section of the chapter referred to in the bullet.
8-214	8	6	4	6	8	It is suggested to delete that paragraph because it is more appropriate to address such issues the report of Workling Group III. Furthermore this statement might be interpreted such that within other metrics (e.g. GWP) the order would be different. In order to avoid such misinterpretatiion it is suggested not to provide any such detail. [Klaus Radunsky, Austria]	Rejected. Emission metrics are a specific topic of ch 8 in WGI. In addition to defining the metrics, the chapter assesses their utility and this bullet is a summary highlight of one of the types of analysis thiese metrics are useful for.
8-215	8	6	4		8	Delete, doesn't belong in chapter. [Stephen E Schwartz, United States of America]	Rejected: See previous comment.
8-216	8	6				8.SM.2.3 Satellite Measurements: Satellite uncertainty: There is substantial uncertainty in satellite calibration. Recommend referring to NPL's TRUTHs project and the quantified potential to reduce satellite TSI etc. uncertainties by an order of magnitude. [David L. Hagen, United States of America]	Taken into account. Due to space limitations, we will comment on the TRUTHS project in the Supplementary Material section 8.SM2.1.

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8-217	8	6				8.SM 2.3 Recommend adding: “Fox et al (2011) quantify how the uncertainty in satellite TSI measurements could be improved by an order of magnitude by adding primary SI traceability on board. e.g. to reduce uncertainties from 3.60% for MODIS/VIIRS to 0.30% for TRUTHS. This would reduce by 67% to 75% the time required to achieve trend accuracy.” [David L. Hagen, United States of America]	Taken into account: As above (8-216)
8-218	8	6				8.SM 2.3 Source: Nigel Fox et al. ‚Accurate radiometry from space: an essential tool for climate studies. Phil. Trans. R. Soc. A 2011 369, 4028-4063 doi: 10.1098/rsta.2011.0246 [David L. Hagen, United States of America]	Taken into account: As above (8-216)
8-219	8	6				8.SM.2.3 Satellite Measurements Re: Satellite TSI ARIM Gap calibration: The current discussion strongly understates the published uncertainties in TSI trends and the potential solar contribution to global warming between the PMOD, IRMB, and ACRIM camps. Consequently, the present discussion strongly overstates the confidence in anthropogenic warming attribution. This was reviewed in detail by Scafetta (2011). I recommend addressing the alternative ACRIM total solar irradiance (TSI) composite (Wilson and Mordvinov, 2003.) [David L. Hagen, United States of America]	Taken into account. We read the Scafetta (2011) paper. However, Ball et al. (A&A, 541, A27, 2012) used continuum images and magnetograms in the SATIRE-S model (Krivova et al, 2011) to reconstruct TSI over cycles 21–23. To maximise independence from TSI composites (ACRIM,PMOD, RMIB), SORCE/TIM TSI data were used to fix the one free parameter of the model. The reconstruction supports the PMOD composite as being the best historical record of TSI observations, although on timescales of the solar rotation the RMIB composite provides somewhat better agreement, though all three composites are similar on this shorter time scale. Then based on the general independence of the model presented in this this paper, we consider the PMOD as the best TSI composite. Due to space limitations we cannot address the other composites.
8-220	8	6				8.SM.2.3 Recommend adding: “Scafetta and Wilson (2009) bridge the TSI gap between ACRIM1 and ACRIM2 satellites using Krivova et al's (2007) solar magnetic flux proxy. Their ACRIM TSI composite and mixed ACRIM and PMOD TSI composites demonstrate 0.037%/decade and 0.033%/decade increase in TSI between the solar activity minima of 1986 and 1996. Using three different TSI composites, Scafetta (2011) found that the TSI minimum in 1996 was 0.30 ± 0.40 W/m2 higher than the TSI minimum in 1986. The three major alternative TSI reconstructions imply that solar variations contributed 15%, 50% or 66% of the global warming since the 1970s.” [David L. Hagen, United States of America]	Taken into account: As above (8-219)
8-221	8	6				8.SM.2.3 Sources: Nicola Scafetta & Richard C. Wilson, ACRIM-gap and TSI trend issue resolved using a surface magnetic flux TSI proxy model, GEOPHYSICAL RESEARCH LETTERS, VOL. 36, L05701, 5 PP., 2009 doi:10.1029/2008GL036307 Nicola Scafetta, Total Solar Irradiance Satellite Composites and their Phenomenological Effect on Climate, Ch 12, in Evidence-Based Climate Science. 2011 Elsevier pp 289 – 316. DOI: 10.1016/B978-0-12-385956-3.10012-9 {See preprint posted at: http://people.duke.edu/~ns2002/pdf/Scafetta-easterbrook.pdf} [David L. Hagen, United States of America]	Taken into account: As above (8-219)
8-222	8	6				Table 8.SM.2: Radiative forcing (RF) by emitted components as shown in Figure 8.17c. This table is missing the non-insignificant contributions of O2 and N2. See: Höpfner, M. Milz, S. Buehler, J. Orphal, G. Stiller, The natural greenhouse effect of atmospheric oxygen (O2) and nitrogen (N2), Geophysical Research Letters, Vol. 39, L10706, doi:10.1029/2012GL051409, 2012 [David L. Hagen, United States of America]	Rejected, the table and figure shows the anthropogenic forcings.No scientific support to add O2 and N2 as anthropogenic components.
8-223	8	6				8.SM.2.3 Recommend adding: “Höpfner et al. (2012) 'found that on global average under clear-sky conditions the OLR is reduced due to O2 by 0.11 Wm2 and due to N2 by 0.17 Wm2. Together this amounts to 15% of the OLR-reduction caused by CH4 at present atmospheric concentrations. Over Antarctica the combined effect of O2 and N2 increases on average to about 38% of CH4 with single values reaching up to 80%.' These impacts will likely be incorporated into future models.” [David L. Hagen, United States of America]	Rejected, see 8-222

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8-224	8	6				8.SM.2.3 Comment: Comment: Höpfner use a Line By Line (LBL) modeling with global approximations. A more detailed Line By Line analysis is provided by Ferenc Miskolczi (2010). He uses actual radiosonde profiles and applies a Planck weighting to obtain a Planck weighted global optical depth for all the significant green house gases (except oxygen and nitrogen.) [David L. Hagen, United States of America]	Rejected, no of sufficient importance to include in the forcing chapter.
8-225	8	6				8.SM.2.3 Recommend adding: "Applying HARTCODE Line by Line (LBL) evaluation, Miskolczi (2010) obtained the average global Planck weighted optical depth of 1.867. The optical depth varied about +/- 1% and the atmospheric absorption by about +0.5%, -0.3%, primarily due to variations in H2O with small contributions from CO2 using NOAA Earth System Research Laboratory 61 year radiosonde data (1948–2008)." Source: "Ferenc Miskolczi (2010) The Stable Stationary Value of the Earth's Global Average Atmospheric Planck-weighted Greenhouse-Gas Optical Thickness, Energy & Environment Vol. 21, No. 4, 2010, p243 – 262." {Paper posted at http://www.eike-klima-energie.eu/uploads/media/EE_21-4_paradigm_shift_output_limited_3_Mb.pdf#page=85} [David L. Hagen, United States of America]	Rejected, no of sufficient importance to include in the forcing chapter.
8-226	8	7	1	11	48	Section 8.1 There is no specific mention of where emissions data come from. Are these estimated from something else? Are these provided by governments or governing bodies? Are the data sufficient? How could they be improved? Are there regions/countries for which data are not provided? [European Union]	Taken into account. We have added a comment on this in section 8.1.2.
8-227	8	7	1	11	48	Section 8.1 This section needs some description of the observations and/or modelling procedures that are used to estimate radiative forcing and forcing compounds in 1750. At present this feels a little like a black box. Transparency in this aspect will help the reader to understand the uncertainties surrounding this aspect of the climate system. [European Union]	Noted. These are different for each agent, so we believe it is better to have these discussed in each relevant chapter section than in the more general introduction to forcing.
8-228	8	7	1			Sect 8.1. The subdivisions of this section could be more helpful as guidance to the material. Within 8.1.1 there is a single subsection 8.1.1.1. Most of 8.1 is 8.1.1, and I think 8.1.2 could be merged into 8.1.1 as well. More helpful subsection divisions might include instantaneous RF, stratospherically adjusted RF, aerosol forcing and other tropospheric adjustments, definition of AF, methods for estimating AF, differences between RF and AF. The general point is worth making early in 8.1 that RF is a global concept, related to global-mean temperature change, and hence not to patterns of change in temperature or to other quantities. Further information about this, however, really belongs in other chapters that examine forced climate change, I would say. [Jonathan Gregory, United Kingdom]	Taken into account. We agree, and have reorganized sections 8.1.1 and 8.1.2. We have also added a comment that forcing is most applicable to global mean temperature.
8-229	8	7	3		10	Skip the philosophisizing; cut to the subject of the chapter. [Stephen E Schwartz, United States of America]	Rejected.This material is useful background in our opinon.
8-230	8	7	7	7	7	"metrics intermediate" should be "metrics that intermediate" [George Ban-Weiss, United States of America]	Rejected.We prefer the current wording.
8-231	8	7	8	11	48	This introductory section on RF is well written. It is very light on references and I approve of this as it is largely tutorial and makes the whole section much easier to read - I would resist adding too many more [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Noted. Thanks, we agree regarding references.
8-232	8	7	11	7	12	I feel that this is not the clearest way to express the dates. RF is evaluated by comparing one state with another, not by considering a period of time. Of course you choose 1750 and 2011 so that the RF measures the effects of the industrial period, but in fact you are giving RF at 2011 and at future times up to 2100 in each case with respect to 1750. You say this at lines 22-24, and maybe that sentence should be placed at this point. In that sentence you oppose two ideas, but they are not opposed, so this is confusing. The point is that showing RF at only one time would be an incomplete picture; you want to know it as a variable function of time, but it's always wrt a reference, and it does not relate to a period. [Jonathan Gregory, United Kingdom]	Taken into account. Agreed, we've clarified the dates and the later sentence.
8-233	8	7	11		12	**Refer to Annex II Tables for RF history and projections. Please get the correct values (based on your plots) to Annex II. [Michael Prather, United States of America]	Taken into account. Added.
8-234	8	7	13		14	Omit last sentence. [Stephen E Schwartz, United States of America]	Taken into account. This is useful to readers, we believe, so maintain, but we have clarified the relevance to forcing.

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8-235	8	7	16	7	24	This explanation of RF is not entirely clear. The authors should consider adding some wording that appears much later in this chapter (p. 52). Basically, RF describes the amount of energy added to (or subtracted from) the climate system by some change. [Government of United States of America]	Rejected. The more general suggested wording about energy added is not quite correct, as changes in energy can take place (e.g. evaporation changes due to irrigation) that are not radiant.
8-236	8	7	18	7	20	About the definition of Radiative Forcing: "RF is a measure of the net change in the energy balance of the 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]	Rejected. The definition already does state this. It seems this reviewer has simply repeated their FOD comments here.
8-237	8	7	18	9	24	The introduction rambles. Start with brief (2-3 sentences) on how the flow of radiation affects climate. Describe effect of albedo, shortwave scatterers and absorbers, and longwave absorbers. Then define radiative forcing and its relationship with surface temperature (I), and why radiative forcing provides an attractive metric. Then go through the different kinds of definitions of radiative forcing, making clear that AF represents a new standard way to report forcing in IPCC documents. [Loretta Mickley, United States of America]	Rejected. We appreciate this editorial suggestion, but prefer the current style as much of the information was put in specifically in response to prior review comments.
8-238	8	7	19	7	19	I agree that the RF is usually presented as an average over time (usually one year, based on the annual cycle of change in perturbation of concentration). However, this averaging is not a part of the definition and I think it is confusing to state it up front here. I suggest to mention this later in section 8.1.1.1 [Terje Berntsen, Norway]	Rejected. While we agree this is not part of the definition, this description was put in in response to multiple previous reviewer comment, and it is part of the standard practice, so we prefer to maintain the current wording here.
8-239	8	7	19	7	19	Would it be useful to say here that is it is usually (or maybe always, in this chapter) per square meter of the Earth's surface area i.e. it is a global mean. [Jonathan Gregory, United Kingdom]	Rejected. We present spatial maps of forcing (8.6), so the chapter does not always talk about global mean.
8-240	8	7	21			Section 8.1: Please change eventual to potential [Government of Poland]	Accepted. Done.
8-241	8	7	28	7	32	Are references needed for alternative definitions of RF or for superiority of tropopause vs TOA for predicting lower atmosphere effects? [Government of United States of America]	Rejected. We prefer to emphasize the newer findings, and this has been known for decades (see also comment 231 re: maintaining few references here).
8-242	8	7	28	7	32	Why is surface radiative forcing excluded? Many published studies of aerosol and cloud radiative forcing use surface radiative forcing. Since the real public concern is temperature changes near the surface where people live, and the surface radiative energy budget drives the near surface temperatures, it seems all the more important to include this discussion. [Government of United States of America]	Taken into account. The discussion here emphasizes the global mean. Surface forcing is important for localized responses, and hence appropriate for the discusison in section 8.6, where indeed it is discussed. We have revised to clarify the focus here on global mean.
8-243	8	7	28	7	49	The definitions of RF here are much more clear than definitions that use "net flux imbalance," because net already implies imbalance. The authors should, therefore, consider using this definition elsewhere. [Government of United States of America]	Noted. Thanks. We were unable to find any uses of the phrase 'net flux imbalance' however.
8-244	8	7	28		32	The text defines "radiative forcing" (RF) as the change in net total (solar + infrared) radiative flux at the tropopause". The problerm is that the tropopause is a constantly changing surface on a daily basis and much more so on a seasonal basis. Furthermore the tropopause is not even a continuous boundary separating the troposphere from the stratosphere (the tropopause jumps up at the transition from mid-latitudes to the tropics). It is understood that, for the purpose of comparing different RF estimates, the radiative flux differential is actually computed over a specified reference surface that espouses the shape of the physical tropopause as well as possible. It would be nice if the authors would spell out this convention in the main text or add a brief explanation in the glossary for the sake of facilitating readers' understanding of the actual computation. [Government of France]	Accepted. We agree, and have clarified that a climatological tropopause is typically used.
8-245	8	7	31	7	32	Is it right that the tropopause is better for unadjusted RF? With stratospheric adjustment the tropopause might be better, but for instantaneous forcing, is this also true? (Please excuse my ignorance!) A drawback with the tropopause definition is that there are also non-radiative heat fluxes at this level, and excluding them may make the RF inaccurate (Gregory et al 2004). [Jonathan Gregory, United Kingdom]	Taken into account. This was indeed the case, e.g. in Hansen et al, JGR, 1997, and is important for iRF while for RF the net flux is the same throughout the stratosphere so the level at which RF is diagnosed is

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							less important (provided it's the tropopause or higher). We've added a comment on the latter point to the next section on limitations of radiative forcing.
8-246	8	7	34	7	34	Is "counteract" the right word here? [Government of Canada]	Editorial. We believe this is ok as is.
8-247	8	7	36			"equilibrium". It is not equilibrium (requirement of detailed balance); it is steady state. Frequently called equilibrium. [Stephen E Schwartz, United States of America]	Editorial. We believe this is ok as is.
8-248	8	7	37	7	37	I think the correct IPCC term is just "climate sensitivity parameter" (not "equilibrium"). [Jonathan Gregory, United Kingdom]	Accepted, revised.
8-249	8	7	41	7	42	This is unclear. I suggest inserting "due to the imposed change alone" after "TOA". [Jonathan Gregory, United Kingdom]	Accepted, revised.
8-250	8	7	42			Section 8.1: Please change cleanly to clearly [Government of Poland]	Accepted, revised.
8-251	8	7	46	7	47	"also called stratosphericall-adjusted..". It's not clear if this is referring to RF or instantaneous RF. I would change it to "…radiative forcing RF (also called stratospherically-adjusted RF, as distinct from instantaeous RF)… [George Ban-Weiss, United States of America]	Accepted, revised.
8-252	8	7	46	7	52	Explain that what is called stratospherically-adjusted forcing was once called "adjusted forcing" in previous IPCC reports. Explain why the stratospheric response (and not other responses) was included. Quantify what is meant by "rapid" as in "rapid response." [Loretta Mickley, United States of America]	Rejected. In our opinion, a more detailed discription of the historical evolution of forcing terms would be distracting here (we already refer back to the AR4 definition of RF). Rapid is not used here as we are only discussing iRF and RF, which have no associated timescales.
8-253	8	7	49	7	49	"RF is" should "Adjusted RF is" for clarity here? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Editorial. As the previous sentence defines RF to include stratospheric-adjustment, we do not believe this is needed.
8-254	8	7				Section 8.1: A brief remark in section 8.1 to tie the sections in the chapter together would be helpful. It will be helpful to state that the chapter explains the steps on how one gets changes in concentration from emissions (section 8.2), gets radiative forcing from changes in concentrations for anthropogenic sources and land-use change (section 8.3), changes in forcing from natural source/activities (section 8.4), temporal evolution of radiative forcing past and future (section 8.5), geographical distribution of forcing (section 8.6). Finally one can bin the emissions according to sectors/regions, and aggregates the change in concentration/RF. Section 8.7 is about metrics for emissions. [Government of United States of America]	Accepted. We now describe the sections of chapter 8 at the end of the opening paragraph of 8.1.
8-255	8	8	1	9	24	Please quantify what is meant by "rapid" or "fast." There are many references to rapid or fast responses, but the reader does not know the meaning. [Loretta Mickley, United States of America]	Rejected. As stated in the text after the definition of AF that we use is provided, based on that definition "...there is no a priori timescale defined for adjustments to be rapid. The majority take place on timescales of seasons or less, but there is a spectrum of adjustment times. Etc." Thus we believe this is already addressed.
8-256	8	8	3	8	3	I think "necessarily" is unnecessary; they are not accurate indicators for some agents. [Jonathan Gregory, United Kingdom]	Accepted, revised.
8-257	8	8	4	8	4	Omit "eventual". This issue applies to all climate-response timescales, not only to equilibrium. [Jonathan Gregory, United Kingdom]	Accepted, revised.
8-258	8	8	12	8	14	The first two sentences are quite unclear to me. [Terje Berntsen, Norway]	Taken into account. The second sentence was unclear, and we have revised.
8-259	8	8	19	8	23	Shine et al. (GRL, 2003) is relevant here; they are particularly concerned with aerosol forcing. [Jonathan Gregory, United Kingdom]	Accepted, revised.

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8-260	8	8	19		27	Somewhere, perhaps here, there should be a note that other 'fast-response' indirect effects of aerosols on the chemical composition and hence RF are known (Martin et al. 2003 JGR 108, 4097; Bian et al 2003 JGR 108, 4242) but not included here. Indeed, do not all the chemical indirect effects fall under the AF category now ? (e.g. NOx ==> minus CH4, plus O3) This would simplify the discussion. [Michael Prather, United States of America]	Taken into account. This discussion is most relevant to the former section 8.1.3 on attribution to emissions, but we have added a sentence here on chemical indirect effects falling under RF and AF and a reference to the later section.
8-261	8	8	24	8	26	Gregory et al. (2004) did not use the term "fast feedback". In fact the word "fast" does not occur in that paper, and I agree that such effects should not be called "feedbacks". We wrote, "This leads us to suggest a practical distinction between a forcing and a feedback: Radiative forcing is a change in [the heat flux into the climate system] brought about by the presence of the forcing agent, developing much more rapidly than the climate can respond." Gregory and Webb (2008) decided to use the term "tropospheric adjustment" for the radiative effect of changes, especially in clouds, occurring on the rapid timescale. The distinction you make here between adjustment and feedback is the one we were arguing for as well, except that some local (not global) surface temperature changes can occur as part of the adjustment. [Jonathan Gregory, United Kingdom]	Accepted. Thank you. We have changed the text to give a correct example reference.
8-262	8	8	26	8	27	I feel that this sentence is not helpful, unless more detail is added and more specific refs to ch9 and ch12. Since this chapter is about forcing, I suggest omitting this sentence. [Jonathan Gregory, United Kingdom]	Accepted, revised.
8-263	8	8	29	8	29	Why do you write "attempt to"? Does this imply that they fail to do so? [Jonathan Gregory, United Kingdom]	Accepted, revised.
8-264	8	8	29	8	31	The regression method of Gregory et al (2004) and Gregory and Webb (2008) includes all rapid adjustments, including stratospheric adjustment, not just the ones listed. [Jonathan Gregory, United Kingdom]	Accepted. Revised this and subsequent paragraph with the description of the regression method discussed in these papers unified within the latter.
8-265	8	8	32	8	32	along with Andrews et al, you should probably cite Bala, G., Caldeira, K. and Nemani, R., 2010. Fast versus slow response in climate change: implications for the global hydrological cycle. Climate Dynamics, 35: 423-434. [George Ban-Weiss, United States of America]	Noted. This phrase was removed during revisions so the citation would no longer be relevant.
8-266	8	8	32	8	32	Andrews and Forster (2008), Andrews et al. (2009, on surface energy balance) and Andrews et al. (2011, Surv Geophys) are relevant to the separation of adjustment and feedback. Shine et al. (2003) proposed a version of radiative forcing including adjustments. [Jonathan Gregory, United Kingdom]	Accepted. Added the Andrews review, which includes references to the earlier studies. Shine et al added to previous paragraph.
8-267	8	8	33	8	35	I'm not sure that the lifetime effect warrents an "especially" (line 33) because there are other utilities such as including aerosol semi-direct effects. You could add as another sentence on line 35, "Other studies have demonstrated the utility of including rapid adjustments associated with atmospheric heating from black carbon (Hansen et al., 2005; Ban-Weiss et al. 2012). Citation is Ban-Weiss, G, Cao, L, Bala, G, Caldeira, K (2012) Dependence of climate forcing and response on the altitude of black carbon aerosols. Climate Dynamics. 38:897-911. [George Ban-Weiss, United States of America]	Accepted. Revised to note that applicable to additional effects, specifically noting lifetime and semi-direct and adding the suggested references.
8-268	8	8	35	8	35	Maybe here or elsewhere you could explain that Hansen's semi-direct forcing is a kind of tropospheric adjustment, like indirect aerosol forcings are. [Jonathan Gregory, United Kingdom]	Accepted, revised.
8-269	8	8	37	8	40	"all or a portion of the surface unchanged": This is could be confusing: what is the definition employed here? (all unchanged or just fixed SSTs?). The situation was clear in AR4 (fig 2.2). To introduce a new RF definition, it is important to keep the same level of clarity and simplicity of interpretation. In particular, you call your metric "Adjusted Forcing", but it appears to be what Hansen (2005) referred to as "Fixed SST Forcing, Fs", while many readers could think that "Adjusted Forcing" is what it is in Hansen's paper, ie. the traditional RF with troposphere adjustement. [Philippe Marbaix, Belgium]	Taken into account. We have clarified that the intial part of this paragraph refers to the general concept of AF, while later we describe the specific definition we will use. This is also presented in the new box on forcing definitions.
8-270	8	8	37	8	56	I think it would be helpful to include an explicit definition of AF, as applied in the chapter, since it has not been previously given such precedence in IPCC assessments, and it is key here. On line 51 the text states that there is a low bias since the land temperature response is included. But the Hansen (2005) definition of AF, referenced here, includes a correction for this. Is this correction included here or not? [Nathan Gillett, Canada]	Accepted. Clarified in text, also in new Box giving definition.
8-271	8	8	37	8	56	Clarify further the fixed-SST methodology adopted here. Are pre-industrial SSTs or present-day SSTs to be used? Climatological or interannually varying? What are the implications of such choices for the values computed? [Larry Horowitz, United States of America]	Taken into account. We have clarified that climatological SSTs are used. It is not possible to give a time period applicable to all cases as, for example, preindustrial to present-day calculations have typically

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							used preindustrial SSTs while future calculations have typically used present-day SSTs.
8-272	8	8	39	8	39	I think it should be "net downward radiative flux", not "irradiance". [Jonathan Gregory, United Kingdom]	Accepted, revised.
8-273	8	8	39	8	40	I think it is unnecessary to mention "land albedo" explicitly, since that is actually an aspect of surface conditions. Therefore I would suggest "after allowing atmospheric temperatures, water vapour and clouds to adjust, but with some or all of surface conditions unchanged." [Jonathan Gregory, United Kingdom]	Accepted, revised.
8-274	8	8	40	8	40	Again, "surface conditions unchanged" could be more specific. Change to "surface temperatures over a portion of the globe?" [George Ban-Weiss, United States of America]	Rejected. This would be incomplete, as many things can change in addition to surface temperature (e.g. albedo).
8-275	8	8	41	8	42	In describing this method, I think that it is important to say (a) that the forcing agent is included (b) that the results are obtained by diagnosing the system after it has achieved a steady state, not during the transient. [Jonathan Gregory, United Kingdom]	Accepted. Added that the response is to steady state. Added that both forcing agent and response to the agent are included in new box on forcing definition.
8-276	8	8	44	8	44	The word "implied" seems unnecessary to me. [Jonathan Gregory, United Kingdom]	Accepted, revised.
8-277	8	8	45	8	46	What is the basis for this comparison? Are there references? The % accuracy depends on the magnitude of the forcing and the size of the internal variability. Is 10% a standard error, or a 5-95% confidence interval, or some other statistic? [Jonathan Gregory, United Kingdom]	Accepted. Added reference to Andrews et al (2012) for the uncertainty on the regression forcing, added the imposed forcing in that analysis, and that the 10% is a 95% confidence interval.
8-278	8	8	45			Doesn't the uncertainty depend on the magnitude of the forcing? [Nathan Gillett, Canada]	Accepted, revised to give forcing.
8-279	8	8	47	8	48	I can't find a statement like this in Andrews et al. (2012). The "Hence" starting the next sentence seems to imply that a smaller spread across models means the answer is more reliable, but why would this be so? The two methods are actually not diagnosing exactly the same quantity. [Jonathan Gregory, United Kingdom]	Noted. This is not explicitly stated in Andrews et al, but can be calculated from the values presented in their table of forcings.
8-280	8	8	47	8	49	What SSTs are used for calculating AF in AR5? Present-day or 1750s? [Loretta Mickley, United States of America]	Noted. It is not possible to give a time period applicable to all cases as, for example, preindustrial to present-day calculations have typically used preindustrial SSTs while future calculations have typically used present-day SSTs.
8-281	8	8	49	8	49	Another practical consideration, which favours the regression method, is that it is easy to do the experiment with fixed forcing in a coupled model, and indeed it might be done anyway, whereas the fixed-SST technique requires a special experiment with prescribed oceanic boundary conditions. [Jonathan Gregory, United Kingdom]	Noted. We agree, but as most groups are set up to do AMIP style prescribed SST experiments we do not feel this is a major technical issue.
8-282	8	8	53	8	53	I think another problem with AF is that it uses fixed SSTs and this makes the surface energy balance response to a forcing agent by definition an artificial one. Thus, you will have adjustments in surface energy terms (i.e. latent heat, sensible heat etc.) due to a forcing that may be different than those that would occur if SSTs were free to change. Therefore, some of the 'rapid adjustments' that are reliant on these surface terms may be different if the SSTs were free to change. This means in turn that the AF may not be a true representation of the forcing seen in a coupled system. [David Paynter, United States of America]	Rejected. While we agree that surface adjustments could be somewhat different if SSTs responded, those would be feedbacks related to oceanic changes that we are explicitly excluding here. The key point is that, as the cited references demonstrate, AF is a good predictor of the global mean response.
8-283	8	8	54	8	56	I do not find this statement in Andrews et al. (2012). I reckon the difference between the methods in the model mean is 2%, from their Table 1. [Jonathan Gregory, United Kingdom]	Noted. This statement is based on our evaluation of Table 1 results rather a statement in the Andrews et al paper. While the mean across all models does indeed differ by ~2%, we have restricted our sample to the subset of models for which forcing values using both techniques are available to avoid sample bias, and that yields the 7% we quote.
8-284	8	8	55	8	56	"Despite the low bias…calculation".You're saying that despite a low bias, something is lower? This seems oddly worded [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Rephrased.

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8-285	8	8	55			If the uncertainty for the estimate of forcing for a single simulation is 10%, then the uncertainty in the mean forcing estimated from N runs is 10%/sqrt(N). So, for say, 25 models, the uncertainty should be 2%. This would be low enough that a 7% low bias in one method compared to the other could be robustly identified. [Nathan Gillett, Canada]	Noted. The 10% uncertainty is due to noise in a single model integration. However, there is also spread between the models as they include different radiative transfer calculations and rapid adjustments. The N models can differ by as much as 33%. Hence the uncertainty in the multi-model mean is not as low as suggested here.
8-286	8	8	56	8	56	You could add, "Alternatively, another study found that the regression technique was a more consistent predictor of temperature change than the fixed-SST technique for black carbon at different altitudes (Ban-Weiss et al, 2012, see Fig 3). Citation is Ban-Weiss, G, Cao, L, Bala, G, Caldeira, K (2012) Dependence of climate forcing and response on the altitude of black carbon aerosols. Climate Dynamics. 38:897-911. [George Ban-Weiss, United States of America]	Taken into account. This seems less appropriate to the point being made here, but we have added a reference to this study later in this section (see response to similar comment #295).
8-287	8	9	1	9	5	The first sentence is exactly the distinction made by Gregory et al (2004) and Gregory and Webb (2008), in similar words to ours. This definition encompasses radiative forcing estimated by both the fixed-SST and the regression method. There are differences, as already discussed, but they are quite small and the concept is the same. I can see reasons for choosing one method or the other in particular circumstances but I do not understand why you prefer the fixed-SST method as a general principle, either practically or conceptually. I wonder how you reach this assessment of the literature, since you don't give references in support of the conclusion. The distinction between forcing and feedback is one of timescale. As you say, in the fixed-SST method the timescale is not obvious, and you define the forcing by arbitrarily fixing some parts of the system. On the other hand the regression method suggests that "rapid" means by comparison with the rate of forced climate change. It also suggests that "rapid" means effects that happen on timescales much less than a year, because if you plot annual means for the first couple of decades you see a straight line. Thus, the regression method in some senses suggests a less arbitrary definition of forcing. [Jonathan Gregory, United Kingdom]	Accepted. We have clarified that the choice of which technique to use was made largely on the basis of availability of model results rather than the technical merits of the two main choices.
8-288	8	9	6			How come snow cover takes so long to respond? Snow cover (unless it is on glacier) is seasonal, so it comes and goes each year. I would expect that it would respond to forcing on a timescale of a year or less. Do you mean ice sheets and glaciers? [Nathan Gillett, Canada]	Accepted. Much snow is seasonal, but not all. We've revised to refer to both snow and ice.
8-289	8	9	9	9	10	The RF is identical if computed at the tropopause instead of the TOA (since stratospheric temperatures are allowed to relax). The sentence implies this is special for the AF, but not the RF. [Robert Portmann, United States of America]	Accepted, revised.
8-290	8	9	9	9	10	For the AF "identical" is too strong a word, as there is no constraint that delta Q in the stratosphere is zero and fast adjustments can change the stratospheric heat budget slightly (e.g., via ozone, water vapor, or dynamical changes). I would suggest "nearly identical". For the RF, the tropopause and TOA flux changes are identical. [Robert Portmann, United States of America]	Accepted, revised.
8-291	8	9	12			Omit "Using the RF concept"; dangling participle; the climate sensitivity parameter doesnt use anything. [Stephen E Schwartz, United States of America]	Accepted, revised.
8-292	8	9	13	9	13	Change to: The response to RF from a particular agent relative to the response to RF from CO2………… [Terje Berntsen, Norway]	Accepted, revised.
8-293	8	9	15	9	15	This point was demonstrated nicely by Shine et al. (2003). [Jonathan Gregory, United Kingdom]	Noted. We agree, but have already included this and several other points when first motivating the use of AF.
8-294	8	9	15	9	18	This doesn't make sense to me. Surely the actual perturbation to the physical climate is a change in BC. RF and AF are just two ways of diagnosing the radiative response. What does it mean to say that there are large differences in the impact of BC RF as a function of altitude but a uniform response to BC AF? [Nathan Gillett, Canada]	Accepted, revised.
8-295	8	9	18	9	18	Along with Hansen et al 2005, you could site Ban-Weiss et al 2012, which also found nearly uniform AF for black carbon at different altitudes. Citation is Ban-Weiss, G, Cao, L, Bala, G, Caldeira, K (2012) Dependence of climate forcing and response on the altitude of black carbon aerosols. Climate Dynamics. 38:897-911.	Accepted, revised.

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						[George Ban-Weiss, United States of America]	
8-296	8	9	18	9	18	Ming et al. (2010) should be cited. [Yi Ming, United States of America]	Accepted, revised.
8-297	8	9	19			Maybe use "hereinafter" ? Some one may want to use it again - though they shouldn't!! [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted, revised.
8-298	8	9	20	9	20	Remove the hanging "however" at the end of the line. [Robert Portmann, United States of America]	Accepted, revised.
8-299	8	9	20			Sentence difficult to understand [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted, revised.
8-300	8	9	24			Because the phrase "in some cases" refers only to providing a more useful metric, this would be more clear as "...to allow quantification of more complex forcing agents and, in some cases, to provide a more useful metric than RF. This also maintains the parallel construction between "to allow" and "to provide." The authors should consider revising the text accordingly. [Government of United States of America]	Accepted, revised.
8-301	8	9	26			Sect 8.1.2. Most of this material could be merged into the previous paragraphs. The first couple of sentences are about technical disadvantages of either method of AF compared with RF. Material follows about scientific differences between AF and RF. Both of these subjects have already come up in the foregoing, and it would be more logical to put this material at those points. The following paragraphs are about response and feedback, not about forcing, so I tend to think they could be omitted from ch8; perhaps they belong in another chapter. The last sentence of 8.1.2, about non-radiative forcings, is a new point; perhaps it could come earlier in 8.1. [Jonathan Gregory, United Kingdom]	Editorial. We believe that merging this material with the previous section would make the description of the forcing concepts very difficult to read - one would get lost in the caveats and limitations and miss the main points emphasized there. For the forcing/feedbacks, we have shortened, but we do point out that the point is to explain what forcing is and is not useful for, which we believe is highly relevant to this chapter.
8-302	8	9	28	9	29	The regression method to estimate AF does not always require dedicated experiments, since the experiment with instantaneous forcing imposed might have been done in any case. [Jonathan Gregory, United Kingdom]	Noted. Any experiment might in principal be done anyway, and this refers to dedicated runs needed that are not say standard historical transients that could be used for iRF.
8-303	8	9	28	9	30	A practical point perhaps, but I think for most modelling groups it is much easier to run a Hansen experitment for 50 years with a standard version of the model, than to adapt the model with two calls to the radiation etc in order to diagnose the standard RF. Diagnosing AF may take more computer time, but it takes less human time. [Nathan Gillett, Canada]	Noted. This may be true, but it depends on how many groups already have the capability to diagnose RF. We think most do, but don't feel it's that important or useful to speculate on this in the text.
8-304	8	9	28	9	51	Make clear that the larger intermodel variability in AF is due to the uncertainty in climate feedbacks. [Loretta Mickley, United States of America]	Accepted, revised.
8-305	8	9	29	9	32	A comment for the whole chapter really but this is it's fist occurance. Model is used very often in the text as here without specifiying what sort of model. Here you mean GCM or AOGCM. Elsewhere you mean CTM or LBL radiation code. Not everywhere, but here and elsewhere I think you need t specify.Sometimes you may need to specify when Earth system feedbacks are included [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted, revised in several relevant places.
8-306	8	9	30	9	30	Change "meteorological variability" to "effects of climate variability" to be more general [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted, revised.
8-307	8	9	32			I would use simulation here and elsewhere intead of experiement and say what the simulation is of - model again not clear in text [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted. Clairifed that these are radiative transfer calculations, not really a 'model' at all.
8-308	8	9	35	9	35	But they can be very different for absorbing aerosols (Shine et al., 2003). [Jonathan Gregory, United Kingdom]	Accepted. This is discussed later in this same paragraph.
8-309	8	9	55	10	20	Chapter 2 discusses methane in the context of long-lived greenhouse gases. Box 8.1 provides a discussion on grouping forcing compounds by common properties. The terminology is changed from ‘long-lived’ to ‘well-mixed’ and Methane is classified as both ‘well-mixed’ and ‘near-term’. This change in terminology from what is used in chapter 2 is unhelpful. It is also unhelpful not to have methane clearly defined. If near term is to be defined as “those compounds whose impact on climate occurs primarily within the first decade after their	Accepted. We have discussed with chapter 2 and agreed to use the common terminology of WMGHG. The box on grouping forcers by common properties says explicitly that methane sits in both groups, and why.

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						emission" this definition should be included in the glossary. This definition will likely get used in international negotiations. A clear explanation of where methane sits would be useful. [Government of Australia]	
8-310	8	9	55	10	20	The discussion on grouping into WMGHG and NTCF in Box 8.1 is confusing. Each concept is useful when taken by itself. However, discussing them together may not be the best possible presentation. In the context of the chapter, NTCFs are species whose concentration depends on emissions from the immediate past 10 years, but not on emissions older than that. Why did the authors pick such a large cut off value. WMGHGs are gases that are well-mixed in the troposphere so that there is a simple way to relate past emissions to present burden, and then to global averaged forcing. In addition, it's unclear why section 8.7.1 is referring to Box 8.1 in the context of the "cause-effect chain". There is a lack of clarity in the sentence on line 47 -51 on p 8-9: "Hence while RF and AF are generally quite similar for well-mixed greenhouse gases (WMGHGs), AF typically provides a more useful indication of climate response for near-term climate forcers (NTCFs)". Each part of the two parts sentence is true, but there is no reason to put them together in a sentence. The authors should consider revising accordingly Finally, one should be cognitive of the possibility that the daughter products could have longer lifetime than the parent. Is that why the authors use the term "whose impact occurs primarily within the first decade"? [Government of United States of America]	Taken into account. We think it is important to have these together, especially because of methane being in both groups. The decadal timescale is reflective of a clear difference between the timescales associated with shorter-lived compounds and with the longer-lives ones rather than a particular value of 10 years. A slightly shorter or longer value would not change the grouping other than for some of the minor halocarbons. The rationale for the two groupings is already clearly defined in our opinion (see e.g. comment 315) We have clarified the sentence referred to in the middle of the comment. For the last comment, yes, we use impact since we are aware that the impact can be indirect.
8-311	8	9	55	10	20	Concept is good; why not "short lived" rather than "near term"? [Stephen E Schwartz, United States of America]	Rejected. Many scientists objected that methane is not short-lived, hence we deliberately avoided that terminology.
8-312	8	9	55			Chapter 2 is using LLGHGs - I don't mind which is used but I suggest the report is homogonised [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted. We have discussed with chapter 2 and agreed to use the common terminology of WMGHG.
8-313	8	10	2		3	Are NTCFs the same as shortlived climate forcers? The latter name is the one I am more familiar with. Perhaps at least either say that this is the same as a shortlived climate forcer, or describe how they are different. [Nathan Gillett, Canada]	Accepted. We have added that NTCFs have sometimes been called SLCFs or SLCPs.
8-314	8	10	3	10	14	The NTCF acronym is barely used (eg. Not on line 14). It is used in the metric section but by the time I got to it I had forgotton what it stands for [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted. Now used througout chapter when appropriate.
8-315	8	10	5		20	Great job on clarifying the terms. Nice box. [Michael Prather, United States of America]	Noted. Thank you.
8-316	8	10	5			Please see above comments about this use of the descriptor "well-mixed". [Adrian Simmons, United Kingdom]	Accepted. We have ensured that the box describes both the reason why the degree of mixing is important and notes that this applied primarily to the troposphere.
8-317	8	10	7	10	7	should be small hemispheric gradients? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted, revised.
8-318	8	10	10	10	11	"but the physical property…mixing within the atmosphere". Well, it's both- they're well mixed as their atmospheric lifetimes are much greater than atmospheric mixing timescales- why not say this explicitly? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted, revised.
8-319	8	10	14	10	20	I don't really get this, I'm afraid. Aren't you simply talking about species with short lifetimes? Because they are short-lived, they mostly don't have time to be well-mixed. Methane survives long enough to be well-mixed and has distributed sources, but still it is a fairly short-lived species. Wouldn't it be simpler to call them "short-lived forcing agents" than to give them an alternative and essentially equivalent name of "near-term climate forcers"? Short-lived species obviously only have RF on short timescales. All RF, however, has an effect which lasts as long as the memory of the climate system; the memory of surface climate is much shorter than that of the deep ocean. [Jonathan Gregory, United Kingdom]	Rejected. Many scientists objected that methane is not short-lived, hence we deliberately avoided that terminology.
8-320	8	10	21		48	To be sure the concentrations of the forcing agents, which are what govern the forcing, are consequences of emissions of the substances or precursors. We all know that. What you are advancing is the utility of expressing forcing per emission vs forcing per concentration or in addition to concentration and suggesting	Rejected. Note that this refers to p 11, not p 10. We believe this discussion presents an important concept as emission-based forcings are given much more

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						that this is a useful concept. Don't belabor the point. Its pretty simple. However in some cases the relation between emissions and concentrations is complicated; that is why we have chem transport models. So in some cases the concept isnt all that useful. period. full stop. [Stephen E Schwartz, United States of America]	prominence in AR5 than in previous assessments, so we feel it's necessary to discuss these issues more fully.
8-321	8	10	24	9	5	You are confounding short term responses with geographical inhomogeneity. Suggest strike any discussion of geog inhomogeneity and focus on defintion of adjusted forcing. [Stephen E Schwartz, United States of America]	Rejected. The point being made here is that forcing is quite useful for estimating global mean, but not so much fo regional, which we feel is important for readers to be reminded of when seeing all the forcing values we give in the chapter.
8-322	8	10	24	10	34	I agree with the first sentence: global-mean AF does not tell us about the patterns of climate change. However this is true for all forcings, not just inhomogeneous ones. The pattern of response to CO2 isn't like the pattern of forcing, for example. The pattern of response is, to first order, independent of the pattern of forcing, and that is one reason why the separation of forcing and feedback is practically useful. The rest of this para is about climate response and feedback, and hence not the subject of ch8. The seasonal and diurnal cycles, mentioned in the last para, are included in the methods of diagnosing forcing anyway, so this point seems unnecessary to me. [Jonathan Gregory, United Kingdom]	Taken into account. Indeed, this is why we wrote 'especially', but we have further clarified this. Our point is to give readers a sense of the uses and limitations of forcing in this section prior to presenting those forcings in the remainder of the chapter. We have shortened, and in particular deleted the lines about diurnal and seasonal.
8-323	8	10	36	10	41	For AF, we are considering timescales on which the heat capacity of the atmosphere is negligible. Hence the heat flux perturbation at the TOA and the surface (not purely radiative) must be equal (relevant papers are Shine et al., 2003, Gregory and Webb, 2008, Andrews et al., 2009, and I expect there are others). [Jonathan Gregory, United Kingdom]	Accepted. We have changed this to refer to 'solar radiation' rather than total - thank you for pointing out that our previous statement was flawed.
8-324	8	10	38	10	38	Is that not also true for longwave forcing agents. For example the flux reaching the surface from a doubling of CO¬2 is smaller than that at the tropopause. This value can also be highly influenced by the spectral overlap with water vapor. So I think it is worth making it clear that surface forcing for both shortwave and longwave agents could well be important and require further investigation. [David Paynter, United States of America]	Accepted. The cited papers discuss the shortwave fluxes, so we have clarified that this particular discussion refers only to those.
8-325	8	10	41	10	42	The "atmospheric RF" is ambiguous here. The phrase used in Andrews et al. 2010 is "atmospheric component of the RF". The key result is that this component is different for every gas, as opposed to the part dependent on the global mean temperature changes. I would recommend adding this to the discussion. [Robert Portmann, United States of America]	Accepted. Rephrased to clarify.
8-326	8	10	41	10	52	These sentences are concerned with response and feedback, not forcing, and are thus not the subject of ch8. [Jonathan Gregory, United Kingdom]	Rejected. This discussion is needed to support the conclusions in lines 50-52 of the SOD about what forcing is and is not useful for (see also reply to 321 and 322).
8-327	8	10	41			What does 'atmospheric RF' mean here? Is this the 'atmospheric heating' defined on the previous line, or is this RF? [Nathan Gillett, Canada]	Accepted. Rephrased to clarify.
8-328	8	10	42	10	42	Along with Andrews and Ming et al 2010, please cite Ban-Weiss et al 2012, which related precipitation changes seperately to the atmospheric RF and slower response to global temperature change for black carbon at different altitudes. Ban-Weiss, G, Cao, L, Bala, G, Caldeira, K (2012) Dependence of climate forcing and response on the altitude of black carbon aerosols. Climate Dynamics. 38:897-911. [George Ban-Weiss, United States of America]	Accepted, revised.
8-329	8	10	46	10	55	I find that this paragraph to be slightly unclear in its criticism of RF. It seems to be stating that using globally averaged RF in conjunction with a climate sensitivity to obtain a globally averaged temperature change is a limitation of radiative forcing, because it tells us nothing about regional climate change etc. However, this is just a limiting factor of using globally averaged RF not of RF itself. The fact that a regionally calculated value of RF may tell us very little about the regional temperature response is a different issue. I think it would be clearer just to say that most of the RF values presented in this chapter are globally averaged which can only really tell us about global temperature change. Then state that regional RF value may not tell us much about the local temperature/precip response due to atmospheric/oceanic transport of energy away from the region of the forcing. [David Paynter, United States of America]	Noted. This paragraph does say that RF is quite useful for understanding global mean temperature, so we do feel it's a criticism of RF but rather this section is summarizing the previous paragraphs to give a perspective on what RF is and is not useful for.
8-330	8	10	46			Is "yield" really the right word herE? [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted, revised.

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8-331	8	10	55	8	55	there are many earlier studies than Andrews et al 2012b that show CO2 Physiological Forcing. E.g. Cao L, Bala B, Caldeira K, Nemani R, Ban-Weiss GA, (2010) Importance of Carbon Dioxide Physiological Forcing to Future Climate Change. Proceedings of the National Academy of Sciences. 107, 9513-9518. (and references therein) [George Ban-Weiss, United States of America]	Taken into account. True, but our intention here was merely to give an example and not review the literature. We've added 'e.g.' before the citation to make that clear.
8-332	8	10	55	10	55	Also Doutriaux-Boucher et al (GRL, 2009). [Jonathan Gregory, United Kingdom]	Taken into account. True, but our intention here was merely to give an example and not review the literature. We've added 'e.g.' before the citation to make that clear.
8-333	8	11	1			Fig 8.1 is the fixed ground T cartoon needed - it is never discussed as far as I can tell? [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Noted. The fixed ground T is useful as that's illustrative of the regression method, which is used in ch 7 & 8.
8-334	8	11	4	11	5	Change dT0 and dTs to ΔT0 and ΔTs to make consistent with what is shown in Figure 8.1. [Lazaros Oreopoulos, United States of America]	Accepted, revised.
8-335	8	11	9			What sort of model? [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Noted. Any kind of model can be used here, so we've left this case generic.
8-336	8	11	10	11	10	Suggest replacing 'present' with 'a later date' to be more precise. Although it is in common usage, 'present' depends on the date of the report or study, and is often actually based on data that ends even earlier. [Philip Cameron-Smith, U.S.A.]	Accepted, revised.
8-337	8	11	16	11	17	This sentence is false. Even if it can be proven that emissions cause significant warming that would not mean that other plausible causes, especially poorly-modelled naturasl forces, should be rejected. Of course if natural forces are to blame then making policies to limit them would be rather futile. [John McLean, Australia]	Rejected. Analyses of all known substantial forcing (see section 8.5) shows that changes in composition are in fact by far the largest contributors to forcing.
8-338	8	11	18			Reference to the MATCH groups work on analysis of attributing RF and climate change to historical national emissions (Brazil Proposal to UNFCCC) would be appropriate here (Hoehne et al 2011 Climatic Change 106, 359-391; den Elzen etal 2005 Environ Sci Policy 8:614–636) [Michael Prather, United States of America]	Accepted, revised.
8-339	8	11	21	11	40	This paragraph is rather long and does not refer to the literature. I wonder therefore if it could be shortened and focussed better. It might be relevant to refer to 6.4.1, in which the related issue of Earth system feedback is discussed. [Jonathan Gregory, United Kingdom]	Editorial. This paragraph is long because we believe these are important concepts, especially as the greater emphasis on them is rather new, but we have shortened this section. The references to literature are given later in the chapter when results from specific calculations are provided rather than here in the more general introduction. Reference to 6.4.1, added.
8-340	8	11	21			Again what sort of model ? [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted, revised.
8-341	8	11	37	11	38	This is not correct. There are a number of studies e.g. for BC that do not include atmospheric chemistry but a applies a bulk (fixed) aging time of BC. [Terje Berntsen, Norway]	Noted. This sentence was deleted during revisions.
8-342	8	11	37	11	38	More information is needed here on emission-based forcing as it relates to ozone and aerosols. With the inclusion of more emission-driven models in CMIP5, it is becoming more natural to estimate AF resulting from (historical or future) emissions of a particular species (e.g., SO2). This topic is somewhat covered in Section 8.5.1 (Figure 8.17c), but it should be introduced here and discussed more fully elsewhere in the Chapter (not just in the metrics section, 8.7). [Larry Horowitz, United States of America]	Taken into account. Most of these are specific details to ozone and aerosols, so we feel are more appropriate to later sections, but we have added some information here (see also reply to next comment).
8-343	8	11	37	11	38	Clarify how climate-change induced feedbacks on aerosol concentration are treated in the concentration-based AF framework considered here. Are these effects considered forcings or feedbacks? [Larry Horowitz, United States of America]	Accepted. Clarified that climate changes should not be included for total aerosol or ozone AF, but sometimes are.
8-344	8	11	55	12	1	Temperature should be mentioned as a factor, as reactions can be temperature-dependent, and temperature is a key factor in formation of PSCs and the consequent occurrence of heterogeneous reactions. [Adrian Simmons, United Kingdom]	Rejected, not of sufficient importance for the general description in the introduction.

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8-345	8	11				Section 8.2 All of these summaries are global in scope. Is there anything that can be said at the regional level at least in terms of emission? [European Union]	Taken into account, information will be included in the supplement
8-346	8	11				Section 8.1.3: To more clearly reflect the content of the section, the authors should consider changing the title to "Calculation of RF from concentration or emission changes" [Government of United States of America]	Accepted, revised.
8-347	8	11				Section 8.2: Much of this section is repeated in Section 8.3 (e.g. the tropospheric ozone budget).The authors should, therefore, consider combining the two sections, at least in terms of discussions with respect to specific forcing agents.The most important part of this section is to provide background on the ACCMIP excercise. Details on specific forcing agents should be discussed along with the present day RFs (i.e. Sec 8.3). [Government of United States of America]	Rejected: after discussing with the CLAs, we have decided to keep the sections 8.2 and 8.3. We will however reduce the amount of overlap to a minimum, keeping the chemistry discussion to 8.2 and RF to 8.3
8-348	8	11				Section 8.2 Many of the sections pointed to here are incorrect I think - they reference to 2.4.1.1.? But they should be 2.2.1.1.? [Kate Willett, United Kingdom]	Editorial: This was due to the fact that section was rearranged their sections witouht our knowledge. This will be fixed in the final version.
8-349	8	12	1	12	2	change "a strongly interacting…system" to "characterised by many interactions, with variability on many timescales"? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Taken into account, by rewriting of the sentence
8-350	8	12	5	12	7	“This section will assess updates in understanding of processes, modeling, and observations since AR4 Section 2.3, on all reactive species contributing to RF.” What is missing in this chapter is a link of the measurements of the trends in tropospheric ozone that are reported in WGI Chapter 2 (Figure 2.7 and Table 2.A.2) to the predictions of the CTM discussed in Chapter 8.2. While the time horizon of the measurements is limited nevertheless, the trends that have been observed are significant and distinct by region. They can provide an important test for the current CTM models and can lead to a better understanding of their strengths and weaknesses as they illustrate the response of tropospheric ozone to regionally changing anthropogenic emissions. Such comparisons with models that are part of ACCMIP have been presented in recent publications (for example: Lamarque et al., acp-10-7017-2010, Lamarque et al., Climatic Change 109, 191, 2011). [Michael Trainer, United States of America]	Accepted: We will mention the link to the trends.
8-351	8	12	5	12	7	“This section will assess updates in understanding of processes, modeling, and observations since AR4 Section 2.3, on all reactive species contributing to RF.” What is missing in this chapter is a link of the measurements of the trends in tropospheric ozone that are reported in WGI Chapter 2 (Figure 2.7 and Table 2.A.2) to the predictions of the CTM discussed in Chapter 8.2. While the time horizon of the measurements is limited nevertheless, the trends that have been observed are significant and distinct by region. They can provide an important test for the current CTM models and can lead to a better understanding of their strengths and weaknesses as they illustrate the response of tropospheric ozone to regionally changing anthropogenic emissions. Such comparisons with models that are part of ACCMIP have been presented in recent publications (for example: Lamarque et al., acp-10-7017-2010, Lamarque et al., Climatic Change 109, 191, 2011). [Michael Trainer, United States of America]	Accepted: same as above (comment 8-350)
8-352	8	12	6			"all"; you can never do all. you can do key, you can do important, but you can never do all. [Stephen E Schwartz, United States of America]	Editorial: sentence corrected.
8-353	8	12	9	12	15	Need a definition or short introduction for CMIP5 and also perhaps for RCPs. [Robert Waterland, United States of America]	Rejected: This is not the place for this Introduction. This is done in Chapter 1
8-354	8	12	9			I had issues with the tone of this section - it read as if IPCC authors were doing the research. Table 8.1 is the sort of thing you would have in a research paper not an IPCC report. Is it needed? Statements on line 43 such has how invitations occurred, should be of no relevance to an IPCC report [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted: We have removed Table 1 as it is now published in the literature and can be referenced.
8-355	8	12	9			Section 8.2.2. The motivation behind this section, and the conclusions coming out of it are not clear. Is this supposed to be background on ACCMIP results described in the following sections? In particular, ln 17-21 immediately goes into detail on ACCMIP experimental design without much motivation. I would start this section by saying that coupled chemistry simulations were required to diagnose the radiative forcing of many	Accepted: We included a few introductory statements as suggested

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						NTCFs, since their distributions are not well-constrained from obs etc. This subsection briefly describes these simulations. Then re-order the section - ln 11-15, ln 40-49, ln 17-29. [Nathan Gillett, Canada]	
8-356	8	12	9			section 8.2.2. This section has no merit whatsoever unless you can demonstrate that the simulation of all natural climate forces is 100% accurate, which I believe you cannot. [John McLean, Australia]	Rejected: not relevant
8-357	8	12	17	13	6	Keep in mind you are describing an approach, not new understanding or findings. [Stephen E Schwartz, United States of America]	Noted: We considered in the rewrite of revision
8-358	8	12	18	12	19	Suggest clarifying what is being referred to in second half of sentence (ie, 'one for each of the RCPs'). [Philip Cameron-Smith, U.S.A.]	Editorial: we have added "simulation"
8-359	8	12	20	12	29	Also mention that the emissions of aerosols and most short-lived reactive gases in the RCPs are on the low end of the range of possible future emissions. [Twan van Noije, Netherlands]	Editorial There are indeed now publications to substantiate this statement and we have included it, including updating the emission figure
8-360	8	12	25	12	26	"Finally, it is important to recognize that RCP biomass burning projections are only crudely represented, with no feedback between climate change and fires (Bowman et al., 2009; Thonicke et al., 2010; Pechony and Shindell, 2010)." Have these authors the obvious relationships between warming, droughts, heat waves and fires, and the consequent release of CO2? The increase over the last 30 years or so in incidence of fires reported by Munich Re-Insurance suggests otherwise. [Andrew Glikson, Australia]	Editorial:We havel clarified the sentence to indicate that the listed studies do have these feedbacks. Not the RCPS.
8-361	8	12	25	12	26	"Finally, it is important to recognize that RCP biomass burning projections are only crudely represented, with no feedback between climate change and fires (Bowman et al., 2009; Thonicke et al., 2010; Pechony and Shindell, 2010)." Have these authors considered the relationships between warming, droughts, heat waves and fires, and the consequent release of CO2? The increase over the last 30 years or so in incidence of fires reported by Munich Re-Insurance suggests otherwise. [Government of Australia]	Editoral: same as above (comment 8-360)
8-362	8	12	31		38	This figure needs to be reconciled with the overall assessment, including the revised/assessed values in Chapter 11. The historical is very important and needs to be archived in Annex II. For the future, it should be part of our overall assessment of the best values of the Earth system following the RCP estimates of anthropogenic emissions, not just a documentation of what the ACCMIP models used (that belongs as a reference). For example, it is argued in Chapter 11 that the RCPs are in error with what is our best current scientific knowledge of the anthropogenic emissions for N2O and CH4. Thus the RCP concentration pathways and even emissions are inconsistent. [Michael Prather, United States of America]	Accepted: we have coordinated with Annex 2 to include additional information if available
8-363	8	12	42			This is a general comment about this and subsequent sections. It is encouraging to see citations where observations have been used to evaluate climate models--in particular ACCMIP models. However, the subsequent text doesn't really discuss the implications of that evaluation on the conclusions made in the chapter, particularly with respect to radiative forcing. They are generally very qualitative in fashion using terms such as "reasonable". As a consequence, observations play practically no material role in informing our understanding of radiative forcing in spite of significant effort to use those observations. [Government of United States of America]	Noted: we have used more quantitative statements where applicable
8-364	8	12	43			Stevenson paper has (almost) no evaluation against obs; Young et al does [David Stevenson, United Kingdom]	Editorail: we have made the correction
8-365	8	12	45	12	46	"only a limited number" - please indicate roughly how many models were involved. Consider including this information in Table 8.1. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Rejected: Table 1 was removed
8-366	8	12	48			) [David Stevenson, United Kingdom]	Editorial: included
8-367	8	12	52	12	52	Table 8.1: consider replacing the "C" and "1" in this table with the number of models that have contributed to the specific simulations. The core runs can be highlighted by entering the number in bold font. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Noted: Table 1 is removed
8-368	8	12	52	13	1	Table 8.1: List of ACCMIP experiments: Please add the explanation what Core and Tier 1 mean. [Michael Trainer, United States of America]	Noted: same as above (comment 8-367)

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8-369	8	12	52	13	1	Table 8.1: List of ACCMIP experiments: Please add the explanation what Core and Tier 1 mean. [Michael Trainer, United States of America]	Noted: same as above (comment 8-367)
8-370	8	12	52			First I wonder whether this table is required - perhaps this is unecessary detail for the chapter. Second, if retained more explanation is needed. For example, line 4 of the table says 'Year 2000 emissions/1850 SSTs and GHGs'. It doesn't make sense to me to have 2000 GHG emissions, but 1850 GHG concentrations. Does this mean emissions of aerosol precursors? Or short-lived forcers only. This isn't explained. 'Tier 1' is not defined. If retained a caption with more explanation is required. [Nathan Gillett, Canada]	Noted: same as above (comment 8-367)
8-371	8	12	52			Table 8.1 is not really useful - a better, shorter table might be similar but just give the number of models contributing, and note that some were very short for any statistics (4 years for some?). In terms of chemistry Table 8.2 is far more important than 8.1. [Michael Prather, United States of America]	Noted: same as above (comment 8-367)
8-372	8	12				Table 8.1: It's not clear what the "Core" and "Tier 1" notations really refer to -- does this imply something about how heavily results from each RCP/emissions were weighted? It's not well explained in the text. The authors should consider adding further explanation, either in the text or the table footnotes, about what Core and Tier 1 mean? If this is explained somewhere else in the report, then a reference to that section could be added. [Government of United States of America]	Noted: same as above (comment 8-367)
8-373	8	13	6	14	25	The economics are very important to the politicians. The short discussion about other metrics should be extended to give them a bit more information to use. It is realised that this falls outside the scope of the usual scientific review but it forms an important portion of the whole picture - especially since the financial aspects seem tobe a big stumbleblock in the Protocol. [Pieter Aucamp, South Africa]	Rejected: irrelevant comment to section 8.2
8-374	8	13	9	23	7	Again, it would be good to include one or two lines about the societal drivers of these changes. As done for Nitrous oxide for example. [Government of NORWAY]	Rejected: Difficult to fully understand the comment and its relevance to the section
8-375	8	13	11	16	36	Not sure why all this chem in a forcing chapter. I recommend it not be in the chapter and perhaps not in the report. [Stephen E Schwartz, United States of America]	Noted: This is the only place in the AR where atmospheric chemistry is discussed to define the processes leading to the distributions of radiatively active gases
8-376	8	13	12		28	It feels like Pandis and Seinfeld (a truly great compendium) is over referenced here compared, e.g., with Logan et al 1980 for early clean-air trop chemistry. [Michael Prather, United States of America]	Editorial: have updated the references to provide more historical background
8-377	8	13	13			To clarify the text, the authors should consider rewriting it to read: "As a short-lived GHG, the RF from tropospheric ozone strongly dependent on its vertical and spatial structure through radiative coupling with temperature, water vapour, and clouds" (Lacis and Hansen, 1974, Worden et al, 2008, Worden et al, 2011, Bowman et al, 2012) If methane and CO2 were shorter-lived, there'd also be a strong spatial dependence. So, an explanation as to why it's dependent--and the fact that there's been progress on quantifying this dependency--is warranted. "Consequently, to compute the forcing since pre-industrial times, it is necessary to estimate its full three-dimensional distribution, which can only be obtained through simulations using global models" The latter clause "only be" implies that observations play no role, which is clearly not the case given references in the first sentence. The point of those references is not that you need to use a global model, rather that you need to accurately describe the vertical and spatial structure of ozone and its radiative effect in the presence vapor, clouds, etc. Perhaps another phrase, like this one, could be used instead: "Consequently, it is necessary to accurately estimate its full spatio-temporal structure using global models and observations". References to support these suggested revisions include:	Noted: this was useful rewrite and was considered in the final version.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Worden, H. M., Bowman, K. W., Kulawik, S. S., and Aghedo, A. M.: Sensitivity of outgoing longwave radiative flux to the global vertical distribution of ozone characterized by instantaneous radiative kernels from Aura-TES, J. Geophys. Res., 116, doi: 10.1029/2010JD015101, http://dx.doi.org/10.1029/2010JD015101, 2011. Bowman, K., Shindell, D., Worden, H., Lamarque, J. F., Young, P. J., Stevenson, D., Qu, Z., de la Torre, M., Bergmann, D., Cameron-Smith, P., Collins, W. J., Doherty, R., Dalsøren, S., Faluvegi, G., Folberth, G., Horowitz, L. W., Josse, B., Lee, Y. H., MacKenzie, I., Myhre, G., Nagashima, T., Naik, V., Plummer, D., Rumbold, S., Skeie, R., Strode, S., Sudo, K., Szopa, S., Voulgarakis, A., Zeng, G., Kulawik, S., and Worden, J.: Observational constraints on ozone radiative forcing from the Atmospheric Chemistry Climate Model Intercomparison Project (ACCMIP), Atmos. Chem. Phys. Discuss., 12, 23603-23644, doi:10.5194/acpd-12-23603-2012, 2012 [Government of United States of America]	
8-378	8	13	14	13	14	Replace Worden et al ref with Shine and Forster (1997) [Katharine Law, France]	Editorial: reference included
8-379	8	13	14	13	16	Data assimilation can also be used to adjust model simulations so as to be closer to reality for estimation of the current state and its short-term fluctuations. [Adrian Simmons, United Kingdom]	Rejected, unclear what this comment refers to. Ignored
8-380	8	13	15			its full -> the change in ozone's [David Stevenson, United Kingdom]	Editorial: corrected
8-381	8	13	16	13	16	Does this statement about ozone decreasing plant productivity mean ozone at the surface specifically? [European Union]	Editorial: corrected
8-382	8	13	16			The authors should consider this additional reference: Fishman, J., J.K. Creilson, P.A. Parker, E.A. Ainsworth, G.G. Vining, J. Szarka, F.L. Booker and X. Xu, An investigation of widespread ozone damage to the soybean crop in the upper Midwest determined from ground-based and satellite measurements, Atmos. Environ., doi:10.1016/jatmosenv.2010.01.015., 2010. [Government of United States of America]	Editorial: reference included
8-383	8	13	17			also unclear what 'its' refers to: rephrase; +typo possible [David Stevenson, United Kingdom]	Editorial: clarifired that it refers to the ozone impact on productivity
8-384	8	13	18			The authors may wish to include the following references, as well: 1) Fishman, J.; Ramanathan, V.; Crutzen, P. J.; and Liu, S. C.: Tropospheric Ozone and Climate, Nature, Vol. 282, No. 5741, 1979, pp. 818-820. 2) Portmann, R. W., S. Solomon, J. Fishman, J. R. Olson, J. T Kiehl and B. Briegleb, Radiative Forcing of the Earth's Climate System due to Tropical Tropospheric Ozone Production, J. Geophys. Res., Vol. 102, D8, April 27, 1997, pp. 9409-9417. [Government of United States of America]	Rejected, most focus on assessment of science since AR4.
8-385	8	13	22	13	24	"Because of the catalytic role of nitrogen oxides in ozone production, tropospheric ozone chemistry is strongly nonlinear in its dependence on nitrogen oxides (Seinfeld and Pandis, 2006)." The nonlinear dependence of tropospheric ozone chemistry on the precursors nitrogen oxides and CO, CH4, and VOCs is important. However, it is not just simply due to the catalytic role of nitrogen oxides. What does the nonlinearity in the chemistry imply for the predictions of global models with coarse grid resolution? [Michael Trainer, United States of America]	Editorial: We have rephrased this discussion and there no explicit mention on the nolinearity that would allow for the changes suggested.
8-386	8	13	22	13	24	"Because of the catalytic role of nitrogen oxides in ozone production, tropospheric ozone chemistry is strongly nonlinear in its dependence on nitrogen oxides (Seinfeld and Pandis, 2006)." The nonlinear dependence of tropospheric ozone chemistry on the precursors nitrogen oxides and CO, CH4, and VOCs is important. However, it is not just simply due to the catalytic role of nitrogen oxides. What does the nonlinearity in the chemistry imply for the predictions of global models with coarse grid resolution? [Michael Trainer, United States of America]	Editoral: same as above (comment 8-385)
8-387	8	13	24	13	13	Seinfeld and Pandis (2006) is not an original reference for ozone photochemistry - either change it or at least add "and references therein". Likewise Line 28. [Katharine Law, France]	Editorial: have updated the references to provide more historical background

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8-388	8	13	24	13	27	"As emissions of these precursors have increased (Figure 8.2), over the last decades (Parrish et al., 2009; Cooper et al., 2010; Logan et al., 2012), but with important regional variations (Section 2.2)." See comment above, here is the opportunity to show an example of how well the current CTMs can capture this regional variation of measured ozone trends and to emphasize the synergy of measurements and models in the improvement of our understanding of the complexities of the nonlinear photochemistry of tropospheric ozone. [Michael Trainer, United States of America]	Accepted: changes were made to the text to mention long-term ozone trends
8-389	8	13	24	13	27	"As emissions of these precursors have increased (Figure 8.2), over the last decades (Parrish et al., 2009; Cooper et al., 2010; Logan et al., 2012), but with important regional variations (Section 2.2)." See comment above, here is the opportunity to show an example of how well the current CTMs can capture this regional variation of measured ozone trends and to emphasize the synergy of measurements and models in the improvement of our understanding of the complexities of the nonlinear photochemistry of tropospheric ozone. [Michael Trainer, United States of America]	Accepted: same as above (comment 8-388)
8-390	8	13	27	13	28	Dry deposition is also a major loss for tropospheric ozone. [Terje Berntsen, Norway]	Accepted: We meant chemical loss, so sentence will be corrected
8-391	8	13	33	13	33	Should "upper troposphere" be "upper polar troposphere"? The tropics should be different? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Noted: since the statement is "up to a year", we are leaving it unchanged
8-392	8	13	33			In the marine BL the decay time for O3 is never as short as 1 day, perhaps 5 days. The 1 day might occur in regions with tons of isoprene or other alkenes - but is not relevant here. [Michael Prather, United States of America]	Noted: text was modified
8-393	8	13	34			25 ->22 - the global mean lifetime in Stevenson et al (2006) is 22+/-2 days [David Stevenson, United Kingdom]	Editorial : corrected typo
8-394	8	13	36	13	45	It is not clear if all budget estimates in Table 8.2 are results after AR4. Some references are certainly based on the results before ACCMIP or CMIP5. The authors should consider updating the Table with AR5 model simulations. [Government of United States of America]	Noted: AR5 simulations, if relevant, are included in ACCMIP. Other references include work since AR4 or not included in AR4.
8-395	8	13	37	13	37	Budget terms are not shown for the ACCMIP model results - presumably they are going to be added? [Katharine Law, France]	Accepted: They are available (see Young et al., 2013) and have been added
8-396	8	13	41	13	45	Do all these acronyms need explaining? [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Noted: we left as is since the references clearly indicate what the acronym refers to.
8-397	8	13	44	13	44	What does "reasonable agreement" mean? [Michael Trainer, United States of America]	Noted: we use quantified statements where available
8-398	8	13	44	13	44	What does "reasonable agreement" mean? [Michael Trainer, United States of America]	Noted: same as above
8-399	8	13	48	13	48	Table 8.2: Please clarify what you define as "present conditions". [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: This means ca. 2000; this is now made clear
8-400	8	13	48	13	49	Include Wu et al. (2007) in Table 8.2. Wu, S., L.J. Mickley, D.J. Jacob, J.A. Logan, R.M. Yantosca, and D. Rind, Why are there large differences between models in global budgets of tropospheric ozone?, J. Geophys. Res 112, D05302, doi:10.1029/2006JD007801, 2007. [Loretta Mickley, United States of America]	Accepted: we included that information
8-401	8	13	48	14	1	Table 8.2: "Summary of tropospheric ozone global budget model and observation estimates for present conditions. Focus is on modeling studies published since AR4." I would suggest to clearly distinguish in Table 8.2 which models are post AR4, which of these models are part of the ACCMIP group, which models are part of the ACCENT group and which entries give the means of a group of models. [Michael Trainer, United States of America]	Rejected: Multi-model averages are already stated. So it is not clear how much additional information is required.
8-402	8	13	48	14	1	Table 8.2: "Summary of tropospheric ozone global budget model and observation estimates for present conditions. Focus is on modeling studies published since AR4." I would suggest to clearly distinguish in Table 8.2 which models are post AR4, which of these models are part of the ACCMIP group, which models are part of the ACCENT group and which entries give the means of a group of models.	Rejected: same as above

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Michael Trainer, United States of America]	
8-403	8	13	48	14	1	Table 8.2: "Summary of tropospheric ozone global budget model and observation estimates for present conditions. Focus is on modeling studies published since AR4." I would suggest to clearly distinguish in Table 8.2 which models are post AR4, which of these models are part of the ACCMIP group, which models are part of the ACCENT group and which entries give the means of a group of models. [Michael Trainer, United States of America]	Rejected: same as above
8-404	8	13	48			Define 'STE' in the caption. [Nathan Gillett, Canada]	Editorial: done
8-405	8	13	48			The table says obs. estimates and so should include the observational STE fluxes from Murphy (450 Tg/y w/range of 195 - 875) and later S.C. Olsen (see ref for next page) 550±140 Tg O3/yr [Michael Prather, United States of America]	Accepted: numbers included
8-406	8	14	1			Table 8.2: Include Wu et al 2007? And Skeie et al 2011? Remove Hauglustaine et al 2005 and maybe some others. [David Stevenson, United Kingdom]	Accepted: however Skeie is not included because not enough information is available from the paper.
8-407	8	14	3			Regarding the statements: "To establish credibility in simulating the recent atmospheric composition, model simulations for present-day conditions or the recent past are evaluated (Figure 8.3) against frequent ozonesonde measurements (Logan, 1999; Tilmes et al., 2011) and additional surface, aircraft and satellite measurements. The ACCMIP model simulations indicate a reasonable representation of tropospheric ozone, especially when the multi-model ensemble mean (or median) is considered, albeit with significant biases." What does "reasonable representation" mean? Particularly when the observations show significant biases? "Reasonable" needs to be tied to where ozone is radiatively important. Fig 8.3 from Young et al, 2012 does show a significant biases but with a large spread in the standard deviation because the sondes are sparse and have strong variability from local processes. However, as shown by the TES radiative kernels in Fig 3. of Worden et al, 2011 and Fig. 1 of Bowman et al, 2012, matching satellite and sondes from 30-90s at 500 hPa is not nearly as important as the upper troposphere in the tropics, e.g., 30S-EQ, where there is a significant underestimate relative to ozonesondes and TES (satellite). Fig 6 from Bowman et al, 2012 shows that this underestimate leads to a 100 mW/m^2 underestimate in the mean distribution centered primarily over the Tropical Atlantic Bowman et al (Fig 5.) On the other hand, many models overestimate in one region and underestimate in another region leading to more reasonable global differences. Nevertheless, these regional biases could lead to uncertainties in the climate response. Perhaps another way of phrasing this sentence is: "ACCMIP model simulations show good agreement with satellites and sonde data in the midlatitudes with radiative differences less than 10 mW/m^2 for the ensemble mean. However, there are significant biases in the tropics of up to 100 mW/m^2 where ozone is more radiatively efficient. These biases could have an impact on the regional response predicted by ACCMIP (Shindell et al, 2009)", with the relevant additional reference being: Shindell, D. and Faluvegi, G.: Climate response to regional radiative forcing during the twentieth century, Nature Geosci, 2, 294–300, http://dx.doi.org/10.1038/ngeo473, 2009/04//print. [Government of United States of America]	Editorial: we have rewritten the sentence to be more quantitative, as suggested by the reviewer.
8-408	8	14	3			The authors' lack of confidence is revealed in the phrase "to establish credibility." I would suggest leaving the phrase, but I expect you will retreat to the more conventional and generally neutral "to evaluate the simulations" or the like. [Stephen E Schwartz, United States of America]	Noted: It is not intended to indicate "lack of confidence" so text is changed.
8-409	8	14	5	14	7	How can there be "reasonable agreement" and at the same time "significant biases"? This point seems to gloss over the fact that models don't always perform that well as shown in Figure 8.3. The text should be updated to more accurately report on general model behaviour. [Katharine Law, France]	Noted: we have rewritten those statements.
8-410	8	14	8			As written it sounds like CMIP5 models used different anthropogenic and biomass burning emisisions, but I	Noted: They mostly use the same ones and we have

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						think they used the same. Clarify. [Nathan Gillett, Canada]	modified the text to clarify
8-411	8	14	8			The ACCENT intercomparison was never officially called 'ACCENT-AR4', and probably should not be referred to as such. Its official name (although rarely used) was ACCENT PhotoComp Experiment 2 (Experiment 1 was Gauss et al., 2006) [David Stevenson, United Kingdom]	Editorial: Text is corrected and changed to ACCENT
8-412	8	14	10			A similar multi-model mean' - it is unclear if you are referring to the ozone burden or individual budget terms [David Stevenson, United Kingdom]	Editorial: Burden: this is now stated.
8-413	8	14	11		12	Is it the estimate of the burden or the estimate of the ability to calculate this burden that has not changed. [Stephen E Schwartz, United States of America]	Editorial: this means both and this is now clarified
8-414	8	14	12	14	12	Skeie et al…sentence is hanging and needs a preface [Vaishali Naik, United States of America]	Editorial: text modified
8-415	8	14	12	14	12	The Skeie et al. 2011 study finds a 5% increase in the anthropogenic contribution to the tropospheric O3 burden, not in the whole tropospheric burden! Assuming a current burden of 340 Tg, this represents at most a 1.5% increase in the whole tropospheric burden. (Note that the ACCMIP studies show only a 4% increase in burden from 1980 to 2000, see Young et al., 2012). [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Accepted: included the correction as sugegsted.
8-416	8	14	18			More uncertain than what? [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Editorial: than burden. Text is clarified.
8-417	8	14	19	14	19	The net chemical production is about 450 Tg/yr, not 300 Tg/yr as stated here. The net production must roughly balance the loss from deposition (950-1000 Tg/yr) and the source from STE (530-550 Tg/yr) as outlined in Table 8.2. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Editorial: typo is corrected.
8-418	8	14	20			and to some extent AR4' - it is not apparent in Table 8.2 as it stands [David Stevenson, United Kingdom]	Accepted: text is ephrased accordingly.
8-419	8	14	23	8	25	902 for deposition and 636 for STE are not the numbers in Table 8.2 (Stevenson et al 2006) numbers. These numbers look wrong to me. [David Stevenson, United Kingdom]	Accepted: values corrected
8-420	8	14	23	14	25	Where do the fluxes from deposition (902 Tg/yr) and STE (636 Tg/yr) come from? The text cites ACCENT-AR4, but these fluxes are included in Table 8.2 from Stevenson et al., 2006, and are stated as 1000 and 550 Tg/yr respectively. Are these data from ACCMIP or CMIP5? The data are not entirely consistent with the values for previous studies shown in the Table. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Accepted: values corrected
8-421	8	14	24	14	27	A caveat should be added stating that model derived estimates of the net flux of ozone into the troposphere are very dependent on tropopause definition. [Katharine Law, France]	Accepted: There is indeed some sensitivity to the position of the tropopause, so added qualifier.
8-422	8	14	28			The other observation-based STE flux paper corrects and updates the flux based on the Murphy-Fahey approach: 550±140 Tg O3/yr (Olsen, S. C., etal 2001 JGR 106(D22), 28,771-28,784) - I think your should give this observationally derived STE flux in addition to the model fluxes (line 25). [Michael Prather, United States of America]	Accepted: values included
8-423	8	14	30			95 Tg. In Figure 8.4, it looks more like 100 Tg - and in Young et al (Table 4) the value is 98 Tg (however, in Young et al Table 1, the absolute numbers for 1850 and 2000 differ by 88 Tg, so there is some inconsistency to be cleared up). [David Stevenson, United Kingdom]	Accepted: This is due to our use of intermediate numbers. We have now used the final published numbers.
8-424	8	14		14		Table 8.2 uses STE. This should be defined for clarity as Strat-Trop Exchange [M Daniel Schwarzkopf, United States of America]	Editorial: done
8-425	8	14				Table 8.2: please define "STE" [Räisänen Petri, Finland]	Editorial: done
8-426	8	15	1		4	I do not see that the conclusion here is obvious from Fig 8.4 - in fact the size of the box or whiskers does not obviously diminish as one goes from absolute to difference w.r.t. 1850. Am I missing something? [Michael Prather, United States of America]	Accepted: Our statement was based on an earlier version of the plots. The comment is accurate and text is revised accordingly.
8-427	8	15	7	15	8	This isn't what you said on page 11, lines 9 to 10. These statements should be consistent. [John McLean, Australia]	Noted: statement rewritten to be more clear

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8-428	8	15	8	15	11	The wording of the second part of this sentence is confusing. What data is "assessed to be of qualitative use only"? The Montsouris data? Data from Morenco et al., (1994)? Or is the statement intended to mean that the Schonbein method of O3 analysis is controversial? If so, a study that puts the Schobein method in doubt should also be referenced here. Additionally, it should clarified that "Schonbein" is the O3 analysis method for the early records of O3. The phrase: "Schonbein paper analysis" might mislead the reader into thinking that there is a relevant "analysis" of this early O3 data in a "paper" authored by someone named "Schonbein". The authors should consider revising the text accordingly. [Government of United States of America]	Noted: we have rewritten this discussion of the Schonbein paper and added a reference.
8-429	8	15	10	15	11	Either say what this 'additional information' is or omit. As written this is only clear to specialists that have read these papers. [Nathan Gillett, Canada]	Accepted: this was meant to say: additional information on ozone levels from … It is rephrased
8-430	8	15	11			With reference to Parella et al, doesn't this suggest that their present-day O3 must be some way out (if the pre-industrial is now consistent with obs, and the pre-industrial to present change is the same as other studies)? This makes the bromine chemistry explanation rather less attractive. [David Stevenson, United Kingdom]	Accepted: It is indeed such a research area. Reference is removed.
8-431	8	15	17	15	20	This sentence is not clear as written: How can you have observed trends for "decades since present"? Wouldn't those be in the future? Also, there appears to be a grammatica/typographicall problem: "a recent evaluation … questions" (s missing). Finally, the phrase "over long periods" is confusing and contradictory, since the sentence starts out discussing shorter time scales. As a result of these comments, the authors should consider rewriting this statement. [Government of United States of America]	Accepted: it is meant as previous decades. This is clarified. Typo is fixed.
8-432	8	15	17	15	20	This sentence is very misleading - models also have difficultly reproducing measured surface ozone trends - this data is more reliable than the sonde data. Ozone changes may be due to emission trends but natural variability should also be mentioned which can affect transport pattersm like NAO (e.g. Pausata et al., 2012) and natural emissions - there is no dicsussion about these issues in this chapter. [Katharine Law, France]	Accepted: we have clarified the surface ozone aspect of the comment. The discussion of transport patterns is of more relevance to other chapters of AR5 and is not discussed here.
8-433	8	15	20			on -> in [David Stevenson, United Kingdom]	Editorial: corrected
8-434	8	15	22	15	27	How were historical estimates for tropospheric ozone constructed and why was this done independently from the land use-land cover changes used the carbon cycle simulations? Over what period were these estimates made given that ozonesondes and satellites are available for the more modern period? [European Union]	Noted: The statement on land-use refers to emissions.
8-435	8	15	23			Two sentences start Furthermore! [David Stevenson, United Kingdom]	Editorial: done
8-436	8	15	24	15	26	It is not very clear what the authors are trying to convey in this sentence: "Furthermore, the historical estimates were constructed …." [Vaishali Naik, United States of America]	Taken into account, there was no attempt at consistency between land-use changes and ozone precursor emissions. Text is revised
8-437	8	15	26	15	27	Setence should be removed - IPCC should not call for more research [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted, sentence removed.
8-438	8	15	26			of -> for [David Stevenson, United Kingdom]	Taken into account see comment 8-437
8-439	8	15	36	16	2	This section talks very little about stratospheric water vapor. Although only part of it is driven by anthropogenic forcing, the authors should mention the estimated stratospheric water vapor trend, and their radiative impact for global surface energy balance. References can be cited: Hurst, D. F., S. J. Oltmans, H. Vömel, K. H. Rosenlof, S. M. Davis, E. A. Ray, E. G. Hall, and A. F. Jordan (2011), Stratospheric water vapor trends over Boulder, Colorado: Analysis of the 30 year Boulder record, J. Geophys. Res., 116, D02306, doi:10.1029/2010JD015065. Solomon, S., K. H. Rosenlof, R. W. Portmann, J. S. Daniel, S. M. Davis, T. J. Sanford, and G.‐K. Plattner (2010), Contributions of stratospheric water vapor to decadal changes in the rate of global warming,	Noted: this section only discussed the chemistry part of stratospheric water vapor. The RF is in section 8.3. We will include a discussion of the observed trends.

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						Science, 327, 1219–1223, doi:10.1126/science.1182488. [Government of United States of America]	
8-440	8	15	36	16	3	This section needs some work. It doesn't contain any data on changes in stratospheric ozone burdens and the information on water vapour is limited to one sentence on page 16 lines 1-2 which has to do with oxidation of methane and seems to be better dealt with in the following section 8.2.3.3. [Robert Waterland, United States of America]	Rejected: this section only discussed the chemistry part of stratospheric water vapor.
8-441	8	15	36			Would it be worth noting here (or elsewhere) the importance of getting solar spectrum correct in radiation scheme for stratospheric heating and ozone? See Zhong, W., S.M. Osprey, L.J. Gray and J.D. Haigh (2008) The influence of prescribed solar spectrum on calculations of atmospheric temperature. Geophys Res Lett, 35, L22813, doi:10.1029/2008GL035993 [Joanna Haigh, United Kingdom]	Rejected: this is not the right place for this discussion.
8-442	8	15	38	15	39	« Stratospheric ozone has experienced significant depletion since the 1970s due to bromine and chlorine containing compounds (Solomon, 1999) ». According to models, it started in 1960s (WMO, 2011). It is in agreement with the following statement :"It starts to decline in the 1960s » made at p8.23, l29 [slimane bekki, France]	Accepted: text corrected
8-443	8	15	38	15	42	Has stratospheric ozone decreased or increased relative to 1750? It is stated here that it has decreased since the 1970s but does that imply a decrease since 1750 also? [European Union]	Noted: We don't know what it was in 1750. Indications are that it probably increased some from 1750s until the late 1960s.
8-444	8	15	38	16	2	For a section titled 'Stratospheric Ozone and Water Vapour' there is very limited discussion of stratospheric water vapour. This could be improved given that there are observations of stratospheric water vapour - see Hurst & Rosenlof 2012: Hurst, D. and K. Rosenlof, 2012: [Global Climate] Stratospheric water vapour [in "State of the Climate in 2011"]. Bulletin of the American Meteorological Society, 93(7), S48-S49. What to do future projections show for stratospheric water vapour? Are we confident? Is it important? [European Union]	Noted: this section only discussed the chemistry part of stratospheric water vapor. A short discussion on trends is included.
8-445	8	15	46		47	The ODP of N2O has not really been evaluated by the community and derived with a large enough set of 3D models. This statement reflects that one paper, but it needs more back up to be made with such certainty here. Also we are not redoing the ODPs are we? [Michael Prather, United States of America]	Accepted: this is one model only and the use of the specific reference is made.
8-446	8	15	49			2032 sounds very specific and certain. There is a wide range of uncertanties -> please rephrase. [Volker Grewe, Germany]	Accepted: This is the multi-model mean, we have clarified and added range
8-447	8	15	50	15	52	Grewe 2007: Investigated stratospheric ozone changes and the impact on tropspheric ozone [Volker Grewe, Germany]	Editorial: reference included
8-448	8	15	54	15	56	It is stated that "observationally-based estimates of recent trends in age of air (Engel et al., 2009; Stiller et al., 2012) are not in agreement with the acceleration of the stratospheric circulation found in model simulations". It is much too strong, especially when just after, it is added that observational trends are not reliable suggesting large error bars. Therefore, taking into account theses uncertainties, trends for models and observations are not inconsistent. Replace 'are not in agreement' by 'do not appear to be entirely consistent'. [slimane bekki, France]	Accepted: rewritten as suggested.
8-449	8	15				Section 8.2.3.2: There is no new information added about stratospheric water vapor in this section. Therefore, the authors should consider changing the section title to "Stratospheric Ozone", and then also moving the statement about CH4 being a source of strat. water vapor to the next section (8.2.3.3) in which methane is discussed. [Government of United States of America]	Accepted: the idea is to separate the discussion of chemistry from the discussion on RF. We'll remove all unnecessary duplications in final version.
8-450	8	16	1			This sentence hangs a bit. Revise to "and hence the long-term increase in CH4 contributes an anthropogenic forcing in the stratosphere." It would also be useful to note that increasing CH4 generally leads to increasing stratospheric O3 itself, i.e. a 10% increase in CH4 gives about 1% increase in strat O3, including lower strat. (Hsu & Prather, GRL, 2010), but this has not(?) been evaluated in terms of RF. [Michael Prather, United States of America]	Accepted: rewritten along the lines as suggested
8-451	8	16	4	16	32	Sect. 8.2.3.3. Methane. There is no mention here of the anthropogenic vs. natural sources of methane emission, although sinks are discussed. This is in contrast with the treatment of N2O in sect. 8.2.3.4 and	Rejected: methane emissions are discussed in chapter 6, as referenced.

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						halogenated species and aerosols in following sections. [Robert Kandel, France]	
8-452	8	16	4	16	36	It is unclear from the discussion whether the cited lifetime on line 19 (and the lifetime in table 8A.1) is the pulse decay lifetime or the lifetime calculated using the instantaneous removal rate in the present day atmosphere. Its sounds as though it might be the former. If that is the case, then the text in line 14 - 21 is misleading and should be rewritten. To be consistent, the pulse decay lifetime should be used in the GWP and GTP definitions. [Government of United States of America]	Noted: it does not mention "pulse decay" so it is indeed the instantaneous removal rate. This section has been rewritten to address such issues
8-453	8	16	6	16	6	Suggest rephrasing, because 'has increased by 2.5 times' could be misleading, since CH4 has increased by 150% since preindustrial times (ie, increasing from 0.7ppm to 1.8ppm). [Philip Cameron-Smith, U.S.A.]	Editorial: done
8-454	8	16	6	16	6	Table 2.12 says no such thing. [John McLean, Australia]	Editorial: text modified
8-455	8	16	6	16	6	Replace "concentration" with "surface mixing ratio". [Robert Waterland, United States of America]	Editorial: text modified
8-456	8	16	6			"with the high-end scenario RCP8.5 projecting a further doubling" [Michael Prather, United States of America]	Editorial: text modified
8-457	8	16	7		8	Language awkward. Present estimates of methane emisson range from x to yl [Stephen E Schwartz, United States of America]	Accepted: The new text takes this suggestion into account
8-458	8	16	8			"with bottom-up emissions estimates ranging from 479 to 706 Tg/y and a recent top-down estimate with formal uncertainty ranges of 554 +- 56 (68% confidence interval) Tg/yr (Prather etal, March 2012, GRL)." This needs to reflect best current knowledge and not just a range of results. Table 6.7 also needs to be fixed (similar review comments made for Chapter 6). [Michael Prather, United States of America]	Accepted: rewritten as suggested.
8-459	8	16	8			The range in emission 47% substantially exceeds that of sink rate 39%, but much more focus on sink than source in discussion. why? I will bet dollars to doughnuts that the models with high source rates have high sink rates to get the right answer. Would be valuable for you to examine that. [Stephen E Schwartz, United States of America]	Rejected: The discussion on emissions is in Chapter 6.
8-460	8	16	11			See notes on figure below. This should reflect our best assessment of the RCP emissions and resulting abundances, and not just the published RCP/MAGICC results. [Michael Prather, United States of America]	Noted: we have updated with additional information, if available.
8-461	8	16	14	16	14	Replace comma with a full stop after "…troposphere." The same for the next sentence after (Seinfeld and Pandis, 20006). [Vaishali Naik, United States of America]	Editorial: done
8-462	8	16	14	16	14	replace comma (,) with period (.) [Räisänen Petri, Finland]	Editoral: same as above
8-463	8	16	14	16	21	The authors should consider making a few clarifications or additions here, that wouldclarify the text: - the secondary reactions that produce OH are a result of chemistry involving biogenic VOCs; there is currently a very poor understanding of these reactions. - There is some recent evidence that instrument artifacts might be responsible for elevated OH being observed in regions heavily influenced by biogenic VOC emissions (Mao et al., Atmos. Chem. Phys., 2012). - bacterial uptake of methane should be briefly explained. I assume this is occurring in soils? Perhaps a reference could be included here? [Government of United States of America]	Accepted: Text was rewritten and simplified, while acknowledging the poorly understood nature of the recent OH recycling chemistry papers. Bacterial uptake is indeed in soils.
8-464	8	16	16	16	16	again Seinfeld and Pandis reference [Katharine Law, France]	Editorial: have updated the references to provide more historical background
8-465	8	16	16			Again, reference Levy 1971 or Logan 1980 for trop OH, not the Seinfeld and Pandis compendium here [Michael Prather, United States of America]	Editorial: have updated the references to provide more historical background
8-466	8	16	19	16	20	Need references for the bacterial uptake and stratospheric loss of CH4 [Vaishali Naik, United States of America]	Editorial: reference included
8-467	8	16	23	16	25	Is Montzka also a modelling study? Is the message that modells tend to underestimate the methane lifetime?	Noted: Montzka is based on methylchloroform

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						[Volker Grewe, Germany]	observations and does not use a 3d model. Yes, that is the message. Text is clarified.
8-468	8	16	23	16	25	The relation to the Prather study should be made more clear! [Volker Grewe, Germany]	Accepted: text is rephrased accordingly.
8-469	8	16	24	16	24	Should 'nt' be 'and' ? [Philip Cameron-Smith, U.S.A.]	Accepted: typo is corrected
8-470	8	16	24	16	24	change "nt" to "consistent" ... with a multi-model mean similar to prior studies consistent with methyl … [Jasmin John, United States of America]	Accepted: typo is corrected
8-471	8	16	24	16	25	There seems to be a disconnect in the sentence "…prior studies nt with methyl …." [Vaishali Naik, United States of America]	Accepted: typo is corrected
8-472	8	16	24			"nt" ? -> "based on" ? [Volker Grewe, Germany]	Accepted: typo is corrected
8-473	8	16	31	16	36	There is a recent study that quantifies methane feedbacks on RF which might be useful to include in this section: Isaksen et al., GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 25, GB2002, doi:10.1029/2010GB003845, 2011. [Government of United States of America]	Noted: reference not adequate for this section.
8-474	8	16	31	16	36	This paragraph should finish with a conclusion. [Twan van Noije, Netherlands]	Rejected: Our writing is consistent with other subsection so we will not include a conclusion
8-475	8	16	31		36	This is great, what needs to be added is that: "Thus the steady-state lifetime for a CH4 increment (i.e., the added burden divided by the added source, a perturbation lifetime, see Isaksen & Hov 1987 Tellus B 39, 271–285) is 12.4+-1.4 yr, which is 1.36 times larger than the lifetime of the total CH4 burden, 9.14 yr. Note that this increase in lifetime of a perturbation includes the feedback on the OH lifetime, but not to other CH4 losses (stratosphere, soils, tropospheric Cl). " these numbers here are from the Prather 2012 GRL analysis and can be readily redone with uncertainties from the spreadsheet in GRL suppl. material. [Michael Prather, United States of America]	Accepted: Thank you for the compliment. Text was rewritten.
8-476	8	16	33	16	33	Isn't the OH feedback the change in CH4 due to the change in OH which is due to CH4, rather than dOH/dCH4? Isn't dOH/dCH4 a sensitivity rather than a feedback? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted: paragraph was rewritten
8-477	8	16	33		36	Holmes etal (2011, PNAS, 108(27), 10997-11002) is the most recent review of the CH4 feedback factor (includes TAR, Fiore et al and new calculations) gives -0.32 (+-16%), but does not include the ACCMIP. There may also be others. Please pick a recommended value & uncertainty here, so that the methodology in Prather (2012 GRL) can be used to update all the budgets and lifetimes. [Michael Prather, United States of America]	Accepted: paragraph was rewritten
8-478	8	16	35	16	36	Given that there are only two model results from the ACCMIP study, the importance/significance of these individual model results is not clear. If these results are to be included, it is important to note the number of models included in the Fiore et al 2009 study (12 models). [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Accepted: we have balanced the ACCMIP information and rewritten the paragraph
8-479	8	16	36			It would be useful to put in a few sentences here linking "adjustment time" used later in the tables, but not really defined: "Chemical feedbacks change the time scale for decay of a CH4 perturbation. The adjustment time of a perturbation to CH4 is calculated as the steady-state lifetime of the perturbation itself (i.e., added burden divided by added emissions), and it exactly describes the integrated impact of a single pulse (e.g., Prather 2002, Peters et al 2011?). The actually decay of the pulse includes a mixture of times scales and is approximated here as a single e-fold using the adjustment time as given in Table 8.A.1." [Michael Prather, United States of America]	Accepted: paragraph was rewritten
8-480	8	16	40	16	40	Table 2.12 says no such thing. [John McLean, Australia]	Noted: coordinated with Chapter 2.
8-481	8	16	40		47	It would be useful to add a brief note here: "The addition of N2O to the atmosphere changes its own lifetime through feedbacks that couple N2O to stratospheric NOy and O3 depletion (Prather 1998 Sci; 2001 TAR	Accepted: text is rephrased accordingly.

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						Table 4.5; Ravishankara 2009; Prather & Hsu 2011 Science 330: 952-954) so that the lifetime of a perturbation is less than that of the total burden, 121+-10 y " (see Prather etal 2012 GRL for spreadsheet and numbers). [Michael Prather, United States of America]	
8-482	8	16	41	16	16	Change "Increases in N2O deplete…" to "Increases in N2O lead to depletion of…" since N2O does not directly deplete ozone. [Robert Portmann, United States of America]	Accepted: rewritten as suggested.
8-483	8	16	42	16	43	A useful clarification here would be that the increase in lower stratospheric O3 is a result of increased UV radiation at these altitudes when mid to upper O3 is depleted, if, indeed, this is the concept the authors wish to put forth. If that is inaccurate, then the authors should consider revising the text to be more clear. [Government of United States of America]	Accepted: comment included
8-484	8	16	42	16	43	Add photolysis changes to the list of causes of tropospheric changes. [Robert Portmann, United States of America]	Accepted: comment included
8-485	8	16	44	16	44	Rather than "agricultural (fertilizer)" suggest revising as "agricultural (nitrogen sources)". A relevant Canadian reference would be: Rochette, P., D. Worth, T. Huffman, J. A. Brierley, B. G. McConkey, J. Y. Yang, J. J. Hutchinson, R. L. Desjardins, R. Lemke and S. Gameda (2008). "Estimation of N2O emissions from agricultural soils in Canada. II - 1990-2005 inventory." Canadian Journal of Soil Science 88: 655-669. [Government of Canada]	Accepted: remove fertilizer. Reference not used as it is too regional in nature for his section.
8-486	8	16	45	16	46	Also fertilizer induced N2O emissions are mostly due to soil emissions and also natural emissions are affected by atmospheric N deposition. Approx. 2/3 of all atmospheric N2O sources are linked to soil emissions (see chapter 6, or e.g. Fowler et al., 2009, Atmospheric composition change: Ecosystems – Atmosphere Interactions. Atmospheric Environment, 43, 5193-5267. [European Union]	Accepted: Text explcitly includes the numbers indicated and the reference is used.
8-487	8	16	45	16	48	The text here suggests that this report recommends using 131 years for the lifetime of N2O. However, Table 8A.1 still has 121 years. The authors should reconicle these differences. [Government of United States of America]	Accepted: inconsistency fixed.
8-488	8	16	48			One important new piece of the N2O chemical feedbacks is the impact on CH4 (Prather & Hsu 2011 Science 330: 952-954). This paper is the first/only analysis to date of the coupled N2O-CH4 strat-trop chemistry and calculates that this coupling reduces the N2O GWP by 4.5% (CH4 reductions alone, not including any additional trop O3). The GTP effects were not estimated. Note that the CH4 reductions are not on the CH4 decade mode time, but the N2O century mode time. [Michael Prather, United States of America]	Accepted: coupling mentioned and reference added.
8-489	8	17	4	17	16	It would be useful to say here how the ACCMIP aerosol results have been evaluated and/or how they are diffirent from the Aerocom comparisons mentioned on Page 25. This is important given that the authors are suggesting we now have high confidence in aerosol simulations for estimating aerosol direct effects. [Katharine Law, France]	Rejected: Aerosols are discussed in Chapter 7. It is unclear where a statement as described in the comment is made.
8-490	8	17	8	17	9	For clarity of meaning, secondary inorganic and secondary organic aerosols should be separated more clearly. I would suggest using a semi-colon instead of a comma: (secondary inorganic aerosols (SIA): sulphate, nitrate, ammonium; and secondary organic aerosols (SOA)) [Eimear Dunne, Finland]	Editorial: corrected
8-491	8	17	25	30	30	The section refers to the human influence on radioactive forcing, but it is never really explained what this means. It needs to be spelled out that humans are responsible for the economic systems, governance structures, sectorial activities, etc. It is about the behaviors of individuals and social systems in certain times and in certain places, which have an impact on the global energy balance through radiative forcing. [Government of NORWAY]	Rejected: This chapter is a scientific assessment of the impact of emissions. It is not relevant to discuss the role of humans as 'protagonists' here.
8-492	8	17	25		34	This is a great opening. I agree that it is important to label this ANTHROPOGENIC RF, as opposed to just the 2010 minus 1750.0 RF, but it implies that we know how much of the GHG change is natural. Thus somewhere in this section, it is important to note the century-scale variations in CO2 (10 ppm), N2O (10 ppb), CH4 (40 ppb) prior to 1750 but in the holocene that have been given in Chapter 5. These represent natural variability, that we have to consider as would have possibly continue to 2010. Thus the 'natural' background of the gases has some additional uncertainty of this order. Larger scale changes to natural emissions caused by climate and land-use change should be attributed to anthropogenic, but these late holocene fluctuations are part of the	Taken into account: by adding a description of text in 8.3.2 and quantification of uncertainty for RF of WMGHGs. A table in supplementary is also added.

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						inherent anthropogenic uncertainty and give us a more accurate with uncertainty estimate of the anthropogenic changes in the big 3 GHG. [Michael Prather, United States of America]	
8-493	8	17	27	17	34	For a section that is set out to discuss the anthropogenic RF it does not talk much about the anthropogenic part. The role of humans (and society) as drivers of change does not come out very strongly (this is also apply to the other chapters in WGI). By starting with RF or emissions an important part gets lost and it will later in the assessment be difficult to introduce humans as the "protagonists" who can influence the RCPs and pursue "climate resilient pathways" and stabilization pathways through transformations, when they have not been firmly introduced as the agents of change in WG1. Right now the focus seems to be on emissions, rather than who is causing them. [Government of NORWAY]	Rejected: This chapter is a scientific assessment of the impact of emissions. It is not relevant to discuss the role of humans as 'protagonists' here.
8-494	8	17	27		34	The role of humans (and society) as drivers of change does not come out very strongly in in this part. By starting with RF or emissions an important part gets lost and it will later in the assessement be difficult to introduce humans as the "protagonists" who can influence the RCPs and pursue "climate resilient pathways" and stabilization pathways through transformations, when they have not been firmly introduced as the agents of change in WG1. Right now the focus seems to be on emissions, rather than who is causing them. [Government of NORWAY]	Rejected: This chapter is a scientific assessment of the impact of emissions. It is not relevant to discuss the role of humans as 'protagonists' here.
8-495	8	17	31			a reference decribing the interactions would be good, otherwise this sentence is very unspecific. [Volker Grewe, Germany]	Taken into account. This paragraph has been reworded
8-496	8	17	36	17	36	I think that the WMGHG should be replaced by GHG, as I assume that there is no plan to deal with changes in the spectral properties of non WMGHG in a separate section. Furthermore, in the section you mention water vapor, which earlier was defined as a non WMGHG and also not as RF gas in the troposphere. I should add that if changes in our understanding of water vapor spectroscopy are discussed somewhere else in the IPCC report then some of my comments below about the water vapor continuum might be better directed to that section. [David Paynter, United States of America]	Accepted. Text revised.
8-497	8	17	36	18	27	HITRAN versions differ by progresses on spectral line positions and number often related to specroscopic data corrected from laboratory measurements at higher resolution or in other wavelength ranges (microwave) where some parameters characteristic of the molecules are more accessible. The intensity measurements of the absorption of whole bands are rare as the community relies on careful low resolution studies performed maybe more than fifty years ago. It is thus normal that the influence of modifications on radiative transfer is weak, however, for the sake of consistency I would recomment working always with the more recent HITRAN data as the HITRAN process offers internal verification and tens to be always closer to the best laboratory measurements. [Christian Muller, Belgium]	Accepted. Text revised.
8-498	8	17	39	18	15	CCMIP models but are being developed. [M Daniel Schwarzkopf, United States of America]	Rejected, not of sufficient importance for current forcing estimates.
8-499	8	17	39	18	15	continuing this comment: use of stochastic cloud schemes in CMIP5 models is an advance ove previous generations. This should be mentioned here. [M Daniel Schwarzkopf, United States of America]	Rejected, not of sufficient importance for current forcing estimates.
8-500	8	17	39		55	This paragraph needs more background and explanation in order to be accessible to non-specialists. HITRAN is introduced without saying what it is (a spectral database?). The continuum is mentioned as a significant source of uncertainty, but it is not introduced, and there is no explanation of how the continuum differs from the radiative properties of the gases included in HITRAN. [Nathan Gillett, Canada]	Rejected, because of paragraph limitation and not so much related.
8-501	8	17	40	17	40	‘absorption data’, I would use ‘spectral properties’ as in the title. [David Paynter, United States of America]	Accepted. Text revised.
8-502	8	17	41	17	41	I think it should be made clear that these spectral properties come from HITRAN. At the moment it is not apparent what HITRAN is. Something along the lines of, ‘HITRAN (High Resolution Transmission) is a database which contains spectral properties for all of the major atmospheric gases and is widely used in radiative…’ [David Paynter, United States of America]	Accepted. Text revised.
8-503	8	17	50	17	50	‘Among continuum formulations’. This seems a bit unclear, do you mean the continuum of water, co2, ozone? Especially as MT CKD contains information on them all. [David Paynter, United States of America]	Accepted. Text revised.

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8-504	8	17	50	17	53	This sentence about new vs. old continuum formulations is rather cryptic. What counts as new/old? I agree newer versions of MT CKD certainly are better than say the Roberts continuum. However, as a statement about our understanding about the water vapor continuum it fails to capture that the water vapor continuum (which for the rest of this comment I will refer to as the continuum) is still an active field of research in radiative transfer with numerous unresolved issues, especially with respect to shortwave radiative transfer. These unresolved issues have a minor, but notable, impact on our ability to model the radiative transfer of water vapor in a changing climate. I understand that space issues are a concern, and the continuum is certainly far from the most pressing concern to our understanding of global climate, but I still think it warrants at least a sentence or so here mentioning some of the unresolved issues. I have summarized below my perspective on our current understanding(next 4 boxes, sorry that it is a bit lengthy!) [David Paynter, United States of America]	Accepted. Text revised.
8-505	8	17	50	17	53	There is still a lack of consensus over the cause of the continuum (dimers vs far wings) and accordingly at present there exists no satisfactory theoretical model of the continuum. Thus, any estimate of the continuum is ultimately reliant upon experimental data (for example the parameters in the commonly used MT CKD/CKD model are adjusted to fit measurement data and have little physical merit). A paper (Paynter and Ramaswamy et al., 2012) uses some recent continuum measurements to estimate the impact upon clear sky radiative transfer of uncertainty in the continuum. It showed that in the longwave that uncertainty in the continuum is generally rather small, but can result globally in 1 Wm-2 uncertainty in OLR and 2 Wm-2 in surface longwave downwelling radiation. Additionally, the uncertainty in changes in OLR and longwave down in a warmer climate due to the continuum were found to be small (see Figure 12, Paynter and Ramaswamy 2012) (i.e. less than 5%). [David Paynter, United States of America]	Accepted. Text revised.
8-506	8	17	50	17	53	The paper also investigated the impact of uncertainty in the continuum upon CO2 forcing (due to the spectral overlap between the two). It found that the impact of uncertainty upon the change in OLR brought about by a doubling of CO2 was quite small (<3 %), but moderate upon the increase in downwelling longwave radiation (15% in tropical regions). This could influence some of the 'rapid effects' for a doubling of CO2 which may be dependent upon surface fluxes. [David Paynter, United States of America]	Reject. Irrelated and not of sufficient of imprtance for the focing calculations
8-507	8	17	50	17	53	Recent measurements of the continuum (Paynter et al. 2007, 2009, Baranov et al. 2011, Ptashnik et al. 2011, 2012) all suggest that CKD/MT CKD is underestimating the continuum in the shortwave windows regions between 2000 and 8000 cm-1 (although the very latest version of MT CKD (2.5) has increased in the continuum between 2000 and 3000 cm-1). The absorption of shortwave radiation by the window regions is more sensitive to changes in water vapor than the water vapor bands. Paynter and Ramaswamy (2012) show that as the climate warms, the shortwave continuum accounts globally for 0.15 Wm-2 extra clear sky absorption per every kgm-2 extra increase in the water vapor column (~15% the total increase in clear sky shortwave absorption due water vapor). [David Paynter, United States of America]	Taken into account, text revised.
8-508	8	17	50	17	53	No climate models currently include this larger shortwave continuum, and most include no shortwave continuum at all. It should be noted that there still is considerable uncertainty associated with the measurement data of the continuum at atmospheric temperatures and thus there are fairly large uncertainties in these flux estimates (see Figure 12 of Payner and Ramaswamy 2012). So I think it would be best to summarize the shortwave continuum by simply adding a sentence saying that new measurement of the shortwave water vapor continuum suggest a larger continuum than previously thought and that calculations suggest may moderately increase our prediction of the absorption of shortwave radiation by water vapor as the climate warms, but that there exists a large uncertainty over the exact contribution of the shortwave continuum. [David Paynter, United States of America]	Accepted, text revised.
8-509	8	17	53	17	55	"Differences in absorption data from various HITRAN versions and updates on the water vapour continuum are very likely a small contributor to the uncertainty in RF of WMGHG." Since water vapour is not a WMGHG, a reference to its continuum really implies that it is the absorption overlap between WV continuum and WMGHG spectral lines that is not affected much by the continuum formulation. This is a very subtle point that will most likely missed by the reader, so I suggest that "updates on the water vapour continuum" be removed. [Lazaros Oreopoulos, United States of America]	Accepted. Text revised.
8-510	8	17	54	17	54	I agree with this statement, but I find including the water vapor continuum in the statement about WMGHG a	Accepted. Text revised.

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						bit misleading, as water vapor is not a WMGHG. Thus, the only way it can influence the RF of WMGHGs is through its spectral overlap with them (i.e. as I mention above, but has not been mentioned in the text). So I think if the continuum is mentioned in this statement it should be made clear that updates to the continuum could still be important for processes that depend upon the amount of shortwave radiation absorbed by the atmosphere (cf my discussion above) . [David Paynter, United States of America]	
8-511	8	17				Section 8.3: The key results in section 8.5.1 and Table 8.7 belong in this section. The authors should consider using the materials in section 8.5.1 to lead off section 8.3. [Government of United States of America]	Rejected, the synthesis made in 8.5.1 covers both anthropogenic and natural forcing and therefore is more appropriate after these section in our view.
8-512	8	17				Section 8.2.3.6 Aerosols: The authors should consider changing the title to "Anthropogenic Aerosols" to be distinguished from "Natural Aerosols" from volcanoes. [Government of United States of America]	Rejected, this is small general section on aerosols
8-513	8	17				Section 8.3: It would be useful to have more comments on the qualitative confidence levels throughout this section. [Government of United States of America]	Taken into account, text throughout the section improved on this aspect.
8-514	8	17				Section 8.3 Many of the sections pointed to here are incorrect I think - they reference to 2.4.1.1.? But they should be 2.2.1.1.? [Kate Willett, United Kingdom]	Taken into account, cross-referencing corrected.
8-515	8	18	1	18	2	"LBL models using the HITRAN dataset as an input are the benchmark of GHG radiative transfer models." (Not ALL radiative transfer models, certainly not for SW or cloud impact on SW calculations! But true for GHG calculations.) The authors should consider revising the text to account for this. [Government of United States of America]	Accepted. Text revised.
8-516	8	18	2			This sentence doesn't make sense. [Nathan Gillett, Canada]	Accepted. Text revised.
8-517	8	18	10			A 30% difference sounds large. What are the implications of such a large difference for models? [Nathan Gillett, Canada]	Noted, as stated our forcing results for WMGHG rely on models better validated with LBL models
8-518	8	18	11	18	11	insert "tropospheric" before "water vapour" [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised.
8-519	8	18	13			It is not clear "the errors between 3% and 200%" refer to RF by stratospheric water vapor or ozone, or the two together. Please clarify. [Government of United States of America]	Accepted. Text revised.
8-520	8	18	14	18	14	"where" should be "were" [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised.
8-521	8	18	14	18	14	replace "where" with "were" [Räisänen Petri, Finland]	Accepted. Text revised.
8-522	8	18	15	18	15	It might be worth referencing Maycock and Shine (2012) who investigated the difference between LBL, narrowband and broadband codes estimating stratospheric water vapor radiative forcing. They also show that narrowband radiation codes seem to better than broadband radiation codes commonly used in GCMs. They also highlight the need of having to use a suitably high spectral sampling in a LBL radiation code [David Paynter, United States of America]	Rejected, we shortened the discussion for stratospheric water vapour and we therefore not find space to expand the discussion on thisissue
8-523	8	18	15			The H20 LBL errors from Forster et al. 2011b are likely wrong and too large - they were not reproducable. It's in a paper but I can't remember which one as I'm on a plane and all my long-term memory is all on google these days. Keith Shine is an author though! [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Taken into account by revising the sentence
8-524	8	18	17	18	27	The discussion of the impact of clouds on radiative forcing should include the results found by Minschwaner et al (JGR, 1998, pp. 23,243-23,253) that was based on observed cloud properties, where the impact on RF was 30-40% compared to clear skies, and was dependent on the GHG. Similarly, the previous discusion in this chapter is concerned with effects that are on the order of 2-5%, and in this regard it should be pointed out that the above paper also found impacts of this magnitude on global average RF due to the latitudinal gradients in GHG and also their vertical gradients above the tropopause, i.e. these gases are called "well mixed" but in fact there are horizontal and vertical gradients that matter at the few percent level. [Kenneth Minschwaner, United States of America]	Rejected, the reference is more than 10 years ago.
8-525	8	18	17		27	This para seems to have been introduced to counter the argument (repeated above at page 3) regarding the	Noted, we have also added that the uncertainties for

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						influence of clouds on the uncertainty in GHG forcing. Reference to Forster 2005 for HFC-34a shows that the several models that included clouds in their forcing calculations found little increase in model-to-model spread due to inclusion of clouds in the model atmosphere. This finding is important and belongs here. I am pleased to learn of it. It may be that I have overstated the consequence of omission of clouds from the estimated uncertainty in CO2 forcing or ghg forcing more generally. However this single intercomparison study would hardly seem to be definitive on the subject. I note as well that the para concedes that the uncertainty in RF calculations in many gcms can be substantially higher. [Stephen E Schwartz, United States of America]	the GCMS both are respect to radiative transfer codes and the meteorological data (including clouds).
8-526	8	18	17			"It is shown that cloud can greatly reduce the magnitude of radiative forcing due to greenhouse gases by about 25% (Forster et al., 2005; Zhang et al., 2011)" Satellite observations of tropospheric ozone radiative effect, which is sensitive to cloud distributions and magnitude, have also been used to look at the impact of clouds on RF. In particular, Worden et al, 2011, Fig 5, show reduction in tropospheric ozone RF due to clouds from 25 mW/m^2/DU to about 17 mW/m^2/DU at 250 hPa using instantaneous radiative kernels, which are computed using LBL codes and constrained with directly measured spectral radiances. These kernels represent good benchmarks for RF codes within GCMs. The authors should consider inserting a sentence or citation to these activities since the AR4. One potential addition could be: "It is shown that cloud can greatly reduce the magnitude of radiative forcing due to greenhouse gases by about 25% (Forster et al., 2005; Zhang et al., 2011, Worden et al, 2011)" Worden, H. M., Bowman, K. W., Kulawik, S. S., and Aghedo, A. M.: Sensitivity of outgoing longwave radiative flux to the global vertical distribution of ozone characterized by instantaneous radiative kernels from Aura-TES, J. Geophys. Res., 116, doi: 10.1029/2010JD015101, http://dx.doi.org/10.1029/2010JD015101, 2011. [Government of United States of America]	Accepted. Text revised.
8-527	8	18	18	18	19	I like to point out that I cannot verify if all my suggestions have been considered (I verified only some of them), since the pages, lines and numbers of the Figures and Tables have been changed. Consequently, from this line up to the end I leave the same comments as for the FOD (and the old numbers of pages and lines). 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]	Taken into account by showing pressure levels much clearer.
8-528	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]	Taken into account, figure removed.
8-529	8	18	26	18	26	I think you need to make it clearer why 10%. I assume it comes from 5% lbl uncertainty + 5% due to cloud overlap from the Forster et al. study. [David Paynter, United States of America]	Taken into account, by modifying the text and stating clearer why GCMs have higher uncertainty.
8-530	8	18	26	18	26	given that the agreement among LBL models is within 5%, or 2-3% for CO2 at the tropopause level, what is the basis for the 10%uncertainty in WMGHG radiative forcing? Uncertainties in the atmospheric distribution of clouds (and their properties) and water vapour? This should be clarified. [Räisänen Petri, Finland]	taken into account, by refering to studies for 5% differences in radiative transfer and 5% for effect of clouds, but not sufficient information to quantify the other uncertainties. It is stated that 10% uncertainty from AR4 is retained based on the available scientific literature.
8-531	8	18	27	18	27	I am not sure this is an 'uncertainty' more errors brought about by necessary parameterizations in order to make radiation codes practical for use in GCMs. [David Paynter, United States of America]	Rejected, we refer to differences in estimates of forcing as uncertainty. This can of course to some extent be quantified, but seldom done.
8-532	8	18	31	18	36	The authors should consider inserting a statement that the forcings are calculated using observed	Accepted

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						concentrations and refer to Table 8.3. It may be worthwhile also to contrast it with the ozone and aerosol results that are model based (p. 21, lines 40-42). [Government of United States of America]	
8-533	8	18	31	18	36	At the end of this paragraph, the authors should consider providing the AR5 estimate of RF for WMGHG. [Government of United States of America]	Accepted
8-534	8	18	35	18	36	Is this coinsistent with LBL estimates? [Nathan Gillett, Canada]	Noted: Yes this is consistent
8-535	8	18	38	18	42	It is not clear whether this uncertainty is quantified and incorporated into the RF error bars discussed for each WMGHG (and the total). If so, how is this uncertainty quantified. By calculating a range of RF estimates for a range of baseline years? If so, what range of years is used? [Government of United States of America]	Taken into account: The different factors contributing to the uncertainty will be presented. However the RF definition will still use the single year 1750 as the baseline and not a range.
8-536	8	18	41			"climate and natural emissions, and also ..." Clearly natural emissions have varied, but do we have clear evidence (other than Ruddiman) that there was human influence prior to 1750? The problem is that this sentence seems to conclude the latter without any qualifier on uncertainty. This would be the place to put in the added uncertainty due to late holocene natural cycles in GHG that are likely to continue to 2010. [Michael Prather, United States of America]	Noted: This sentence does not quantify the magnitude of the human influence so we feel justified in retaining it.
8-537	8	18	46		47	Repeats the myth that CO2 increase from 1750 - 1850 was due to industrialization; it was due large to land clearing. [Stephen E Schwartz, United States of America]	Reject: We disagree that the text makes this statement
8-538	8	18	46			"The lower atmospheric mixing ratio ..." - be careful, global mean includes strat. And is less. [Michael Prather, United States of America]	Accepted
8-539	8	18	47	18	47	Section 2.4.1.1 says no such thing. These incorrect references are very unprofessional. How can reviewers comment on these matters if they can't find the information sources? [John McLean, Australia]	Noted: Revised chapter references
8-540	8	18	50			Unfair; need to tell us the page or give us the formula. [Stephen E Schwartz, United States of America]	Taken into account: specific reference in text and formula provide in SM
8-541	8	18	52	18	52	"Using the simple form from Ramaswamy et al. (2001)". This is very vague. This particular reference contains three empirical formulas for CO2 forcing, see Table 6.2 therein. It is apparent from the value of 1.82 Wm-2 provided here that the first of the three formulas is used (for 390.5-112 ppm pre-industrial and 390.5 ppm present-day values, an increase factor of ~1.4) which is from Myhre, G., E.J. Highwood, K.P. Shine, and F. Stordal, 1998: New estimates of radiative forcing due to well mixed greenhouse gases. Geophys. Res. Lett., 25, 2715-2718. This is then the proper reference that should be provided, or it should be at least added to Ramaswamy et al. (2001). [Lazaros Oreopoulos, United States of America]	Taken into account: specific reference in text and formula provide in SM
8-542	8	18	52	18	54	Is the Ramaswamy et al. (2000) calculation consistent with LBL estimates of the CO2 forcing? [Nathan Gillett, Canada]	Noted: Yes this is consistent
8-543	8	18	52			For clarity, could the "simple formula from Ramaswamy et al. 2001" be included explicitly here? It is referred to a couple times in this section (p 18, ln 52; p 19, ln 35) but not given. [Government of United States of America]	Taken into account: specific reference in text and formula provide in SM
8-544	8	18	53			please put formal uncertainty on the 1.82 W/m2, including the 10 ppm range of natural if you want to call the RF "anthropogenic." [Michael Prather, United States of America]	Accepted: Uncertainties added
8-545	8	18	54			Note here that this is 10% is 90% confidence interval (just to remind on first use). [Michael Prather, United States of America]	Rejected: 90% cl is standard so won't repeat
8-546	8	18	54			10%; As I argue at page 8-105, I think this estimate is quite overconfident. [Stephen E Schwartz, United States of America]	Rejected: We assess the uncertainty in the line-by-line calculations to be 10%.
8-547	8	18	57	19	2	The text in the SPM (page SPM-7, lines 29-30) is not consistent with the main text. Figure 8.6 panel c illustrates that the relative contribution of CO2 has hardly or even not increased since the 1980s, in contrast to the formulation in the SPM. We suggest to change the main text in chapter 8 accordingly to make it consistent with the findings in Figure 8.6 and change the text in the SPM accordingly. [Government of Netherlands]	Rejected: Figure 8.6c does not show that the relative contribution of CO2 has hardly increased. It shows that the rate of change of CO2 RF is slightly increasing (i.e. a slight acceration of the absolute CO2

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							RF).
8-548	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]	Accepted
8-549	8	19	8	19	11	Who are you trying to fool? The warming influence of CO2 is logarithmic with concentration, which means a decreasing incremental warming it causes for each unit increase in concentration. Your graph, with an incomprehensible Y-axis scale for most readers, seems to be trying to claim the opposite. [John McLean, Australia]	Rejected: The CO2 concentration is increasing more than linearly therefore even with a logarithimic dependence the rate of change of RF is still increasing.
8-550	8	19	17	19	17	Andrews and Forster (2008) compare AF and RF for a set of slab models. [Jonathan Gregory, United Kingdom]	Rejectd. These models did not provide the direct comarison needed. We will include recent results from Vial et al. 2013.
8-551	8	19	32			Please put some uncertainty on today's CH4, given the lack of vertical sampling, it has got to be at least 1%. [Michael Prather, United States of America]	Accepted: We now include this.
8-552	8	19	33	19	33	This is becoming a farce. Another incorrect reference, this time to section 2.4.1.1.2 which doesn't even exist. [John McLean, Australia]	Editorial: Revised chapter references
8-553	8	19	35			Unfair; need to tell us the page or give us the formula. [Stephen E Schwartz, United States of America]	Taken into account: specific reference in text and formula provide in SM
8-554	8	19	36			This is where the added CH4 uncertainty (40 ppb in holocene) should be brought in if one interprets this a anthropogenic RF. [Michael Prather, United States of America]	Accepted: Will have added this.
8-555	8	19	38	19	38	Is the change in oxidizing capacity discussed elsewhere? There does not appear to be mention of how significantly the oxidizing capacity has changed since AR4. What is the level of confidence in the change in oxidizing capacity? [Government of United States of America]	Accepted: We have dropped this
8-556	8	19	38			I do not think that this statement on changing oxidizing capacity can be made - drop it - it must be all sources. Given the Montzka etal 2011 work on CH3CCl3, there is no evidence for any change in the OH lifetime. [Michael Prather, United States of America]	Accepted
8-557	8	19	40	19	40	And another incorrect reference. This says a lot about your attention to detail! [John McLean, Australia]	Editorial: Revised chapter references
8-558	8	19	49			Please put uncertainty on 1750 N2O values and consider the natural variability. [Michael Prather, United States of America]	Accepted: We have done this
8-559	8	19	50	19	50	You're really excelling yourself. Another false reference to a non-existent section. [John McLean, Australia]	Editorial: revised chapter references
8-560	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]	Rejected, no reference to Figure in Chapter 2 on this line.
8-561	8	19				Section 8.3.2.4: With respect to ozone depleting compounds in this section - The authors should consider giving mention of the RF impact of these compounds due to ozone depletion. [Government of United States of America]	Rejected: We don't want to attribute the ozone depletion to individual ODSs here.
8-562	8	20	1	20	1	add 'and its amendments and adjustments' – the Montreal Protocol itself would not have the observed effect on CFCs [Rolf Müller, Germany]	Accepted
8-563	8	20	16	20	17	This sentence might be factually correct (ie. the 4AR did make those statements) but where's the error margin and why should any credibility be placed on the mean result of a collation of climate models that don't include all forces with 100% accuracy. (Why would you need an ensemble of models anyway if one was 100% accurate? And if by chance one did exist, it may produce results that are outliers compared to the mean of the results of other models.) [John McLean, Australia]	Rejected, no mention of climate models in this paragraph it is about fully fluoronated compounds and comment not of relevance for this paragraph.
8-564	8	20	18			Why not just give a number for PFC+SF6 of 0.01 W/m2 instead of 0.3%? seems odd to see different units.	Accepted

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						[Michael Prather, United States of America]	
8-565	8	20	27	21	11	Error margins for the concentrations of all elements please. [John McLean, Australia]	Accepted: Uncertainties from Ch.2 added where available
8-566	8	20	27	21	11	Please indicate each element for which the claimed concentrations have been independently audited (compared to a peer-review that was no more than a cursory check for large errors.) [John McLean, Australia]	Rejected: The data is taken from Ch.2. where specific references are given.
8-567	8	20	27			First use of ppt in this chapter. The authors should consider defining it here. [Government of United States of America]	Accepted
8-568	8	20	27			Can you put some uncertainties in the abundances of this table, at least for the big ones? [Michael Prather, United States of America]	Accepted: Uncertainties from Ch.2 added where available
8-569	8	20	29	20	29	The reference to Table 2.12 seems incorrect. Table 2.12 contains no such data. [Lazaros Oreopoulos, United States of America]	Editorial: Revised chapter references
8-570	8	20	31	20	31	“Radiative efficiencies for the minor gases are taken from Hodnebrog et al. (2012). What about the other gases? What is the source for their radiative efficiency values? [Lazaros Oreopoulos, United States of America]	Taken into account: We refer to the formulae in Mhyre et al. 1998
8-571	8	20	32			I applaud radiative efficiency. This is a useful concept. 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 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. Check your number for CO2: 1.37 x 10^-5 W m-2 ppt-1; 13.7 x 10^-6 W m-2 ppt-1; 13.7 W m-2 ppm^-1 (it is late at night Nov 30 as I do this). Seems three orders of magnitude too high. [Stephen E Schwartz, United States of America]	Accepted: CO2 efficiency revised
8-572	8	20				Table 8.3: Take the RFs of 2011 as an example, CFCs+HCFCs+(CF4+C2F6+CH3CCl3+CCl4)=0.333, which is still lower than the RF of 0.357 for Halocarbons. The authors should consider explaining that the RFs of other halocarbons are included and indicate those halocarbons if available (Table 8.A.1?). [Government of United States of America]	Rejected: The reviewer hasn't included HFCs in the calculations. When these are included the numbers do add up.
8-573	8	21	14	21	42	There is little mention of the updated estimate of stratospheric water vapor radiative forcing. The authors should consider including the new science from such relevant references as: Myhre, G., J. S. Nilsen, L. Gulstad, K. P. Shine, B. Rognerud, and I. S. A. Isaksen (2007), Radiative forcing due to stratospheric water vapour from CH4 oxidation, Geophys. Res. Lett., 34, L01807, doi:10.1029/2006GL027472. Myhre, Gunnar and Kvalevag, Maria and Rädel, Gaby and Cook, Jolene and Shine, Keith P. and Clarke, Hannah and Karcher, Fernand and Markowicz, Krzysztof and Kardas, Aleksandra and Wolkenberg, Paulina and Balkanski, Yves and Ponater, Michael and Forster, Piers and Rap, Alexandru and Rodriguez de Leon, Ruben (2009) Intercomparison of radiative forcing calculations of stratospheric water vapour and contrails. Meteorologische Zeitschrift, 18 (6), pp. 585-596. DOI: 10.1127/0941-2948/2009/0411. [Government of United States of America]	Accepted: Add ed these references.
8-574	8	21	16	21	18	Surely if models underestimate preindustrial tropospheric ozone, then assuming their present-day levels are correct, they would overestimate the change in tropospheric ozone and overestimate its radiative forcing. Wouldn't this imply that the 95th percentile should be lower than an estimate obtained directly from the models? [Nathan Gillett, Canada]	Accepted: "Understimate" should have been "overestimate". This is corrected.
8-575	8	21	17	21	17	should "underestimates" be "overestimates"? [Räisänen Petri, Finland]	Accepted

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8-576	8	21	17			Should "underestimates" be changed to "overestimates" in this sentence? Presumably, the authors are referring to the models not being able to reproduce the low Montsouris O3 observations. Thus the models "overestimate" the pre-industrial O3. [Government of United States of America]	Accepted
8-577	8	21	23	21	30	Global average number are given here for the RF of tropospheric and stratospheric origin and from precursors and ODSs. A figure in the submitted paper (Shindel et al., 2012a) on the other hand shows a large latitudinal gradient in these forcings. The negative overall forcing is located completely below 40S. The positive forcings of the ODSs are much more homogeniously disctributed. I think this warrents a discussion here (and a statement in the ExSum), since it is very relevant for the effects on climate (the terms not simply cancel each other). [Guus Velders, Netherlands]	Rejected. Not appropriate to add detail on this here.
8-578	8	21	23	21	33	The split by altitude and by forcing agent is very useful. It would even be better if the other 4 numbers can be provided: A. tropospheric ozone from precursors B. stratospheric ozone from precursors C. tropospheric ozone from ODS D. stratospheric ozone from ODS rather than giving (A+B), (C+D), (A+C), and (B+D). [Government of United States of America]	Rejected: There is not enough information in the literature to break the results down further
8-579	8	21	23			Studies that are referenced here should be cited here. [Government of United States of America]	Accepted: Added references
8-580	8	21	27	21	29	If the net is calculated as strat + trop (perhaps it isn't), its uncertainty should be larger than separate uncertainties. [Jonathan Gregory, United Kingdom]	Rejected: The uncertainties are assessed from total ozone forcing
8-581	8	21	27	21	30	It is unfortunate that neither here nor in Table 8.4 is there a breakdown between SW and LW radiative forcing. Is such information not available? [Lazaros Oreopoulos, United States of America]	Rejected: Table 8.4 does include separate LW and SW forcing where available in the literature
8-582	8	21	27	21	30	The numbers given are not found in Shindell et al. (2012d), but in 2012a. [Guus Velders, Netherlands]	Editorial: Revise reference
8-583	8	21	28	21	28	Should "forcing agent" be replaced by "emitted species" here? [Larry Horowitz, United States of America]	Accepted
8-584	8	21	28	21	28	Is this the correct reference? Should it be Shindell et al. (2012a)? [Larry Horowitz, United States of America]	Editorial: revised reference
8-585	8	21	28	21	30	Say more about how these numbers are obtained. In the stratosphere, they cannot be unambiguously separated (e.g., CH4 and the ODS changes around 2000 are non-linear, see Portmann et al., Phil. Trans. R. Soc. B, 2012, doi:10.1098/rstb.2011.0377). [Robert Portmann, United States of America]	Rejected: The methods for obtaining these numbers are described more fully in the relevant subsections
8-586	8	21	30	21	30	This is (presumably) the effect of ODS on ozone, not the direct RF of the ODS themselves. Clarify. [Larry Horowitz, United States of America]	Accepted: Clarified
8-587	8	21	35	21	36	Give the sign of the disagreement between chemistry models and 19th century ozone observations, and some references. [Nathan Gillett, Canada]	Taken into account: referred to 8.2.3.1
8-588	8	21	35	21	36	This sentence repeats ealier discussion on page 8-15, line 7-20; consider adding a cross reference to Section 8.2.3.1 [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Taken into account: referred to 8.2.3.1
8-589	8	21	40	21	41	WMGHGs [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Editorial
8-590	8	21	40	21	42	The authors should consider moving the sentence to the beginning of this sub-section 8.3.3 (line 16) to improve clarity and relevance. [Government of United States of America]	Accepted:
8-591	8	21	40			What studies are the exceptions to only using models? What are the implications of neglecting recent observations of tropospheric and stratospheric ozone on assessing present day ozone and ozone RF?	Taken into account: This is discussed more fully.

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						[Government of United States of America]	
8-592	8	21				Section 8.3.3.1: The authors should consider including a brief discussion of long wave vs short wave forcing in the troposphere. [Government of United States of America]	Rejected: Not appropriate here.
8-593	8	22	4	22	21	This paragraph is largely repetitive of an earlier paragraph in Sec. 8.2. However, the estimate of the tropospheric ozone increase from 1850 to 2000 of 98 (+/-20) Tg is inconsistent with what was mentioned previously: 95 (+/-50) Tg. The authors should be sure to use the correct values in the final revision. [Government of United States of America]	Taken into account: Paragraph revised
8-594	8	22	5	22	21	The text about Figure 8.7 talks about Tg of tropospheric ozone increase, but Figure 8.7 itself is in DU units -- either the figure should be changed to Tg units, or vice versa, to ensure consistency and allow for more clarity of the text. [Government of United States of America]	Noted: Figure 8.7 has been removed.
8-595	8	22	5			Bowman et al, 2012 also estimated ozone RF using a combination of satellite and the ACCMIP multimodel runs. The authors should consider including this reference in the citation list, particularly related to ACCMIP results. Bowman, K., Shindell, D., Worden, H., Lamarque, J. F., Young, P. J., Stevenson, D., Qu, Z., de la Torre, M., Bergmann, D., Cameron-Smith, P., Collins, W. J., Doherty, R., Dalsøren, S., Faluvegi, G., Folberth, G., Horowitz, L. W., Josse, B., Lee, Y. H., MacKenzie, I., Myhre, G., Nagashima, T., Naik, V., Plummer, D., Rumbold, S., Skeie, R., Strode, S., Sudo, K., Szopa, S., Voulgarakis, A., Zeng, G., Kulawik, S., and Worden, J.: Observational constraints on ozone radiative forcing from the Atmospheric Chemistry Climate Model Intercomparison Project (ACCMIP), Atmos. Chem. Phys. Discuss., 12, 23603-23644, doi:10.5194/acpd-12-23603-2012, 2012. [Government of United States of America]	Taken into account: Discussion of this paper added
8-596	8	22	7	22	7	"approximately 98 Tg" - page 8-14 line 30 states "approximately 95 Tg" from the same dataset. Please ensure consistency here [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Taken into account: Numbers revised
8-597	8	22	8	22	8	"2000 estimate" should read "2000 burdens" [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Accepted
8-598	8	22	9	22	9	"quite similarly" - note that the standard deviation in the O3 change is also almost 20 Tg (Table 4 of Young et al 2012 indicates 17 Tg), quite a large spread. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Taken into account: This paragraph has been reworded
8-599	8	22	12			Is the correct figure: 0.04 W/m^2 or 0.40 W/m^2? [Government of United States of America]	Noted: The text is correct as it stands
8-600	8	22	13			Bowman et al, 2012 also showed that ACCMIP results constrained with satellite observations estimated an RF of ~0.4 W/m^2, which is consistent with Skeie (2011a). The authors might consider reflecting this fact in the text. [Government of United States of America]	Taken into account: Discussion of this paper added
8-601	8	22	14	22	14	Publication year missing for Sövde et al. [Räisänen Petri, Finland]	Editorial: Year added
8-602	8	22	14			What is the 'Sovde et al' result? Presumably in the Sovde et al (2011) paper? Do you mean Skeie et al? I think this refers to the O3 RF estimate from the Oslo model. This is clearly a useful study, but it seems odd to say that the best estimate of O3 RF is the average of one 17 model study and one single model study, especially as the 17 model study also includes the Oslo model (in almost the same set-up). The net result is not at issue, as both studies found a value of 0.4 W/m2, but it seems unreasonable to weight these two studies equally. [David Stevenson, United Kingdom]	Accepted: Weighted mean used.
8-603	8	22	15			Does this uncertainty include the bias between observed and simulated present day ozone distributions? If not, then the uncertainty estimate is likely underestimated. It appears that observations are playing no meaningful role in the ozone RF calculation, which neglects the dramatic increase in observations over the last decade relevant to ozone. The authors should consider revising the text to account for both of these issues. [Government of United States of America]	Noted: There is little evidence for a systematic bias in the upper tropospheric ozone. The increased availability of ozone observations doesn't yet provide a constraint on the pre-industrial to present day RF
8-604	8	22	17	22	17	“The tropospheric ozone RF is sensitive to the assumed ‘pre-industrial’ levels”. What are the implications of	Taken into account: Further discussion of this added

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						the difficulty to determine the ‘pre-industrial’ levels of tropospheric ozone? [Michael Trainer, United States of America]	in the text, both in section 8.2 and 8.3.3.
8-605	8	22	17	22	17	“The tropospheric ozone RF is sensitive to the assumed ‘pre-industrial’ levels”. What are the implications of the difficulty to determine the ‘pre-industrial’ levels of tropospheric ozone? [Michael Trainer, United States of America]	Taken into account: Further discussion of this added in the text.
8-606	8	22	17	22	21	It would be necessary to reassess nineteenth century ozone data as a lot of observations were rejected when it was dicoverd that the chemical measuring techniques used were not ozone specific but measured the total amount of oxidants, the tendancy of the best stations was then (in the late nineteenth century) to be biased towards low ozone values as these were a mark of the quality of the technique used. The IPCC draft is right in preferring model data to the partial observations available. The reassessment of the sixty first years of ozone observations would however be a wothwhile scientific research subject. The most complete reanalysis of data is: R. Bojkov, surface ozone during the second half of the nineteenth century, J. Clim. and Ap. Meteorology, vol 25, 343-352, 1986, and he points also to a possible undervaluation in the best data set (Parc Monsouris) due to a correction for the effect of atmospheric formaldehyde. [Christian Muller, Belgium]	Taken into account: Further discussion of this added in the text.
8-607	8	22	20	22	21	How is the fact that models are unable to model observed ozone trends at many locations taken into account in the estimate of uncertainty? Also, Stevenson et al. (2012), based on the ACCMIP derived RFs, estimates an error of +/-30% and a model spread of +/- 17%. This error estimate is not discussed here. [Katharine Law, France]	Taken into account: We have no quantitive way of taking into account comparisons between the recent observed surface trends for the calculation of the pre-industrial to present day RF. The Stevenson 30% uncertainty which we adopt here is to account for this.
8-608	8	22	23	22	24	The forcing due to short-lived gases is not shown in Figure 8.8. [Nathan Gillett, Canada]	Taken into account: Removed phrase "short-lived gases"
8-609	8	22	26			I am surprised this works for ozone given Lacis's work cited above,page 8-13, line 13. [Stephen E Schwartz, United States of America]	Taken into account: Addedc aveat on vertical profiile
8-610	8	22	28			Stevenson et al 2012 has a normalised RF of 0.042 Wm-2DU-1 [David Stevenson, United Kingdom]	Taken into account: Used final Stevenson numbers
8-611	8	22	41	22	43	NB This breakdown of the O3 RF into components from Stevenson et al 2012 (ACPD) will likely change in the revised version, as I have some reservations about my methods. The fraction attributable to CH4 is likely to increase, and that from NOx decrease. I will keep IPCC authors posted of these revisions. [David Stevenson, United Kingdom]	Taken into account: Used final Stevenson numbers
8-612	8	22	46	22	46	replace "effect the concentrations" by "affect the concentrations" [Jean Poitou, France]	Editorial
8-613	8	22	48	22	48	replace "are line" by "are in line" [Jean Poitou, France]	Editorial
8-614	8	22	51	22	51	It might be worth briefly stating the mechanism by which tropospheric ozone affects the natural uptake of carbon dioxide (decreased plant productivity?) here so that it is obvious to the reader without trawling back through previous sections. [European Union]	Accepted: Added this.
8-615	8	22	51	22	53	Please clarify the cause and effect in this paragraph. Is the O3 damaging vegetation, and thus impacting CO2 uptake? [Government of United States of America]	Accepted: Have clarified this
8-616	8	22	51		53	This seems like a very large and confident number from only one UKMO modeling study. How about noting the effect and not giving the W/m2. Also could not find anything on ozone-CO2 in 8.3.2.1. do you mean 8.2.3.1? Also there should be something on this in chapter 6? [Michael Prather, United States of America]	Taken into account. Added note on confidence and updated section reference
8-617	8	22	53			Perhaps say that this is the case in a framework where we attribute radiative forcing to emissions. [Nathan Gillett, Canada]	Accepted
8-618	8	22		22		Not clear as to why the confidence range for trop ozone is increased as much, since O3 RF at 1850 is ~0.05 and would be less at 1750. [M Daniel Schwarzkopf, United States of America]	Noted: This is to do with the difference between 1750 ozone (which was significant) and present day
8-619	8	23	1	23	10	We are confused by the value and range given for troposphere total for AR4 "0.35 (-0.1, +0.3)". Please apply the same format for all values and ranges given in this table. Please also note the "REF" and "R2" which	Editorial: Changes made

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						strangely appear in this table. [Thomas Stocker/ WGI TSU, Switzerland]	
8-620	8	23	3	23	3	Table 8.4: The units of NRF are in mW not W. The values in this column are not consistent with the text (page 8-22, line29) for the Sovde study. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Accept: Changes made
8-621	8	23	3			Table 8.4. Does WMO really contain two different estimates of ozone RF with difference signs? [Nathan Gillett, Canada]	Noted: Yes
8-622	8	23	3			The units of NRF should be mW m-2 DU-1 in Table 8.4. The RFs should also have units (W m-2). Explain REF/R2 [David Stevenson, United Kingdom]	Accepted: We made these changes
8-623	8	23	9	23	9	"since 2010" is probably not correct [Räisänen Petri, Finland]	Accepted: Changed to "since 2000"
8-624	8	23	14	23	17	Presumably this is meant to read "decreases …. since 1960 …"? The discusion about the RF related to stratospheric ozone is confusing. [Katharine Law, France]	Taken into account: Re-phrased.
8-625	8	23	20	23	21	“The observed and model mean ozone changes gave RF of different signs (see Table 8.4).” It would have been more informative to understand why this is the case by including the SW and LW breakdown of observed RF. [Lazaros Oreopoulos, United States of America]	Rejected: We do not break the WMO results down further.
8-626	8	23	25	23	27	The best estimate for RF from stratospheric from 1979 to late 1990s is supposed to be –0.05 W m–2. Then it I stated that: « Assuming the forcing follows the equivalent effective stratospheric chlorine these values would be increased by around 30% to account for depletion before 1979 and recovery since 1998 (Cionni et al., 2011; Hansen et al., 2002; Skeie et al., 2011a). » I don't understand how you go from 0.05 to 0.1 W m–2 later on. Ozone depletion and associated stratospheric ozone RF from 1960s to the end of 1970s isclearly much smaller the ozone and RF change from end of 1970s to the end of 1990s. I Where does this 0.1 come from? [slimane bekki, France]	Taken into account: The stratospheric ozone RF has been revised.
8-627	8	23	27		30	The text should make clear that this is a model result, based on CCMs. I don't think 'recovery' (at least in the sense of increasing ozone) has been detected in global column ozone yet. [Nathan Gillett, Canada]	Accepted: This is clarified.
8-628	8	23	31	23	31	replace "due taking into account" by "due to taking into accoun" [Jean Poitou, France]	Editorial: Changes made
8-629	8	23	34	23	36	These indirect effects are in particular caused by the most dramatic ozone loss in the stratosphere, namely the polar ozone loss in Austral winter and spring (i.e. the ozone hole). This is s different effect than mid-latitude ozone loss and shold be emphazised. [Rolf Müller, Germany]	Taken into account. This is now rephrased.
8-630	8	23	38		40	This text seems to be citing itself. Give a section reference if this comes from elsewhere, or just cite the Shindell et al. study if that is the source. [Nathan Gillett, Canada]	Taken into account. This is now rephrased.
8-631	8	23				Table 8-4 is not complete in terms of numbers and references More numbers and references are provided in the text. It is a shame that the following paper cannot be cited: Hassler, B., P. J. Young, R. W. Portmann, G. E. Bodeker, J. S. Daniel, K. H. Rosenlof, and S. Solomon, Comparison of three vertically resolved ozone data bases: climatology, trends and radiative forcings, Atmos. Chem. Phys. Discuss., 12, 26561-26605, 2012. [slimane bekki, France]	Taken into account: Text and table are reconciled. We cannot cite this paper due to the cut-off.
8-632	8	23				Table 8.4 - The authors should consider defining the LW (long wave), SW (short wave), NRF (nomalized radiative forcing) acronyms, as well as explaning REF and R2 in the table caption. Other suggestions to improve the clarity of the table include: -Move NRF column to the far right of the table, since it is a "whole atmosphere" value. As presently constructed, it could be interpreted that NRF is a measure of tropospheric O3 NRF only. - Use vertical lines or some other type of visual separation to distinguish tropospheric, stratospheric, and total column RF values. [Government of United States of America]	Taken into account: Captions and layout clarified
8-633	8	23				Table 8.4: Bowman et al 2012 provides a satellite constrained ACCMIP estimate of 0.40 +- 0.07 W/m^2 and	Taken into account: Discussion of this paper added

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						.369 +- .04 W/m^2. The authours should consider including this in the table. [Government of United States of America]	
8-634	8	23				Table 8.4: The authors should double check the units for NRF, are they (W m-2 DU-1) or (W mol-1) for the numbers shown in the table? From the magnitudes of NRF in Line 29 (Page 22), it seems the units should be (W mol-1) for NRF in Table 8.4. [Government of United States of America]	Taken into account: Numbers revised
8-635	8	24	3	24	4	Air that enters the stratosphere through the tropical tropopause has evenually to leave it through the extratropical tropopause. Changes in the extratropical dynamical processes that are involved in this may occur due to anthropogenic forcing: a poleward shift of jet streams for example. Raising of tropopause height may also be mentioned. Is it clear that these have neglible effect? Moreover, Chapter 12 (page 12-40, lines 2-19, identifies a likely increase in the stratospheric Brewer-Dobson circulation, which should be classed as an anthropogenic effect. [Adrian Simmons, United Kingdom]	Rejected: This is a feedback not a forcing
8-636	8	24	4			"oxidation of methane and molecular hydrogen." - do not forget to note that H2 can also drive changes in strat H2O and we do not report trends in this compound! Although recent changes in H2 are no more than 0.02 ppm (V.V. Petrenko et al, 2012, Atmos. Chem. Phys. Discuss., 12, 18993-19037). [Michael Prather, United States of America]	Accepted: Added this.
8-637	8	24	4			"oxidation of methane and molecular hydrogen." - must not forget that H2 can also drive changes in strat H2O and we do not report trends in this compound! Although recent changes in H2 are no more than 0.02 ppm (V.V. Petrenko et al, 2012, Atmos. Chem. Phys. Discuss., 12, 18993-19037). [Michael Prather, United States of America]	Accepted: Added this.
8-638	8	24	7	24	7	Joshi and Shine (J Clim 2003) also looked at this effect [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Noted:
8-639	8	24	7	24	8	stratospheric water vapour may change through changes in tropical tropopause temperatures (see Randel et al., JGR, 2006, and discussion in 2.2.2.1). This does not constitute an anthropogenic forcing but a climate feedback mechanism. I do not think this issue is covered by referring to 'changes in dynamics'. [Rolf Müller, Germany]	Taken into account: Add ed the term "feedback" to this description
8-640	8	24	19	24	21	The last paragraph here seems contradictory to parts of Chapter 7 which suggest aircraft contrails have a non-negligible radiative forcing (see Exec Summary ch 7, pg 3, ln 37-40 and section 7.2.5.1) and also section 8.3.4.5 of this chapter. Should the contribution of aviation to stratospheric water vapour not be mentioned in the first paragraph of this section? [European Union]	Noted: We only refer to gaseous water here, not condensed water.
8-641	8	24	19	24	21	This paragraph does not appear to be comprehensive. Are other aviation emissions fully captured here (e.g.- stratospheric rockets)? [Government of United States of America]	Noted: But we have no information suggesting rockets are important
8-642	8	24	19		20	The text says contributions from the 'current civilian aircraft fleet' are very small. Are contributions from military aircraft much larger? Or small? Clarify. [Nathan Gillett, Canada]	Taken into account: Have removed "civilian"
8-643	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]	Taken into account, the SORCE data is the TIM data, then Figs. 8.10 and 8.11 (previously 8.11 and 8.12) now include the TIM/SORCE data.
8-644	8	24	23	27	51	Seems highly repetitive of chapter 7; can this be omitted or greatly shortened? [Stephen E Schwartz, United States of America]	Taken into account. The section is shorten somewhat. Chapter 7 has no time evolution of forcing. Chapter 8 should be possible to read as a stand-alone chapter
8-645	8	24	28	24	29	Fig 7.2 indicates that aerosol-cloud interaction includes the cloud lifetime effect as well as the cloud albedo effect. Formerly, aerosol-cloud interaction (both of these effects together) was called aerosol indirect effect, I thought. I am uncomfortable with the classification of the cloud-albedo effect (formerly the first indirect effect) as RF. This does not seem right to me because it is not instantaneous. The change to the droplet number and size distributions may be rapid when aerosol is added, but it is an adjustment. It is more obvious, I think, to	Taken into account, no RF aci will be provided from Chapter 7

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						distinguish between (instananeous) RF and everything which involves some adjustment. By this argument, all aerosol-cloud interaction is a forcing adjustment and counted in AF but not RF. This also fits better with the former classification, in that the direct effect effect is RF and all the indirect effects are AF. The more general idea of adjustment thus puts aerosol forcing into the same scheme as forcings due to other agents. I wonder if you and ch7 could reconsider your classification, or if not, give a clear rationale for it. I have made a similar comment on section 7.4.2. [Jonathan Gregory, United Kingdom]	
8-646	8	24	32	24	32	Are you specifying what sources these are? [Government of NORWAY]	Taken into account by adding a reference to section 8.2
8-647	8	24	43	24	43	See comment on page 24 line 28. Also, there is no need to repeat the definition. [Jonathan Gregory, United Kingdom]	Taken into account, by removing part of the sentence.
8-648	8	24				Section 8.3.4.1: A short discussion in the beginning to point out that the effects from aerosols are estimated using model (similar to ozone in section 8.3.3) as opposed to observed concentration (similar to WMGHGs in section 8.3.2) would be useful. [Government of United States of America]	Rejected: Already stated that main source is modelling and AeroCom. However, A sentence is added in beginning of 8.3
8-649	8	25	6	25	12	It would be informative here to give some examples about the improvments to models which result in the RF estimate being more robust in AR5 and with lower uncertainty. Have the results from Bond et al (2012) been included in the analysis? The discussion about Figure 8.23 could be moved earlier (Page 44, lines 45-49). [Katharine Law, France]	Taken into account, sources removed and replaced by models and observation-based methods. Bond et al. have been included. The spatial distribution shown in Fig 8.23 is somthing we will keep in section 8.6 with all discussion of spatial pattern.
8-650	8	25	6		7	Where does this estimate come from? Is this from chapter 7 or somewhere else in the report? If this is derived from the information presented in the rest of the section, then put this at the end of the section. Otherwise add a reference. [Nathan Gillett, Canada]	Taken into account. A reference to 7.5 is added.
8-651	8	25	7	25	32	The aerosol radiation interaction has a range of 0.3W/m2, less that in AR4. But Table 8.5 shows that the components have mostly the same error bars as in AR4, and introduce the SOA component with a large error bar. So why is the total RF error reduced? Lines 31-33 p 25 refer to a combination of methods, but this is not explained. [M Daniel Schwarzkopf, United States of America]	Taken into account, total RF ari uncertainty has been increased.
8-652	8	25	11			Better to cite a publication or a subsection of chapter 7 than a website. [Nathan Gillett, Canada]	Taken into account, a reference to the paper is added
8-653	8	25	15	25	16	Biomass burning aerosol has almost neutral impact on RF but why this aerosol is related to anthropogenic component? [Government of Poland]	Taken into account, we have added anthropogenic activity
8-654	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. Now Fig. 8.11 (perviously 8.13) shows clearly the curve of Krivova et al and Bal et al.
8-655	8	25	31	33	33	please give a brief description of the "combination of methods" used to estimate the best value and respective uncertainty for the total global and annual mean RF of aerosol (aerosol-radiation interaction) [Government of Brazil]	Taken into account by adding "(models and observation-based methods)"
8-656	8	25	36	25	37	This is supposed to be science, not science fiction. Observations don't go back to 1750 for anywhere except Europe and maybe not even there. Proxy measurements simply aren't good enough, and claims that models provide correct answers are fantasies unless you can prove the models to be 100% accurate, which you can't. [John McLean, Australia]	Rejected, the text describes that the estimates are based on model simulations.
8-657	8	25	36	25	40	Table 8.5 This needs consistency checking with Table 7.1 in Ch 7 (and radiative forcing figures in Ch 7). Some of the values are different and the uncertainty bounds must have been derived differently. Are the values in Table 7.1 evaluated over the same time period? Is there any way to bring consistency across these two chapters to keep the messaging consistent? [European Union]	Accepted, values are now consistent.
8-658	8	25	36			The value for black carbon in this table should have a footnote explaining that the value is incomplete, ie that it excludes anthropogenic biomass burning, which makes the table value a lower limit. [David Fahey, United	Rejected, the text includes this information

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						States of America]	
8-659	8	25	36			You should change the title of the Table 8.5, since the SAR, TAR and even AR4 did NOT give the RF for 2010. If you want, call it anthropogenic aerosol RF and note the dates that each AR's RF refers to under the AR subheading. [Michael Prather, United States of America]	Taken into account, by adding a line in the table caption stating the years for SAR TAR and AR4
8-660	8	25	36			You need to change the title of the Table 8.5, since the SAR, TAR and even AR4 did NOT give the RF for 2010. If you want, call it anthropogenic aerosol RF and note the dates that each AR's RF refers to under the AR subheading. [Michael Prather, United States of America]	Taken into account, see comment 6-659 which is identical to this comment
8-661	8	25	36			Numbers in table differ slightly from chapter 7; here I get for the summation for AR5 -0.45 W m-2 and uncertainty range +0.02 to - 0.92 W m-2, (i.e., -0.45 ± 0.47 W m-2)) where the uncertainty assumes independence in the uncertainties of the several terms (addition in quadrature); this might not be correct if, for example, the removal terms of the several substances are the same; such correlations would increase the uncertainty over that obtained assuming the quantities are independent. [Stephen E Schwartz, United States of America]	Taken into account, see 8-657
8-662	8	25				The authors should ensure the data in Table 8.5 is consistent with Table 7.1 [Government of United States of America]	Taken into account, see 8-657.
8-663	8	26	4	26	19	Some information about RF comparison between models and observation as well as model validation should be mention here. In the Chapter 9 page 45 there is a only information about comparison of climate model AOD with MODIS observation. [Government of Poland]	Rejected, model evaluation of aerosol distribution and properties are not part of Chapter 8.
8-664	8	26	8	26	9	"...records, and uncertainties in the emission of aerosols and their precursors used in the global aerosol modeling are larger previously than for current condition." Rephrase as "…records. When previous Assessment Reports were being prepared, the uncertainties in emissions of aerosols and their precursors used in global aerosol models were larger than is currently the case." [Eimear Dunne, Finland]	Rejected, indicate a different meaning than intended in the text.
8-665	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. I have gone through the recommended report. However, in this section we do not assess the surface impact of UV, which certainly is highly complex.
8-666	8	26	12	26	14	What are the implications of the large uncertainties in 1850 (1750?) biomass burning emissions, and the new estimates of these emissions from Lamarque et al., for estimates (and uncertainties) of aerosol RF? [Larry Horowitz, United States of America]	Taken into account, by addition the following 'but RF aerosol-radiation-interaction is close to zero for this component'
8-667	8	26	12		13	Are the Lamarque et al. biomass burning estimates higher or lower than previous estimates? Briefly say why and what the implications are. [Nathan Gillett, Canada]	Taken into account, see comment 8-666.
8-668	8	26	14			This sentence compares 1750 emissions of some aerosol components with 1850 emissions of others. [Nathan Gillett, Canada]	Taken into account by deletion of sentence with 1750 emissions.
8-669	8	26	17	26	19	It is recommended to delete “After 1990 the change has been small with even a weakening of the aerosol-radiation interaction RF, mainly due to a stronger BC RF as a result of increased emissions in East Asia.” Reasons: the original texts emphasize the impacts of BC emissions in East Asia on aerosol-radiation budget. However, there are only limited observations in East Asia. No references are provided here. Actually, many other regions also experienced the increase of BC emissions. Only pointing out East Asia would give an unbalanced formulation. [Government of China]	Taken into account by adding 'and South East'. The increase in emissions in East and South East Asia are supported by several publications.
8-670	8	26	23	26	23	higher temporal-resolution ? [Räisänen Petri, Finland]	Accepted.
8-671	8	26	31	26	37	This is an important paragraph but I couldn't completely follow it [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Taken into account, given the changes to chapter 7, this paragraph has been changed significantly t refelct

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							chapter 7 content.
8-672	8	26	31	26	37	This paragraph is confusing. It is first stated that the RFaci is estimated as -1 Wm-2, and that this is more negative than the -0.7 Wm-2 of AR4, but then this is rescaled to -0.3 Wm-2. Consider reformulating the first sentence so that it makes clear that -1 Wm-2 is a model-based estimate. E.g.: "The average model-based value for the RF due to aerosol cloud interactions (RFaci), formerly known as the first indirect forcing, is -1 Wm-2." [Räisänen Petri, Finland]	Taken into account, No estiamte of RF aci is given in the TOD for reasons now stated and also stated in Chapter 7
8-673	8	26	32			Cite these published estimates (or if this is from chapter 7, cite the subsection). [Nathan Gillett, Canada]	Taken into account, Cited from chapter 7 ,see comment 8-671
8-674	8	26	34	26	37	The smaller RF of aci from satellite observaions can be problematic, as pointed out by Penner et al., (2011, PNAS). Unfortunately, this smaller forcing from satellite approach is the one used in Table 8.7. This may underestimate the magnitude of the current RF of aci. The authors should conside rreviisng the text to reflect this finding. [Government of United States of America]	Taken into account, the reason for a smaller overall effective forcing (ERFaci+ari) than previous estimates is now stated and comes from a number of sources not just satellite observations. The estiamte of the forcing is now a best judgement value weighing these sources of information (process model studies, satellite studeis, added effects of longwave, mixed pahse clouds, convection).
8-675	8	26	34		37	This is a big difference (-1 versus -0.3 W/m^2). Either include more explanation here, or a summary of what is included in chapter 7 if this is discussed there. [Nathan Gillett, Canada]	taken into account, Explanation now give (see also response to #674
8-676	8	26	42	26	42	punctuation needs removing from inside the bracket [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Taken into account, Section rewritten see 8-674 and 8-671
8-677	8	26	44	26	44	"Afari" is used before it is defined. [Robert Portmann, United States of America]	Taken into account, now eliminated and ERF is used as consitent with chap 7
8-678	8	26	53			The authors should consider revising the text here. Is "Significantly" a little overstatement? Section 7.4 discusses both progress and challenges related to the aerosol indirect effect on warm clouds, and the fact that the related AF estimates have not changed much since AR4. [Government of United States of America]	Taken into account, these judgemetn statems have been removed and this secion is meremly a sumamry of judgment estimates discussed and justified in chapter 7
8-679	8	27	4	27	6	Wording is not that clear [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Taken into account, Re-written see 8-671
8-680	8	27	4	27	6	This is not clear. Where does the mean value -1.5 Wm-2 come from? And how come that aerosol-radiation interaction (ari) only plays a secondary role (when on p. 8-50, line 50, a best estimate of -0.5 W m-2 is provided for AFari)? [Räisänen Petri, Finland]	Taken into account, reference to AFari now changed and judgement estimates of ERFari+aci are now offered (per chapter 7).
8-681	8	27	4		6	The sentence beginning 'Its mean value' is unclear. What is the referring to? The first GCM estimate of AF ari+aci or best estimates of this? Hasn't it already been shown that AF ari is substantial? Is this implying that AF ari + AF aci is not the same as AF ari+aci. This needs to be explained more clearly. Second, doesn't this imply that AF ari is positive? [Nathan Gillett, Canada]	taken into account, discussion now changed completely
8-682	8	27	11	27	11	replace "of -0.9" with "is -0.9" [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Noted
8-683	8	27	11	27	11	The uncertainty range for AFari+aci, in my view, is seriously underestimated for the reasons laid out in the comments on Chapter 7. As a result, the uncertainty range of AFaci is only slightly larger than that of CO2 (Fig. 8.17 in Chapter 8). How can one reconcile this with AFaci's “very low” level of scientific understanding (Fig. 8.16 in Chapter 8)? [Yi Ming, United States of America]	Taken into account, uncertainty range that is quoted merely reflects that derived from the new analysis described in Chapter 7
8-684	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, sentence on efficacy deleted.
8-685	8	27	16	27	36	Have the results from Bond et al (2012) been included in the analysis? [Katharine Law, France]	Taken into account, the Bond et al. Study is taken into

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							considerations for the estimate in Ch7
8-686	8	27	29	27	32	Huang et al. (2011) provides an important information on BC depositing in snow of Northern China, while glaciers in Western China are extremely important in water security and greatly concerned since AR4. Their melting were also linked with black carbon. There is a recent publication which can be a supplement to Huang et al. (2011). Since 2004, many investigations on the impacts of black carbon (BC) deposition in the surface of High Asia mountian glaciers (HAMGs) have been intensively conducted in the Tibetan Plateau and Tianshan Mountains. Amounts of surface snow and ice core samples were collected, in which black carbon was measured. Averagely, black carbon concentrations in the surface of the HAMGs ranged below 10 to 150 ppbm with the mean of 50 ppbm. And the radiative forcing caused by BC was calculated as 5 W m-2, and the mean absorption of glaciers in themselves is over 100 W m-2, as measured by radiometers set up on the glaciers. Thus a rough estimate of the impact of BC on the radiation balance was less than 5%. Here is the reference: J. Ming, C. Xiao, Z. Du and X. Yang, An Overview of Black Carbon Deposition in High Asia Glaciers and its Impacts on Radiation Balance, Advances in Water Resources(2012), in press. http://dx.doi.org/10.1016/j.advwatres.2012.05.015 [Jing Ming, China]	Rejected, Section is partly retwritten with focus on trends in BC on snow measurements.
8-687	8	27	30		32	The meaning here is unclear. Does this mean that probably not much BC in the Arctic comes from China? Or that there is a significant contribution of Chinese BC in the Arctic? [Nathan Gillett, Canada]	Taken into account, this sentence has been clarified.
8-688	8	27	44			You need to put in a section (8.3.43.6) on aerosol - chemistry interactions that impact GHG. There are a number of papers showing the uv-radiation effects on O3 and OH beginning with Randal Martin and Huisheng Bian in 2003 - see comment on p8.l19. [Michael Prather, United States of America]	Rejected, covered in section 8.2
8-689	8	27	44			You need to put in a section (8.3.43.6) on aerosol - chemistry interactions that impact GHG. There are a number of papers showing the uv-radiation effects on O3 and OH beginning with Randal Martin and Huisheng Bian in 2003 - see comment on p8.l19. [Michael Prather, United States of America]	See comment 8-688
8-690	8	27	55	30	30	Given the large description of solar radiation management (SRM) in chapter 7 it seems strange not to include discussion here specifically of change in albedo over biofuel regions. [European Union]	Accepted A sentence was added to the introduction "Potential geo-engineering techniques that aim at increasing the surface albedo are discussed in section 7.7.2.3."
8-691	8	27	55	30	30	Section 8.3.5 Have deliberate afforestation activities had any noticeable effect over different regions? There is some discussion of forcing differences for deforestation/afforestation in high-latitude regions where ground is snow covered for some portion of the year. Is there any evidence of differential effects from deforestation/afforestation in the tropics verses the mid-latitudes? [European Union]	Rejected. This is discussed in detail in the (FAO, 2012) document that is referenced in this section. There is no sufficient space to describe the afforestation efforts for each region of the world.
8-692	8	27	55	30	30	Section 8.3.5 Is this a good place to discuss briefly how land use change is dealt with in the CMIP5 models? Is it included as a dynamic parameter? Is it prescribed offline? Is it stationary through time? [European Union]	Rejected. We do not thik it is appropriate to discuss CMIP5 models here
8-693	8	27				Section 8.3.5.1: It would be valaulble if the authors would provide the AR5 RF value for Land Surface changes here to directly compare with AR4 value. [Government of United States of America]	Rejected. The chapter standard is to provide AR4 value, then discuss recent findings and they provide a new value.
8-694	8	28	16	28	21	Also an excellent example of how to bring in social aspects upfront in the discussion. [Government of NORWAY]	Noted
8-695	8	28	19	28	19	China should be mentioned here. Since the late 1970s China is running a significant reforestation program. Annual C storage due to reforestation is estimated to be 0.19 to 0.26 Pg C yr(-1). Cai CZ 2012 Greenhouse gas budget for terrestrial ecosystems in China. SCIENCE CHINA-EARTH SCIENCES 55, 173-182 (see also other publications) [European Union]	Taken into account. We agree that China must be mentionned together with Europe and North America. This is corrected. We did not include the reference as it is well described in the FAO document that is mentionned.
8-696	8	28	19	28	28	Suggest reviewing whether forest area in North America and western Europe has increased not just because of abandonment of agricultural land but also because of active afforestation (or "reforestation") programs, notably in the UK from 1919 until ca 2000. (Not sure whether it continues today). A recent reference was not found for this, but understanding is that forest area in the UK increased several-fold after 1919 due to active afforestation efforts. [Government of Canada]	Taken into account. We now explicitly state that afforestation efforts have had significant impacts in North America, Europe and China.

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697	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. Resolution improved in final version
698	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]	Taken into account
8-699	8	28	23	28	32	See also Nair et al. (2007): Nair, U. S., D. K. Ray, J. Wang, S. A. Christopher, T. J. Lyons, R. M. Welch, and R. A. Pielke Sr., Observational estimates of radiative forcing due to land use change in southwest Australia, J. Geophys. Res., 112, D09117, 2007. [Loretta Mickley, United States of America]	Rejected. This reference is cited later on in the section but does not appear appropriate here
8-700	8	28	36	28	36	Instead of "radiation" use "flux" [Government of Poland]	Noted
8-701	8	28	37	28	37	The following relevant reference could be added following 'grasses and croplands': Betts, A. K., R. L. Desjardins and D. Worth (2007). "Impact of agriculture, forest and cloud feedback on the surface energy budget in BOREAS." Agricultural and Forest Meteorology 142: 156-169. [Government of Canada]	Rejected. Although it is indeed a relevant reference, we do not think it is necessary to provide an specific reference for a general statement which is backed up by other references cited in the section.
8-702	8	28	40	28	41	The statement "is more easily covered by snow that reflects sunlight much more than vegetation does" is vague. Suggested revision could read as: "accumulates continuous snow cover more readily in early winter allowing it to persist longer in spring. This causes average winter albedo in deforested areas to be generally much higher than that of a tree-covered landscape." The following relevant reference could be added at the end of this sentence: Bernier, P. Y., R. L. Desjardins, Y. Karimi-Zindashty, D. E. Worth, A. Beaudoin, Y. Luo and S. Wang (2011). "Boreal lichen woodlands: A possible negative feedback to climate change in eastern North America." Agricultural and Forest Meteorology 151: 521-528. [Government of Canada]	Accepted We changed the sentence and added the reference.
8-703	8	28	43			Say that this is for the year 1750 if that's the case. [Nathan Gillett, Canada]	Noted. 1750 is the reference year for this assesment as stated in the introduction section
8-704	8	28	45	28	49	"the present day flux change due to albedo change from vegetation is on the order of –0.2 W m–2 (range –0.21 to –0.24). The RF, defined with respect to 1750, is in the range –0.17 to –0.18 W m–2." This is confusing. What is meant by the "present day flux change"? It is defined earlier here with respect to the "beginning of the industrial era", which would be 1750, but could be later (say up to 1850). Why then does the next sentence say that RF since 1750 is actually smaller (less negative) than the -0.21 to -0.24 W m-2 stated previously? It could be referring to Figure 8.11 which explains why the last sentence seems contradictory, but then that figure also shows the -0.2 Wm-2 change occurring since 1400, so long before the beginning of the industrial era. Clarity is required here to ensure these statements are understandable here and in relation to Figure 8.11. [Government of Canada]	Taken into account. Sentence was changed to : They estimate that the solar flux change induced by the albedo variation, from potential vegetation to 1992, is on the order of –0.2 W m–2 (range –0.21 to –0.24). The RF, defined with respect to 1750, is in the range –0.17 to –0.18 W m–2.
8-705	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]	Rejected. Comment misplaced. Does not apply to Land Use section
8-706	8	29	21	29	31	What "undesired impacts on global circulation"? Be more specific [European Union]	Taken into account Added ", shifting precipitation patterns "
8-707	8	29	25		45	What about the work on fires as land-cover change and albedo change? Most of these are considered partly human caused - is there a place in this section for this? The influence of burn severity on post fire vegetation recovery and albedo change during early succession in North American boreal forests, J. Geophys. Res., 117, G01036. Jin, Y., J. T. Randerson, M. L. Goulden, and S. J. Goetz (2012), Post-fire changes in net shortwave radiation along a latitudinal gradient in boreal North America, Geophys. Res. Lett., 39, L13403. [Michael Prather, United States of America]	Taken into account. This is discussed in the first paragraph od 8.3.5.4. We have added the Jin et al reference
8-708	8	29	29	29	29	Consider referencing that increasing the winter albedo through tree removal decreases the RF but only during the period of the annual cycle when TOA SW forcing is relatively small. Also, surface blackening of natural vegetation due to fire is relatively short-lived and typically disappears within a year or so. Hence, even a dramatic decrease in albedo (e.g. following a fire) will not have a sustained significant impact. [Government of Canada]	Accepted

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8-709	8	29	30			Is this an affect due to changes in African fires? If so over what period? Or fires versus no fires at all? [Nathan Gillett, Canada]	Rejected. There is no space to discuss in detail the content of the publication which is easily accessible
8-710	8	29	32	29	35	Dust sources associated with land-use have been detected from satellite data over Sahel in West Africa (Ginoux et al., 2010), and with larger contribution, relative to natural sources, over the other continents (Ginoux et al., 2012a). In addition, the mixing of dust and ammonia from cropland has been observed in most arid areas, and shown to affect dust optical properties (Ginoux et al., 2012b). References: Ginoux, P., D. Garbuzov, and N. C. Hsu, 2010: Identification of anthropogenic and natural dust sources using Moderate Resolution Imaging Spectroradiometer (MODIS) Deep Blue level 2 data. Journal of Geophysical Research, 115, D05204, doi:10.1029/2009JD012398. Ginoux, P., J M Prospero, T E Gill, C Hsu, and Ming Zhao, 2012a: Global scale attribution of anthropogenic and natural dust sources and their emission rates based on MODIS Deep Blue aerosol products. Reviews of Geophysics, 50, RG3005, doi:10.1029/2012RG000388. Ginoux, P., L. Clarisse, C. Clerbaux, P.-F. Coheur, O. Dubovik, N. C. Hsu, and M. Van Damme, 2012b: Mixing of dust and NH3 observed globally over anthropogenic dust sources. Atmospheric Chemistry and Physics, 12(16), doi:10.5194/acp-12-7351-2012. [Paul Ginoux, United States of America]	Taken into account. This statement is not contradictory to what was writen. We nevertheless added a few words and a reference: "This, together with the analysis of dust sources (Ginoux et al., 2010), suggests that a significant fraction of the dust that is transported over the Atlantic has an anthropogenic origin and impacts the Earth albedo".
8-711	8	29	41		42	It might be worth saying here that the global impact of these albedo changes is small. [Nathan Gillett, Canada]	Accepted
8-712	8	29	45	29	45	add the words "which is" after the comma [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Editorial
8-713	8	29	56	29	57	With out citations this sentence seems to speculativie [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence removed
8-714	8	30	26	30	26	add (Milankovitch cycles) after the Sun and Earth, optionally some reference. [Government of Poland]	Taken into account, Comment misplaced. However, included in beginning of section 8.4.
8-715	8	30	28	30	29	It seems like th -0.15 forcing was just plucked from the air because your expert judgement said that a forcing of -0.2 Wm-2 was too strong. A think this is the wong way to manaage erros - how can you quanitfiy your expert judgement in this way? It may be better to stick with -0.2 and qualtatively discuss bias. One shuld not really mix quantitiaitve and qualative errors [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Rejected, we disagree. There is new evidence (satellite MODIS observations) indicating lower albedo differences between crops and forest than used earlier. There is therefore some (limited) evidence arguing for a lower value of the RF estimate.
8-716	8	30	37	30	39	It would be simpler ro state that no major asteroid impacts occured during the reference period (1750-2011) and thus that this effect does not have to be considered. [Christian Muller, Belgium]	Taken into account: Text is changed as suggested.
8-717	8	30	44	30	45	"due to wavelength-albedo dependence, solar activity-wavelength dependence and absorption within the stratospehere". This probably needs to be clarified. Absorption of what? Dependence of what on what? Solar activity does not depend on the wavelength, its manifestations do. Should it, perhaps, be dependence of solar irradiance variability or of solar radiation on the wavelength? [Natalie Krivova, Germany]	Taken into account: Text is changed to 'solar radiation-wavelenght dependence.'
8-718	8	30	44			In the Chapter 8.4. you say that solar forcing takes place in several timescales. The timescale for variability affecting our climate in centennial scale is in your text restricted to 11 year cycles. Longer cycles are according to your text connected with astronomical scales of millions of years. This is not the whole truth. There are several partly unknown cycles in solar energy that correlate with past climate changes in centennial and millenial scales with a lag of several decades. So the natural warming caused by solar forcing is still going on, although solar activity has been declining for the last 20 to 30 years. For this reference see Helama, S., Mielikäinen, K., Timonen, M. & Eronen, M. 2010. Sub-Milankovitch solar forcing of past climates: Mid and late Holocene perspectives. Geological Society of America Bulletin 122: 1981-1988. [Kari Mielikäinen, Finland]	Taken into account: We discuss mainly the changes in the 11-years solar cycle. However, centennial changes are implicitly considered when we discuss the radiative forcing since the Maunder minimum and since the year 1750. We will change the text in 8.4 as: " Solar variability takes place at many timescales, that include centennial and millenial scales (Mielikäinen et al., 2010), as the radiant energy output of the Sun changes. Also, changes in the astronomical alignment of....."
8-719	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]	Rejected, not 'shortwave' twice on this line on page 30.
8-720	8	30	46	30	48	See start of above comment, propose to change line content with: " …., the RF is reduced to about 80% of the TOA instantaneous RF. There is low evidence ow the exact value of this number." [Bo Andersen, Norway]	Accepted: We startt now with "There is low confidence of the exact value of this number…".
8-721	8	30	54	31	1	Is the subsection "8.4.1.1.1 Satellite measurements" needed? It is the only subsection of the section "8.4.1.1	Accepted: We will have no subsection 8.4.1.1.1.

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						Observed Variations of TSI". Since only satellite observations are discussed, there seem to be no reason for having the subsection 8.4.1.1.1. [Natalie Krivova, Germany]	
8-722	8	30	54			The authors could consider changing the heading title to "Observed Variations of Total Solar Irradiance (TSI)" or define TSI in the first paragraph of section. The authors could also add Solar Irradiance to Annex [Government of United States of America]	Accepted: We willl spell out TSI in the first paragraph. Since the first order draft we were asked not to write the definition of TSI.
8-723	8	31	1	31	50	As science support to the SOLAR package on COLUMBUS (International Science Station), I globally approve this analysis of satellite observations. [Christian Muller, Belgium]	Noted
8-724	8	31	3	31	47	On line 3 it gives the SORCE TSI as "1360.8 ± 0.5 W m–2 during 2008" yet on line 46 it has "mean TSI for September 2008 was 1365.26 ± 0.16 W m–2," with similar values on line 47. This may very well be because the authors are now quoting PMOD (for variations between SC) rather than SORCE (for absolute TSI), but this is quite confusing as written, especially as the figure that goes here shows values around the 1360 level; not 1365. Please consider revising the text to reconcile these differences - or more clearly explain the differences. The authors could either be much more clear about the data used, or adjust the PMOD values to the SORCE scale, etc. [Government of United States of America]	Taken into account: In fact we use the TIM/SORCE measuremenst (which started in 2003). Below we use PMOD. We use PMOD because the mesurements started in 1978, covering several solar minima and therefore allowing us to obtain a RF along several solar minima. In Fig. 8.10 we standardized the PMOD to TIM/SORCE average (2003-2012).
8-725	8	31	4	31	4	4.46 Wm-2. Providing this number with such a precision makes little sense. The value changes with every update of the PMOD composite on the web site. Since in 2006 Froehlich could not give the value for 2008, it is not clear where this number comes from. Using the value listed on the PMOD page on 28th November 2012, I get the difference of 4.48 Wm-2. I propose to round to 4.5 [Natalie Krivova, Germany]	Accepted. The correct reference is Froehlich, 2009.
8-726	8	31	6	31	9	Replace with simpler sentence: "This calibration gives better linked to national standards and should indicate that the absolute values provided by TIM are the most accurate." [Bo Andersen, Norway]	Accepted
8-727	8	31	9	31	10	The GFDL (CM3) simulations used the TIM values, so the sentence should be “most general circulation models” [M Daniel Schwarzkopf, United States of America]	Accepted
8-728	8	31	11	31	11	replace "is of" with "has" [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted
8-729	8	31	14	31	14	"revision of the TSI" should be "revision of the absolute value of the TSI" [Natalie Krivova, Germany]	Taken into account. The text has space limitations, then the style is at its minimum.
8-730	8	31	21	31	25	The RMIB composite now only starts at the end of 1984. Thus it does not use the HF data before 1981 and cannot show any difference to ACRIM and PMOD before 1984. [Natalie Krivova, Germany]	Taken into account: The text has been corrected as follows.:There are two major differences of ACRIM relative to PMOD. The first is the rapid drift in calibration between PMOD and ACRIM before 1981. This arises because both composites employ the Hickey-Frieden (HF) radiometer data for this interval. Re-evaluation of the early HF degradation has been implemented by PMOD but not by ACRIM. The second one, involving also RMIB, is the bridging of the gap between the end of ACRIM I (mid-1989) and the beginning of ACRIM II (late 1991) observations, as it is possible that a change in HF occurred during this gap. This possibility is neglected in ACRIM and thus its TSI increases by more than 0.5 Wm-2 during SC 22.
8-731	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]	Taken into account by separating the figure into 3 figure under the assumptioin that this is Figure 8.17 (Figure 8.16 has not three panels).
8-732	8	31	31	31	39	I find the discussion of the GCR trends too vague to be considered as solid evidence against the ACRIM	Accepted: the paragraph is changed using this

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						composite. I would propose to shorten this. The last sentence is a stronger argument. For the instrumental problems, the reference is R.B. Lee III et al. (1995, JGR 100). For the models, the best one is Ball et al. (2012, A&A 541) since this model is fully independent of any of the three existing composites. Wenzler et al. (2009, GRL 36,10.1029/2009GL037519) and Krivova et al. (2009, GRL 36,10.1029/2009GL040707) would be the other ones (more appropriate than Wenzler et al. 2006) [Natalie Krivova, Germany]	suggestions and the recommended references is included.
8-733	8	31	39	31	39	In Figure 8.12 the connection between TIM and PMOD is done at one point, this puts the unceratinty in the timesies of each point directly into the shift. To reduce this the fit must be done over time peiode where the gradient is less, i.e. minimum. The curve should be corrected also. Please insert: "For incooperation of TIM data eith the previous and overlapping data the adjustment should made over a period around the last solar minimum to ensure that the internal pointwise uncertainty is minimized in the doward movement of the accepted PMOD data" [Bo Andersen, Norway]	Accepted: We standardize PMOD to TIM average (2003-2012). See also SM S8.6
8-734	8	31	41	31	42	In fact, the SC variation in TSI of about 0.1% for the last 3 cycles is present in all three composites. [Natalie Krivova, Germany]	Taken into account: text changed to indicate this.
8-735	8	31	43	31	43	"compensation" is not a proper word here. Compensation would mean no variability. Maybe "an interplay" or "ensemble acting"? [Natalie Krivova, Germany]	Taken into account: We use interplay.
8-736	8	31	44	31	44	Foukal et al. (2006) is not an appropriate reference here. They did not show this. They mention this indeed, but this was proposed and shown well before this paper. [Natalie Krivova, Germany]	Taken into account: Foukal is removed and new reference is inckluded.
8-737	8	31	46	31	47	The numbers for the minima TSI values in PMOD are misleading at this place. The reader will compare them to the value of 1360.8 listed at the beginning of 8.4.1.1.1. Moreover, these numbers change slightly with updates and revisions of the PMOD composite. E.g. numbers listed on the PMOD web site are different from those in Froehlich (2009). Giving just the difference between 1986 and 2008 (with error-bars) would be less confusing and independent of the PMOD version. The difference is measured more accurately than the absolute values. [Natalie Krivova, Germany]	Accepted: we will give only differences with error bars.
8-738	8	31	48	31	52	We note an inconsistency with the SPM conclusion (SPM-8, lines 34-36). It is mentioned that for 'the last three solar minima PMOD values, between 1986 and 2008 there is a negative RF of – 0.04 ± 0.02 W m–2. Between 1986 and 2011, an interval that includes a substantial portion of the SC variation, a positive RF of 0.01 ± 0.005 W m–2 is calculated'. In the SPM (page 8, lines 34-36) the year 1976 is mentioned, instead of 1986. [Government of Netherlands]	Taken into account: New estimate for 1986-2008 is provided. This is given in our ES and will be provided for the SPM (updated in the last version in beginning of May for the authors).
8-739	8	32	1	32	39	Let me congratulate the authors of this report so intense and interesting. I work in Solar-Terrestrial Physics and I would like to do a contribution to this report in the area of the reconstruction of solar irradiance. Please, note that almost all estimations of TSI variations (as Vieira et al., 2011) since pre-industrial time are based in the Group Sunspot Number index, published in a seminal work by Hoyt and Schatten (1998). This version of Sunspot Number is preferred by researchers respect to International Sunspot Number (Clette et al., 2007) because Group Sunspot Number starts at 1610 and it is the longest time-series based in direct solar observations. However, note that these two sunspot number versions are quite different in the historical period. The results are different trends since the Maunder Minimum (Hathaway et al., 2002). This is important in order to estimate RF since the Maunder Minimum. I think the report should clearly establish this fact. Moreover, note that Group Sunspot Number can be updated recovering early sunspot observations (e.g., Vaquero et al., 2011). [José Manuel Vaquero, Spain]	Taken into account: We this rematks in the Supplementary Material section S8.6, due to space limitations of the main text.
8-740	8	32	1	32	39	Finally, I would note that Svalgaard (2012) have published some preliminary corrections to Group Sunspot Number that could change our actual perspective. These changes could imply a reduction in the solar RF since the Maunder Minimum. Therefore, I also think the report should clearly establish this fact. References Clette et al. (2007) From the Wolf Number to the International Sunspot Index: 25 years of SIDC, Adv. Space	Taken into account: We will quote this new paper by Svalgaard, pointing out the implication of a revision of the Group Sunspot Number in section S8.6.

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						Res., 40, 919–928- Hathaway et al. (2002) Group Sunspot Numbers: Sunspot cycle characteristics, Sol. Phys., 211 (1), 357–370. Hoyt and Schatten (1998) Sunspot numbers: A new solar activity reconstruction, Sol. Phys., 179, 189–219. Svalgaard (2012) How well do we know the sunspot number? Proceedings IAU Symposium No. 286, 27-33. Vaquero et al (2011) Revisited sunspot data: a new scenario for the onset of the Maunder minimum, ApJ 731, L24. Vieira et al. (2011) Evolution of the solar irradiance during the Holocene, Astronomy & Astrophysics, 531. [José Manuel Vaquero, Spain]	
8-741	8	32	10	32	11	The paper by Delaygue & Bard (2011) does not provide any independent estimate of the TSI secular change. This reconstruction is scaled using their reconstruction of the modulation potential and the assumed value of the TSI secuclar increase of 0.08% with the reference to IPCC AR4 (see 1st paragraph of Sect. 5.7 of this paper) [Natalie Krivova, Germany]	Taken into account: We deleted the phrase "independent estimate of TSI".
8-742	8	32	10	32	14	A range of –0.02 to 0.10 W m-2 is mentioned for the solar irradiance, while in the SPM a slightly different range –0.01 to 0.09 W m-2 is mentioned (SPM-8, line 34). [Government of Netherlands]	Taken into account: We will check consistency. However, notice that in the first (large) paragraph of 8.4.1.2 we are concerned with the RF between 1750 and present. The RF between the Maunder minimum and the present is addressed briefly in the last paragraph of this section. See also 8-738
8-743	8	32	10	32	15	Refer to comments 1 and 3. With a small increase in the .78 coefisient to 0,8 (or even 0,82) these numbers will change in addition the large uncertainty descriped in comment 1 and in the litterature as well as in Figure 8,13 we shoul be carefull to be so definite about the numbers. The best would be to use the description made in comment 1. [Bo Andersen, Norway]	Taken into account: We rewote the corresponding text.
8-744	8	32	18	32	18	"an" should be "and" [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted
8-745	8	32	18	32	18	Replace "an" with "and". [Robert Waterland, United States of America]	Accepted
8-746	8	32	28	32	32	It is not at clear in the paragraph what the AR5 forcing number is that you are comparing the AR4 estimate to [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Taken into account. We will change the text to make it clearer
8-747	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]	Accepted
8-748	8	32	43	32	54	What is the reasoning behind most estimates of future total solar irradiance (TSI) being lower than the most recent minima (2008)? Can anything be said here about studies that have tried to approach the uncertainty here by looking at bounding the problem using previous maxima and minima to study potential future changes (e.g., G. S. Jones, M. Lockwood, and P. A. Stott, J. Geophys. Res., VOL. 117, D05103, doi:10.1029/2011JD017013, 2012) [European Union]	Taken into account: In this paragraph we already quoted Jones, M. Lockwood, and P. A. Stott. It is inside the parenthesis as Jones et al., 2012. Considering their results we obtained the RF between the modern minimum in 2008 and this future 21st century minimum: a negative RF of with a range of -0.16 to 0.12 W m–2. The negative values impliy that the TSI in 2008 is larger than the TSI in the future 21 st century minimum. In fact this paper consider previous minima to estimate potential future TSI changes.
8-749	8	32	47	32	49	Please be consistent with how your report negative radiative forcing. On page 31, line 48-49 for example, you include the (-ve sign), e.g., you state "negative RF of –0.04 ± 0.02 W m–2 ". Now here, you don't provide the (-ve sign). [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: All estimates are provided as -0.04 (-0.08 to 0.0).
8-750	8	32	48	32	48	"this 21st century minimum". It is not clear what is meant here. Apparently, this refers to the fact that the Sun has just left the 20th century Grand Maximum. This does not mean, however, that the Sun enters (or will soon enter) the next Grand minimum. Statistically, the probability for the Sun to enter directly into the next Grand minimum in the next 30 years is below 10% (Solanki & Krivova 2011, Science 334). It is not even clear whether the next extremum will be a maximum or a minimum. Thus one can only talk about the Sun leaving the high activity level and returning to normal activity (it is still correct that a negative RF for the next decades	Taken into account: The model of Jones et al , estimate a future TSI minimum similar to the Dalton minimum, it is in this sense that we mention the 21st century minimum. Following your comment, we will distinguish in the text those predictions of the Sun returning to normal conditions, from that one

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						is indeed highly probable, however not because the Sun enters a grand minimum, but because it leaves or has left a grand maximum). [Natalie Krivova, Germany]	estimating an actual minimum.
8-751	8	32	50	32	50	Replace "we have a very low" with "we have no" [Bo Andersen, Norway]	Taken into account. Besides of the paper by Jone et al., there is the paper by Rigozo et al. Then considering that we have two papers, there is very low evidence. Moreover, as the papers do not agree with the the intensity of such minimum, then tere is very low agreement. Togethet this gives very low confidence.
8-752	8	32	51	32	51	Add sentence:" From the extended solar minimum and the seemingly low activity of the current solar maximum there may be changes of an unkown character of the solar variability cycle (see argument above and previously sited references). In addition there are indications that the mean magnetic field i solar spots may be diminishinig on a decadial level. A linear expansion of the current trend may indicate that of the order of half the sunspot activity may disappear by about 2025. (M.J. Penn & W. Livingston, Astrophysical Journal, 649: L45–L48, 2006 September 20 and M.J. Penn & W. Livingston,Proceedings of the International Astronomical Union, IAU Symposium, Volume 273, p. 126-133). [Bo Andersen, Norway]	Taken into account: We add part of this sentence as recommended, it will strenghten the argument concerning a future minimum.
8-753	8	32	53	32	54	It seems that this statement would be true whether or not there is a diminished solar activity in the next few decades. Or at least, as long the solar activity doesn't increase substantially. Or is this sentence trying to compare the increment in GHG forcing from now to (date unknown) against the increment in solar forcing? The authors should consider elaborating upon this. [Government of United States of America]	Taken into account: Yes, the statement is true whether or not there is a diminished solar activity in the next few decades.
8-754	8	32	58	32	58	Uncapitalise "HAIGH" [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Taken into account. It is a problem of the Endnote web that we use to add references in te thext. It automatically gives some references capitalized and some not. This will be fixed during the proof of the chapter.
8-755	8	32	58	32	58	Replace "HAIGH" with "Haigh" [Räisänen Petri, Finland]	Noted, See above
8-756	8	33	4	33	7	The authors should consider clarifying what is meant by "background ozone" and how it relates to "ozone forcing". [Government of United States of America]	Accepted
8-757	8	33	10	33	19	This is an important point that should be referred to at various points in chapter 5 as noted above. [Robert Kandel, France]	Noted
8-758	8	33	15	33	18	"In the stratosphere there are important UV variations, as this region has the potential to affect the troposphere and therefore climate (Gray et al., 2010), the UV may actually have a more significant impact on climate than what the sole TSI suggests." This sentence should have punctuation adjusted to convey the meaning more accurately: "In the stratosphere, there are important variations in UV over a solar cycle; as this region..., the UV may have a more significant impact on climate than changes in TSI alone would suggest." [Eimear Dunne, Finland]	Taken into account: Text is changed as suggested.
8-759	8	33	17	33	17	Pls change "(McClean and Carman, 2011)" to "McClean and Carman (2011)" [HASIBUR RAHAMAN, India]	Rejected, this reference is not in section 8.4.1 nor in Chapter 8 SOD or final order draft.
8-760	8	33	17			Gray et al (2010) is an excellent review paper but not a specific reference for UV influences; I suggest replace by Haigh J D (1996) The impact of solar variability on climate. Science, 272, 981-984. [Joanna Haigh, United Kingdom]	Accepted, reference is included
8-761	8	33	18	33	18	The UV variations are indeed very important in amplitude but also provide an important chemical trigger. The report should mention the Haigh et al nature paper: Joanna D. Haigh,Ann R. Winning,Ralf Toumi & Jerald W. Harder, An influence of solar spectral variations on radiative forcing of climate, Nature 467, 696–699, 2010. This article shows a strong influence of solar UV variations on ozone in the stratosphere leading thus to complex feedbacks with the thermal balance and tropospheric coupling. this is possibly the trigger by which solar activity has had a measurable effect on past climate variations. [Christian Muller, Belgium]	Taken into account: This point is important and we discuss it in the SM S8.6 where we point out that: "The Spectral Irradiance Monitor (SIM) on board of the Solar Radiation and Climate Experiment (SORCE) measurements (Harder et al., 2009) suggest that over SC 23 declining phase, the 200–400 nm UV flux

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							decreased by ~10 times more than expected from prior observations and model calculations and in phase with the TSI trend, while surprisingly the visible presents an opposite trend. However, SIM's solar spectral irradiance measurements from April 2004 to December 2008 and inferences of their climatic implications are incompatible with the historical solar UV irradiance database, coincident solar proxy data, current understanding of the sources of solar irradiance changes, and empirical climate change attribution results, but are consistent with known effects of instrument sensitivity drifts; thus what seems to be needed is improved characterization of the SIM/SORCE observations and extreme caution in studies of climate and atmospheric change (Haigh et al., 2010) until additional validation and uncertainty estimates are available (DeLand and Cebula, 2012; Lean and DeLand, 2012)."
8-762	8	33	22	33	23	Part of the first sentence in this paragraph seems to be missing: "AR4, based on multiple space measurements made in the past 30 years (Brueckner et al., 1993; Rottman et al., 1993)." [Eimear Dunne, Finland]	Accepted.The paragraph has changed as follows: Multiple space-based measurements made in the past 30 years showed that UV variations accounted for ~30% of the SC TSI variations, while ~70% were produced by the visible and infrared (Rottman, 2006).
8-763	8	33	30	33	30	"spectral properties of sunspots" - better "spectral contrasts of different surface magnetic features" (in the UV, the contribution of sunspots is significantly weaker than that of faculae, plage and network). [Natalie Krivova, Germany]	Accepted
8-764	8	33	32	33	34	Sentence is backwards, SOLSTICE is on UARS. [Robert Portmann, United States of America]	Accepted
8-765	8	33	33	33	33	A satellite can't be "on board of" an instrument- reword please [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted
8-766	8	33	33	33	33	"is scaled using" should be "is scaled in the UV using" [Natalie Krivova, Germany]	Accepted
8-767	8	33	40	33	53	The galactic cosmic ray effect description is clearer than in the first draft, I would prefer to say that this effect, out of phase with the solar activity deserves more study to be assessed, I am not in a situation to approve that there is high confidence that it is too weak to influence cloud nucleation. [Christian Muller, Belgium]	Taken into account: This section is a summary of section 7.4.6 and this is based on the assessment made in Ch7
8-768	8	33	51	33	51	Must have missed it but what is SC? [Robert Portmann, United States of America]	Taken into account: SC stands for solar cycle. It is defined at the start of section 8.4.1.
8-769	8	33	55			Section 8.4.2. I would suggest that Gregory (2010, Geophys Res Lett) on the long-term effect on ocean heat content of volcanic eruptions is relevant to this section. It raises the question of what is the appropriate reference level for volcanic forcing, which should probably not be zero because the climate system is an approximate long-term balance with a non-zero occurrence of volcanoes. When there is zero volcanic aerosol, it is in effect a positive forcing. This is a somewhat similar issue to the one you confront in the previous section about solar forcing, which is also one that has relatively large short-term variations superimposed on a possible long-term changes. I tend to think that a running mean would be appropriate for volcanic forcing too, with a meaning period corresponding to the time for ocean heat content to recover from an eruption. This would probably be a small number of decades, but it is not clearly established. [Jonathan Gregory, United Kingdom]	Noted: While in theory, this makes sense, it would change the meaning of RF as it is currently defined, and as the climate system sees it. That is, negative forcing from volcanic eruptions is counter-intuitive. So we did not change the definition as requested.
8-770	8	33	55			Section 8.4.2. Volcanic forcing is not quantified nor included in Table 8.7. If this cannot be done, an explanation would be welcome, especially as you say it is relatively well understood. [Jonathan Gregory,	Accepted: Explanation added.

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						United Kingdom]	
8-771	8	33	55			Section 8.4.2: More discussion is needed on the uncertainties in volcanic emissions and the sensitivity of late-20th century climate simulations to the effects of the several large eruptions during this period. [Larry Horowitz, United States of America]	Accepted.
8-772	8	33				This page is very hard to read and follow as currently written. The authors should consider revising the text to flow more naturally and present the information more clearly. [Government of United States of America]	Noted: Without specifics it is hard to know exactly how to respond.
8-773	8	34	1			Can you give somewhere in this section and estimate of the average RF from Pinatubo and some smaller or larger eruptions. This would be best as a 10-year average (arbitrary, but at least reasonable) to compare with other RFs? [Michael Prather, United States of America]	Accepted. Added here and in Executive Summary
8-774	8	34	3	34	4	Is it clear that volcanic eruptions are the dominant natural cause of climate change on multi-decadal time scales? What is the source of the multi-decadal time scale associated with volcanic effects? The lifetime for sulphate aerosols is quoted as about one year for tropical eruptions and shorter for high-latitude eruptions. Does the multi-decadal variability come from multi-decadal variations in the frequency or intensity of volcanic eruptions? This not made clear. Please see also comment 300 relating to discussion of volcanic effects in Chapter 10. [Adrian Simmons, United Kingdom]	Accepted. It is both the multi-decadal variability and the time scale of the climate system response. This has been made clear.
8-775	8	34	3	34	5	ENSO has a large effect on interannual variability- maybe this should be changed to dominant natural cause of externally forced climate change or something similar? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted.
8-776	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]	Rejected: We don't understand the comment. There is no such text on the lines indicated.
8-777	8	34	6	34	8	The primary reason SO2 (and thus sulfate) makes it to the stratosphere is that it is not washed out in the tropopause region. It is not really just small size and lifetime. [Robert Portmann, United States of America]	Noted, but that is not what is discussed here.
8-778	8	34	9	34	9	add "Global and annual mean of CO2 emission" instead of The emission of CO2 [Government of Poland]	Accepted.
8-779	8	34	9	34	10	The comparison between emissions of CO2 from volcanic eruptions and anthropogenic emissions should be precised. What is the underlying time horizon? [Government of Germany]	Accepted. This is explained now in the text.
8-780	8	34	12	34	12	Stratospheric aerosol lifetime should be two years rather than one year? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Noted. By lifetime, we mean e-folding lifetime, and this is one year. This terminology is used throughout the chapter.
8-781	8	34	13	34	13	change "injections are" to "aerosol results from" [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted.
8-782	8	34	14			Regarding the statement: "Bourassa et al. (2012) showed that sulphur injected into the upper troposphere can then be lifted into the stratosphere over the next month or two by large scale Asian summer monsoon circulation." This assertion has been disputed by Vernier et al., and Fromm et al. in separate Technical Comments submitted to Science, 2012. Therefore, the authors should strongly reconsider including this conclusion. [Government of United States of America]	Accepted. We did not change the conclusion, but we now note the objections of Vernier et al. and Fromm et al.
8-783	8	34	16	34	16	add "the" before "large scale" [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Rejected. Not needed.
8-784	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]	Rejected. This is on p. 40
8-785	8	34	37	34	38	The impact of volcanoes upon the carbon cycle is also discussed in detail in Frolicher et al. 2011. They show that reduction in atmospheric CO¬2 due to a Pinatubo sized volcano is small(~2ppmv)(see figure 1) and thus should have very small effect on RF. [David Paynter, United States of America]	Accepted. This has been added.

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8-786	8	34	42	35	6	It is patently obvious that volcanoes away from the equator will have far less impact on climate than volcanoes near the equator. Fifty percent of the Earth's surface is within 30N to 30S and it's where the primary climate driver, the ENSO operates. (Refer to several statements in IPCC 4AR about ENSO having a significant global impact.) Volcanic eruptions in the tropics cause cooling in that region and, via the Hadley Circulation, temperatures in much of the rest of the world will be impacted. [John McLean, Australia]	Rejected: This is not correct.
8-787	8	34	42			Actually this is not what the figure 8.15 shows - there was a large pulse in 2006, and then a decrease on average for the following 5 years. This is not "an upward trend for the past decade" - also avoid giving temporal references that are vague - give exact years, that way the reader is not confused over the coming 'decade'. [Michael Prather, United States of America]	Accepted. This has been rewritten.
8-788	8	34	46			Is it 'small' or is it 'important' - I do not see how it can be both. If 'small' then important for what? [Michael Prather, United States of America]	Accepted We removed "important"
8-789	8	34	55		57	It might be useful to say how much SO2 this volcano put into the stratosphere (not much?), for comparison with the values reported in the previous paragraph. If the figure is essentially zero, this is worth saying. [Nathan Gillett, Canada]	Accepted.
8-790	8	35	5	35	6	This text is inconsistent with the SPM conclusion (SPM-8, lines 26-27). In this section (8.4.2) the uncertainty range [-0.13 to -0.07] -0.10 W m-2 is not found. The section mentions -0.10, by reference to Solomon et al. 2011, but the uncertainty band is not explicitly mentioned. [Government of Netherlands]	Accepted. Text has been changed and is consistent with SPM.
8-791	8	35	5	35	6	The statement that "mean volcanic forcing for the period 2000-2010 has been estimated to be about -0.1 Wm-2" is not clear. Compared to which period? Late 1990s, or preindustrial? Or is it simply the "volcanic aerosol radiative effect", i.e, the difference to a hypothetical aerosol-free stratosphere? [Räisänen Petri, Finland]	Accepted. It is the radiative forcing. This is now clear in the text.
8-792	8	35	9			Flesh out "shows observations" a bit more - tropical stratospheric profiles.... [Michael Prather, United States of America]	Accepted.
8-793	8	35	33	35	42	Please see comment 240. [Adrian Simmons, United Kingdom]	Rejected. Don't understand the comment.
8-794	8	35	35	35	37	Should mention Gregory (GRL, 2010, doi:10.1029/2010GL045507) since it sheds doubt on the results of Stenchikov et al. 2009. [Robert Portmann, United States of America]	Accepted.
8-795	8	35	36	35	52	Models that are not 100% accurate can't accurately quantify anything. Use the word "estimate" because that's all the models can do. [John McLean, Australia]	Reejcted. This is not correct.
8-796	8	35	37	35	37	"that warms the ocean" is vague- is this a hemispheric warming or the whole Atlantic? Or a smaller region? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted.
8-797	8	35	46	35	47	Clearly the forcing must not be linear if "a substantial part of the solar radiation is blocked" but by then forcing must be huge. Thus, I'm not sure what is being stated here. [Robert Portmann, United States of America]	Accepted.
8-798	8	35	46		47	Say that the forcing is weaker than that predicted based on a linear relationship with the sulphate aerosol injection. [Nathan Gillett, Canada]	Accepted.
8-799	8	35	56		57	Is there a theorteical basis for this 80-year periodicity? I notice that the two papers cited appear to be from climate scientists, but this is a geological question. I'm not an expert on this, but it sounds unlikely. Perhaps cite more critically. [Nathan Gillett, Canada]	Accepted. There is no theoretical basis. This is just a result of a statistical analysis. The text has been changed to reflect this.
8-800	8	35	56			Question at start seems strange, surely we expect several eruptions before 2100? So why concentrate on the next one. Better to say we expect several euuptions over next 100 years but can't predict when? [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted.
8-801	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 expression for the definition of the effect in Chapter 7. [Rubén D Piacentini, Argentina]	Rejected. From the wrong pages.

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8-802	8	36	9	36	30	An important reminder [Robert Kandel, France]	Noted. Thanks.
8-803	8	36	9			Section 8.4.2.5, Volcanic Eruptions as Analogues: Consider putting this section into a short box. Reason - it is not so much an assessment, but rather more general and the style of writing (quite dramatic towards the end) is quite contrasting compared to other sections here. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. It is now in a box.
8-804	8	36	13	36	16	The observation about weaker summer monsoon at the end of 1991 is also consistent with the ENSO, and this is supported by Kijazi,A.L. and Reason, C.J.C (2005) - "Tanzanian rainfall to ENSO link" Ntale, H.K and Gan, T.Y (2004) - "East African Rainfall Anomaly Patterns in Association with El Nino Southern Oscillation" Peel, M.C. et al (2002) - "ENSO impact on precipitation around the world" Don't bury your head in models so much that you ignore natural forces that you struggle to model. [John McLean, Australia]	Rejected. Without complete citations, cannot locate the first or third paper. The second analyzes only the period 1990-1996, and is not relevant to long-term causes of monsoon.
8-805	8	36	18	36	20	"ecology" is woolly- change this to plant physiology and reference the Mercado et al 2009 Nature paper?; "whitening skies" is also a woolly statement which I think should be removed. "as well as the beautiful sunsets" is perhaps not entirely suitable for an IPCC assessment report [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted.
8-806	8	36	18	36	21	The sentence beginning with: "The formation of sulphate aerosols..." is rather long and convoluted. The authors should consider revising the text. Also, is it appropriate to include / is there relevance of "beautiful sunsets" in the context of studying analogues of geo-engineering proposals? [Government of United States of America]	Accepted.
8-807	8	36	21	36	22	Perhaps contrail-generated sub-visual cirrus can be used to test long-term impacts. As shown in Long et al. (2009), the decadal increase in clear-sky downwelling SW reaching the surface for the continental US most likely includes the influence of upper troposphere ice crystal increases from increased commercial air traffic, not the documented decreases in aerosol loading alone, since the diffuse SW increased while the direct SW didn't change. Our historical definition of "clear sky" does include up to an optical depth of 0.15 - 0.2 of primarily sub-visual cirrus (Dupont et al., 2008). With a leveling off of aerosol amount decreases, but continued increases in commercial air traffic, we are effectively already conducting an experiment in this type of increased scattering as a mitigation of greenhouse warming. The authors should consider adding text to this affect with relevant references given below: Dupont J.C., M. Haeffelin, and C.N. Long (2008): Evaluation of cloudless-sky periods detected by shortwave and longwave algorithms using lidar measurements, GRL 35(10), doi:10.1029/2008GL033658. Long, C. N., E. G. Dutton, J. A. Augustine, W. Wiscombe, M. Wild, S. A. McFarlane, and C. J. Flynn (2009): Significant Decadal Brightening of Downwelling Shortwave in the Continental US, JGR, 114, D00D06, doi:10.1029/2008JD011263. [Government of United States of America]	Accepted.
8-808	8	36	24	36	30	Is there any evidence of such widespread cooling from previous nuclear explosions (Hiroshima) that corroborates this? Also, the large uncertainty in these studies on nuclear cooling should be recognised and the fact that only some (not all) volcanic eruptions can be used as analogues. [European Union]	Noted. It is not the explosions, but the smoke. Fortunately, there have not been cities burning with enough data to take direct observations, but forest fire smoke has been used to validate the theory. Yes, only some volcanic eruptions can be used as analogs.
8-809	8	36	26	36	30	The transition from volcanic eruptions to nuclear explosions is rather abrupt and potentially out of place, given the section title. Perhaps the section could be retitled "Short-term climate forcing events" or something similar. [Government of United States of America]	Taken into account, Will be in a box.
8-810	8	36	27	36	30	This sentence is speculative nonsense and has no place in this report. [John McLean, Australia]	Reject.
8-811	8	36	28	36	30	"The use of the current global nuclear arsenal still has the potential to produce continental temperatures below freezing in summer (Robock et al., 2007a; Toon et al., 2008), and the use of 'only' 100 nuclear weapons could produce climate change unprecedented in recorded human history (Robock et al., 2007b)."Indeed. Since the	Accepted. It is now in a box.

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						IPCC is inerently concerned with climate change, nuclear war-triggered climate change should be highlighted further in the AR5 report. [Andrew Glikson, Australia]	
8-812	8	36	28		30	These statements about nuclear winter are very strong with only a limited number of models/references. They certainly should be noted and referenced, but the results as stated here are sensational and may not be as simple as stated here. It certainly depends on how the nukes are used and so I would tame this. [Michael Prather, United States of America]	Taken into account.
8-813	8	36	42	40	50	It would be good if the discussions about forcing agents to a larger degree also include discussions on how humans and societies impact this forcing. This part is an good example where the analysis can be made broader. [Government of NORWAY]	Rejected, this section shows results both by abundance and emissions. A further description of the causes of forcing is not part of Chapter 8
8-814	8	36	42	40	50	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. This is quite throughly discussed in (for example) : 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, details on the aerosol forcing belong to Chapter 7.
8-815	8	36	49		50	Missing a sentence or two: "high precision measurements ... and contrails"? [Michael Prather, United States of America]	Taken into account, by replacing 'and' by as well as'
8-816	8	36				Section 8.5: Perhaps a more appropriate section title would be "Global mean RF, past and future". The word "Synthesis" does not really mean much in this context. At the same time, the key results in section 8.5.1 and Table 8.7 are somewhat lost in the section. The authors should consider using the materials in section 8.5.1 should be used to lead off section 8.3. [Government of United States of America]	Taken into account by changing the section tiltle to 'Synthesis of global mean RF, past and future'. Not agree on moving results to 8.3.
8-817	8	37	3	37	4	The confidence levels expressed here are, according to chapter 1, the author teams' judgements "about the validity of findings as determined through evaluation of the available evidence and degree of scientific agreement." There's at least three glaring problems with this. The "author team" being referred to is, by my judgement, maybe two contributing authors, a lead author and a co-ordinating lead author. That's a whole four people, two of which were nominated by the IPCC and the other two subsequently invited by those nominated. This reeks of bias, besides which the sample of views is far too small to have any credibility. Next, the "evaluation of evidence" is phony when models are used because models do NOT produce evidence; they merely predict outcomes based on their algorithms and the input parameters, everything based in this table that was derived from the output of models should have its "confidence" downgraded. Finally "degree of scientific agreement" is merely a disguise for claiming a consensus, and surely you know that consensus doesn't determine scientific truth. Please find a more honest way to express your views, and make it very clear that it amounts to no more than opinion by a small number of people. [John McLean, Australia]	Rejected, the confidence level is decided by the whole Chapter author team (2 CLAs and 13 LAs) pluss CAs as well as modified by reviewer comments.
8-818	8	37	3	37	6	Climate models that emphasise the influence of CO2 failed to predict the absence of warming over the last 16 years. Because the models were wrong logically you cannot claim that the "confidence level" of CO2 is rated "Very High". [John McLean, Australia]	Rejected, We are discussing the forcing and not how models predict the response.
8-819	8	37	3	37	8	Table 8.6 is very good, especially how it lists changes since AR4. However, the authors should consider elaborating upon the definitions of “Robust”, “Medium” and “Limited” so they are explained more clearly. [Government of United States of America]	Taken into account by adding a sentence in the table caption with further description in Chapter 1.
8-820	8	37	3	38	12	The results presented for tropospheric ozone are confusing - high confidence level in Table 8.6 but medium level of understanding in Figure 8.16. This should be explained somewhere. [Katharine Law, France]	Accepted, by changing the figure from LOSU to confidence level.
8-821	8	37	3	38	25	For aerosol-radiation RF, it is surprising that a high level of confidence is attibuted especially given the lack of information about trends in emissions since pre-industrial time. The arguments supporting this ranking need to appear more clearly in the chapter. [Katharine Law, France]	Taken into account by including a sentence in the caption stating that confidence level for the time evolution may be different than at current time.
8-822	8	37	3			Table 8.6. The terminology "beyond RF" is obscure. This means, I assume, the difference between AF and RF i.e. the effect of rapid adjustment. Could it not be called "forcing adjustment" or something like that (a generalisation of "tropospheric adjustment")? [Jonathan Gregory, United Kingdom]	Taken into account. Rapid adjustment associate with aerosol-cloud interaction and aerosol-radiation interaction is used instead

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8-823	8	37		37		Table 8.6: Entries in rows 5-8 related to aerosols/clouds are very confusing. Why do the row 5-6 entry descriptions start with "RF of…". The entire table refers to RFs! And then in the row 7-8 entry descriptions what does "beyond RF" mean in a table about RF? Are those rows intended to describe RF beyond direct (immediate) radiative responses (such as change in thermodynamic structure by aerosol absorption)? Would then rows 7-8 correspond to AFs rather than RFs (see lines 50-56 in p. 8-24, lines 45-46 in p. 8-38, and Fig. 8.17a,b)? Same comment applies for Fig. 8.16 showing LOSU of RF "mechanisms" (which, incidentally, are called "agents" in table 8.6). [Lazaros Oreopoulos, United States of America]	Taken into account: See comments 8-822
8-824	8	37		104		The LOSU in Figure 8.16 and the confidence level in Table 8.6 do not match for Volcanic aerosol and for Stratospheric water vapor from CH4. [M Daniel Schwarzkopf, United States of America]	Taken into account by using confidence level instead of LOSU in teh figure.
8-825	8	37				I am not convinced by the elevated understanding for the land-use forcing estimate. Given the rather fudged forcing estimate change of -0.2 to -0.15 [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Rejected, the estimate given is based on the available scientific literature.
8-826	8	37				Table 8.6: Given the uncertainties in the simulation of pre-industrial ozone (and implications for RF), why is a "High" level of confidence assigned to RF from tropospheric ozone? [Larry Horowitz, United States of America]	Rejected, the confidence level is based on robust evidence of an increase over the industrial-era and a medium agreement where uncertainties in the pre-industrial level is one of the reasons for a medium agreement. Several forcing mechanisms have lower agreement than for tropospheric ozone changes.
8-827	8	37				table 8.6 states that there is no major change in our understanding for solar irradiance since AR4. Table 8.7 on page 39 states that the solar irradiance estimate has errors of ±.05 W/m2 compared to AR4 with errors ranging from -0.06 to 0.18 W/m2. This seems to suggest that there was a big improvement. [Raimund Muscheler, Sweden]	Taken into account, confindence level for solar is changed.
8-828	8	37				Is the confidence level a reflection of the radiative transfer for the observed amount of material relative to preindustrial, or is it a reflection of the ability to model the amount of the material present? This is an impt distinction. Does confidence level take into account the uncertainty in forcing per amount of material? For example you might know the aerosol optical depth, but not the asym parameter and single scat albedo and thus be substantially in error on the RF. So I am surprised at the high confidence in aerosol radiative interactions .Maybe better if the table included an uncertainty in W m-2 globally. That way one could really get a sense of confidence where it counts. As it stands I don't find the table very useful. Same might be said about Fig 8.16. [Stephen E Schwartz, United States of America]	Rejected, Table 8.6 and 8.7 as well as Figure 8.16 and 8.17 must be seen in connection.
8-829	8	38	1	38	1	About: "Figure [7.1 x])". Remember to complete the "x" value. [Rubén D Piacentini, Argentina]	Rejected, No reference to Figure 7.1x in first line on page 38
8-830	8	38	4	38	5	There you go again claiming that the output of models amounts to evidence, which is a completely bogus claim. [John McLean, Australia]	Rejected, it is the comparison between observations and model results that provide evidence.
8-831	8	38	15	38	15	terminology spelt wrongly [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted: Corrected
8-832	8	38	15	38	15	"terminiology" should be "terminology" [Räisänen Petri, Finland]	Accepted: See 8-831
8-833	8	38	24	38	24	Again "beyond RF" appears, as in Table 8.6 and Fig. 8.16, when no such term was used in the relevant section 8.3.4. [Lazaros Oreopoulos, United States of America]	Taken into account, see comment 8-822
8-834	8	38	34	38	34	Change "Table 8.7 shows the best estimate of the RF for the various RF agents." to "Table 8.7 shows the best estimate of the RF and AF (for AR5 only) for the various forcing mechanisms." [Lazaros Oreopoulos, United States of America]	Accepted, included as suggested.
8-835	8	38	34	38	35	First sentence could be better worded [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Taken into account, sentence rewritten.
8-836	8	38	35	38	36	This is nonsense. Climate models that assumed CO2 to be a major driver of temperature failed to predict the absence of warming over the last 15 years. Your claimed increase in RF due to WMGHG seems to have caused cooling. Clearly your models are wrong. [John McLean, Australia]	Rejected, we are discussing forcing and not temperature changes.
8-837	8	38	46	38	46	change "also compared" to "when compared" ? [Manoj Joshi, United Kingdom of Great Britain & Northern	Accepted, included as suggested.

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						Ireland]	
8-838	8	38	51	38	51	This would be unacceptable from a university student. A time-span of 260 years would be worthless even if empirical data reached back that far, but because that data doesn't (and in fact temperature data coverage is poor prior to 1955), this figure is worthless junk, based only on speculation and the output of models that haven't been verified. [John McLean, Australia]	Rejected, we are discussing forcing and not temperature changes.
8-839	8	39	10			"total anthropogenic RF and AF" - this is ambitious, can you be sure that this includes all uncertainties in attributing the GHG rise to humans alone, for example the natural emissions variations during recent Holocene as noted above? [Michael Prather, United States of America]	Taken into account, we specify the RF for particular year and thus the wording of total anthropogenic has been slightly modified. Uncertainties in the 1750 concentration of WMGHG is taken included as described in section 8.3.2.
8-840	8	39	12	39	15	This is a very important statement (increased anthropogenic RF since AR4) but it is a bit contradictory. Is there really any substantial probability that anthropogenic RF could be negative in total? [European Union]	Taken into account by stating 'virtually certain' a positive ERF and less than 0.1% negative.
8-841	8	39	12	39	15	The statement that the probability of anthropogenic RF being negative is weaker relative to AR4 seems quite odd and potentially quite misleading as to me it suggests that there is still some substantial probability that anthro RF could be negative whereas in actual fact I think this probability is very very low. If the confidence in positive anthro RF is robust and very high there doesn't seem to be any substantial danger of anthro RF being negative. By making this statement here I think it weakens the overall findings that our confidence in the positive nature of anthro RF has increased. So how much chance is there really that anthro RF could be negative? The values given have an estimate of uncertainty that has a 90% chance of encompassing the real value and that spread does not go anywhere near zero to bring the value into the negative. [Kate Willett, United Kingdom]	Taken into account, the total RF is not provided in the final draft. The probability of a negative ERF is given.
8-842	8	39	18			Table 8.7. Uncertainty ranges should be shown consistenly for all reports. I would recommend translating all uncertaies into min,max ranges. At a minimum the AR4 results should be reported as [min,max] ranges, not as numbers to add/subtract from the mean value. This is confusing when compared to the AR5 values. [Nathan Gillett, Canada]	Taken into account, by using (min to max) for the ranges.
8-843	8	39	18			Table 8.7: For the forcings through aerosol-radiation interactions, also show how the estimates for the individual contributions from sulphate, black carbon, organic carbon, nitrate and mineral dust have changed. [Twan van Noije, Netherlands]	Rejected, this is shown in Table 8.5
8-844	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]	Rejected, No line 46 on page 39
8-845	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]	Rejected, No line 55 on page 39
8-846	8	39				Table 8.7 It is unclear whether RF and AF should be summed up. Please, clarify [European Union]	Rejected, already stated on page 38 line 53-54 in SOD
8-847	8	39				Table 8.7. The authors should consider moving the "Solar Irradiance" entry to after the "total Anthropogenic " entry. It is not part of the sum. Also,upon summing the numbers in the AR4 column, one gets 1.7, not 1.6. This would change the 50% to 40%. The 50% value was cited in the chapter summary and the SPM. Or is this a "rounding" issue? The authors should revise the table data, as well as the chapter text, chapter summary and SPM. [Government of United States of America]	Taken into account, solar irriadiance moved after sum anthropogenic. The AR4 RF sum is calculated with Monte-Carlo simulations and note that the uncertainty range are not normal distribution for all agents and thus a simple averaging cannot be made (rejected on this part of the comment).
8-848	8	39				Table 8.7: Should land use change be included in the table? It is in Figure 8.17. [Government of United States of America]	Rejected, the land use is already included in the table (surface albedo (land use)).
8-849	8	39				Table: AR5 Solar irradiance range 0.04 [+/-0.05] includes negative values - is that intended? [Joanna Haigh,	Taken into account, by adopting the best estimate of

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						United Kingdom]	+0.05 (0.0 to 0.10) Wm-2.
8-850	8	39				Table caption needs to specify the time period and the meaning of the square brackets - 5-95%? [Stephen E Schwartz, United States of America]	Taken into account, by including information on time periods. Uncertainty information has been converted to standard AR5 style.
8-851	8	40	20	40	41	For Figure 8.17c the use of vertical error bars is not fully clear to me. I understand e.g. that CH4 also affects O3 production etc., and that the vertical bar may indicate these interactions between components. But what is the basis of this scaling and how to read the graph e.g. with regard to the vertical error bar for CO? [European Union]	Taken into account, by use of much fewer vertical lines
8-852	8	40	20	40	41	The attribution of RF to emitted species is an important result that should be introduced before the "synthesis" section. [Larry Horowitz, United States of America]	Taken into account, this kind of information belongs to the summary section. However, more information on this topic is included in section 8.3.3.
8-853	8	40	20			Fig 8.17c is confusing as there seem to be many ways it could be calculated. I think that you need a simple one or two-line explanation. For example, is it the integrated RF from ALL historical ANTHROPOGENIC? emissions from 1750 to present. How did you calculate those emissions in the first place? Is it consistent with the best current lifetimes and understanding of the budget? What feedbacks are used for OH lifetimes. Is it only the amount, e.g. of impacts left at 2100 from the history of CH4 emissions? Did they include NOx emissions first? (hence more O3) or do NOx emissions get put into a high CH4 atmosphere? (this changes their RF). There are so many arbitrary choices here that I wonder if these values can be supported without so many details. When we evaluate current RF attributable to an emission, it is for a small perturbation and is calculated for the current atmosphere - that is why the CH4 feedback gives us a different perturbation lifetime. I really do not think the results here are reproducible, since there is not unique decomposition of all the historical anthropogenic emissions. A corresponding plot for the 2000s emissions (based on the current atmosphere would be reasonable if calculated as a linear perturbation). [Michael Prather, United States of America]	Rejected, the values included in the figure are based on published litterature. This is now described more carefully in section 8.3.3.
8-854	8	40	23			Replace 'the compounds and changes' with 'the number of compounds'. [Nathan Gillett, Canada]	Accepted, modified as suggested.
8-855	8	40	28	40	30	The affects of CH4 on its own lifetime is not relevant for the RF. It is for the GWP, etc, but not the time series of RF. [Robert Portmann, United States of America]	Rejected, it is relevant in the attribution view.
8-856	8	40	29		30	How can the effect of CH4 emissions on the CH4 lifetime explain a difference in the radiative forcing attributed to emissions versus the radiative forcing attributed to concentration? Any effect of CH4 emissions on the CH4 lifetime will affect the measured CH4 concentration, and therefore will be included in the estimate of RF due to CH4 concentration change, won't it? [Nathan Gillett, Canada]	Taken into account by further clarifications in the text.
8-857	8	40	30		31	What is the difference between the 'direct methane greenhouse affect' and the 'RF from abundance change of CH4'? I didn't understand this sentence. [Nathan Gillett, Canada]	Taken into account, see comment 8-856
8-858	8	40	36	40	37	It is not immediately obvious how emissions of ammonia lead to decreases in sulfate aerosol. Please provide a reference or explain further. [Government of United States of America]	Taken into account by modification of the sentence.
8-859	8	40	38		39	There is an apaprent contradiction here between the effects which are 'typically small' and the 'large divergences' between models. How about ''models typcailly simulate a small effect, but there are large relative differences in the response bewteen models'. [Nathan Gillett, Canada]	Accepted, sentence modified as suggested.
8-860	8	40	47	40	48	It is not clear what is meant by this sentence. The authors should clarify the text. [Government of United States of America]	Taken into account, 'anthropogenic' has been included in this sentence. See also 8-861.
8-861	8	40	48	40	48	Delete the word "produces". [Larry Horowitz, United States of America]	Accepted, modified as suggested.
8-862	8	40	52	42	21	This is also a part where the human and society as drivers of RF in a historical context can be made clearer. [Government of NORWAY]	Rejected, the suggestions is beyond the scope of the chapter.
8-863	8	40	56			Is 'nearly' needed here? [Nathan Gillett, Canada]	Taken into account, 'nearly' replaced by 'almost'.

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8-864	8	40	58	41	1	Don't be so dishonest. The original CO2 data from ice cores showed similar levels 88 years prior to the start of Mauna Loa observations so without any justification that ice core data was date-shifted to make it append. [John McLean, Australia]	Rejected, the data used in this figure is based on the scientific litterature, see Chapter 6.
8-865	8	40		41		Section " 8.5.2 Time Evolution of Historical RF". It would be useful to mention that uncertainty in emissions is not considered. While the time path of some emissions, such as SO2, is relatively well known (Smith et al. 2011), particularly in comparison to forcing uncertainty, this is not true for BC and OC emissions. (We're working on this, but won't be available in time for AR5). So the time path of these components is particularly uncertain. This would add to the uncertainty calculated here. [Steven Smith, United States of America]	Accepted, a sentence is added in the figure caption to state what is included in the uncertainties as well as stating that relative uncertainties can be larger for the time evolution than at present.
8-866	8	40		41		Also should mention that the impact of different regional responses was (I presume) not considered. Changing SO2 over oceans, for example, might have a particularly large impact on trends, as would any saturation in aerosol effects (even direct effects) over china, which dominates global SO2 emissions in recent years.This would add to the uncertainty calculated here. [Steven Smith, United States of America]	Rejected, reginal changes in the emissions are included in the model simulations.
8-867	8	40		41		There is a somewhat fundamental problem with the forcing bounds estimated in this chapter and that is that they are simply statistical combinations of independent estimates without any observational constraint. This problem existed in AR4 as well, and as a result, the AR4 bounds for aerosol forcing were not consistent with some of the observational constraints. Fortunately, this problem looks to be less severe in the AR4. However, this chapter needs to refer more to the results of attribution studies (Stott et al 2006,Forest et al 2006, Shindell & Faluvegi 2009, etc.), observational bounds (Murphy et al. 2009), and simple models (Andronova and Schlesinger 2001) that all provide bounds on total aerosol forcing, some of which are assessed in other chapter. While these estimates are somewhat contradictory this all should be assessed in conjunction with the bounds calculated in this chapter through statistical combination. [Steven Smith, United States of America]	Rejected, the scope of the forcing chapter is to be based on bottom-up estimates of the forcing. Other chapters discusses inverse estimates.
8-868	8	41	8	41	21	Could refer to obs in Chapter 2 here [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted, a reference to Chapter 2, section 2.2.3 is included.
8-869	8	41	8		9	This is an astonishing statement. You seem to be saying that there is less confidence in forcing over the past 20 yrs when we had some pretty good, pretty dense measurements than in 1950-80 when measurements were poor and sparse. Are you sure you want to say this? [Stephen E Schwartz, United States of America]	Taken into account by adding 'in the relative change in'
8-870	8	41	20		21	The meaning of this sentence wasn't completely clear to me. This is comparing the absolute magnitudes of the aerosol and GHG RFs, isn't it? In that case how about 'the ratio of aerosol RF to WMGHG RF during this period was much smaller than the ratio over the 1950-1980 period'. Or is this comparing the trends in forcing over the two periods? [Nathan Gillett, Canada]	Taken into account by using 'offset from the aerosol forcing'
8-871	8	41	23	41	37	See comment on page 33 line 55 on Section 8.4.2. [Jonathan Gregory, United Kingdom]	Rejected, we continue to use 1750 as the reference. This is an important comment and we have discussed various ways to present the volcanic forcing. We tried different methods for the reference level, but found the most appropriate way as already have been adopted.
8-872	8	41	23		42	another chance to give the volcanic RF as a decadal mean - I hope that is what Table 8.8 is about, but the caption is not clear: is this the average RF (relative to 1750) for the period shown? or is it merely the change in RF, e.g. from 2000 to 2010? I hope the former as that usefully highly the mean forcing over periods (i.e., it is more consistent with the GWP concept). The 1990-2000 numbers should show Pinatubo instead of an net zero natural RF? [Michael Prather, United States of America]	Taken into account by removing Table 8.8 and replace it with a figure shown trends in forcing over certain time periods.
8-873	8	41	24	41	25	it is stated that "some weaker eruptions give a current RF that is slightly negative". Again, it should be made clear which year is used as the baseline for defining the RF. [Räisänen Petri, Finland]	Taken into account by adding 'relative to 1750 and slightly stronger in magnitude compared to 1999-2002'
8-874	8	41	27	41	28	Also in lines 40-41 (Table 8.8): How were these numbers derived and/or is there a reference that can be provided? Same applies for Table 8.9 caption and related text. [Lazaros Oreopoulos, United States of America]	Taken into account: see 8-872
8-875	8	41	27	41	37	The text says Table. 8.8 shows RF for "exact years" as well as 5-year means -- so for which exact years are	Taken into account: see 8-872

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						RF values given in this table? It is not clear from the table format. [Government of United States of America]	
8-876	8	41	27	41	40	I'm not clear whether this discussion and Table 8.8 are referring to changes in RF between year i and year j, or the time-mean for year i-j of RF wrt pre-industrial (like Table 8.7). It is probably the former, but in that case I wonder why you say, for instance (line 33), "the 2000-2010 natural RF is negative", rather than, "the change in natural RF from 2000 to 2010 is negative"? Similarly, you could say, "the natural RF has not changed since 1950", rather than "it has been close to zero". I appreciate that really RF is only ever meaningful as a change, but there is a potential for confusion unless this is spelled out. [Jonathan Gregory, United Kingdom]	Taken into account: see 8-872
8-877	8	41	27	41	44	This is quite a complex and confused discussion and table re volcanoes and average - what are the years around 1990 and 2010 to get an average forcing of 0.83? [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Taken into account: see 8-872
8-878	8	41	27	42	21	If all of this is just difference or rates of change in RF (same thing), then 2 tables and 2 figures seems like overkill. The key missing link here is how this is a useful metric for any climate change - either attribution or predictability. [Michael Prather, United States of America]	Taken into account, Table 8.9 is removed.
8-879	8	41	29	41	29	Should "to 1.0 W/m2" be "by 1.0 W/m2"? The time-integrated RF is higher. [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Taken into account, Table 8.9 is removed and text modified.
8-880	8	41	30		32	The text here needs to be more explicit about how the natural forcing is calculated here (it is a difference in RF at the end points of the 30 year period). The phrase 'over the past three decades' implies a trend to me rather than a difference in the end points. Also I think it is an understatement to say that the natural RF depends 'slightly' on the method of calculation. For example a linear trend would probably give a relatively large natural forcing trend over this period. [Nathan Gillett, Canada]	Taken into account, Table 8.9 is removed and text modified, see also 8-872
8-881	8	41	40	41	41	It strikes me that the information shown here is derived from the variation in temperature, more specifically when models are tweaked to produce results that are as similar as possible to observations. You have no evidence that the models are accurate (and how could you claim that when the models have never been validated and the level of understanding of many climate forces is poor?) so the figures here amount to mere speculation. [John McLean, Australia]	Rejected, the figures and results are for forcing and bottom-up estimates. No inverese methods using temperature have been adopted in the derivation of these results.
8-882	8	41	40	41	42	I found this table hard to decipher- the caption says 5 year means despite the time-periods being different: are the numbers in brackets averages of 5 year means or something else? Also the natural RF for the period 1990-2010 is zero, despite Mt Pinatubo's eruption in 1991: can someone check this? Pinatubo's effect would be about -0.2 W/m2 averaged over 20 years maybe- that's more than the RF estimated for solar irradiance changes since pre-industrial times from Table 8.7 [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Taken into account, see comment 8-872
8-883	8	41	51	42	1	Please indicate in the caption that Tab 8.9 shows four periods, not three. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account, see comment 8-872
8-884	8	42	10	42	12	The figure 8.18 caption refers mostly to RF while the y-axis label of the figure indicates AF. This inconsistency should be resolved. [Lazaros Oreopoulos, United States of America]	Taken into account by more specific use of ERF.
8-885	8	42	14			Again I am confused, the text and figure discuss RF for the period 1980-2010. I would think that Chichon and Pinatubo would show up when averaged over this period. If it is the instant RF(2010) minus RF(1980) then say that. If the latter then it is not a very useful diagnostic because the RF could have and dis change in non-liner ways in between and the absolute, instant difference does not tell us about what forced climate over those three decades. [Michael Prather, United States of America]	Taken into account, see comment 8-872
8-886	8	42	16			Replace 'posotive AF' with 'net positive AF'. [Nathan Gillett, Canada]	Accepted, 'net' included.
8-887	8	42	23	43	57	You should consider including the work on future radiative forcing from HFCs. See the work of Velders et al. on the large contribution of HFCs to future forcing Velders, G. J. M., et al. (2009). "The large contribution of projected HFC emissions to future climate forcing." Proceedings of the National Academy of Sciences 106: 10949-10954. This might be a place to refer to replacement of high GWP HFCs with low GWP HFO materials e.g the replacement of HFC-134a with HFO-1234yf in automotive air conditioning systems. See No. 4 above.	Accepted. We have added a sentence about future HFCs in the RCPs in comparison with Velders et al.

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						[Robert Waterland, United States of America]	
8-888	8	42	23	44	15	In terms of future RF, this section misses one key part: the re-evaluation of the RCP scenarios for being in error in terms of anthropogenic emissions and in terms of projecting chemistry and carbon cycle (both of which are analyzed and discussed in Chapters 11 and 12). This was all occurring at once, but we now have published alternatives that use current science. Can we now merge the pieces so that the uncertainties are included correctly? [Michael Prather, United States of America]	Accepted. We have added references to the relevant sections of chapters 11 and 12 on this topic.
8-889	8	42	23	44	16	Suggest future forcing to modeling chapter. [Stephen E Schwartz, United States of America]	Rejected. Not an evaluation (as no future observations), so ch 9 does not want this there, and as it's forcing, we feel belongs in ch 8.
8-890	8	42	23			It might be worth mentioning somewhere here that most CMIP5 models did not include nitrate aerosols (I think this is true, isn't it?). Given the importance of nitrate aerosol demonstrated here, this is important information for other chapters seeking to understand simulated future trends. [Nathan Gillett, Canada]	Accepted, revised.
8-891	8	42	23			Section 8.5.3. I feel that uncertainty should be more evidently quantified in this section. In ch12 and ch13 the projections for global mean surface air temperature change and sea level change respectively for RCPs are given with likely changes. A similar treatment of RF or AF would be valuable here, if possible. Forster et al (2012, which you cite) quantify the AF model spread. I think it's a significant conclusion that the fractional spread across the models declines during the 21st century, because of the decreasing importance of aerosol forcing. I see that forcing projections are also dealt with in 12.3.3. For a reader of the report, it is unclear what the distinction is between these two sections, and it might be more logical to merge them, and have a section on this subject in either ch8 or ch12, but not both. [Jonathan Gregory, United Kingdom]	Accepted. We have added discussion on the uncertainty in the projected forcings, including their change with time.
8-892	8	42	23			Section 8.5.3: It should be acknowledged that the emissions of aerosols and most short-lived reactive gases (except NH3) are on the low end of the range of possible emissions. For aerosols, tropospheric ozone and methane, it is therefore necessary to compare the future radiative forcing projections from ACCMIP with other projections based on alternative emission scenarios, e.g. those by Stevenson et al. (2006). We have recently presented alternative RCP-like scenarios with varying assumptions on emission factors. This new set of emission scenarios is more representative than the original RCPs in terms of the possible range of emissions of aerosols and short-lived reactive gases. In our study we also give estimates of the implications for the future radiative forcings of aerosols, ozone and methane (Chuwah, Van Noije, Van Vuuren, Hazeleger, et al., Implications of alternative assumptions regarding future air pollution control in RCP-like scenarios, Atmospheric Environment, submitted). Assuming the paper will be published before the AR5 deadline, it would be worthwhile to refer to it in this section (see comments no. 16 and 17). [Twan van Noije, Netherlands]	Noted. This paper was not accepted in time, so could not be cited. We do acknowledge this point, however (e.g. see revised Figure showing emissions in RCPs vs other literature).
8-893	8	42	27	42	27	This section uses the phrase "RCP Database" to refer to the MAGICC 6 results of (Meinshausen et al., 2011a). This is an incorrect usage and must be changed. The RCP process was designed to provide *emissions*, *concentration*, and *land-use* results for model inter-comparison studies. The ozone and harmonized emission results from Meinshausen et al., (2011a) (speaking as one of the authors of this work), are, of course, research results that are there to be used, of course, but are not official RCP data products. Therefore it is misleading to refer to this as the "RCP Database". The *only* official RCP data set are the data released at IIASA at http://www.iiasa.ac.at/web-apps/tnt/RcpDb (e.g., emissions, GHG concentrations, and land-use changes). Any other data should be referred to by its journal article reference. [Steven Smith, United States of America]	Accepted, revised.
8-894	8	42	38	42	39	It would be informative to mention here that nitrate is projected to increase due to increases in NH3 emissions (despite decreases in NOx emissions), while sulfate decreases due to projected delclines in SO2 emissions. [Government of United States of America]	Accepted, revised.
8-895	8	42	39	42	41	Figure 8.20 is confusing since the labels just say O3 (strat. plus trop.?) and strat. H2O and the text talks about total ozone (strat. plus trop.) in separate points. It would be clearer to show the changes due to trop. and strat. O3 and strat. H2O separately in the Figure. [Katharine Law, France]	Noted. Quantitative values for tropospheric and stratospheric ozone were not reported separately in the Shindell et al ACCMIP paper, only the total. So we quote the total values, but include discussion of the two regions to clarify why the totals behave as they do. This can be assessed based on the ACCMIP

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							ozone results of Stevenson et al, which are for tropospheric ozone only (but using a different setup, so not quantitatively comparable but qualitatively similiar).
8-896	8	42	52	43	4	Explain what is meant by "RCP database ozone." How is this different from the ACCMIP results? [Loretta Mickley, United States of America]	Accepted. Revised as suggested in review comment 893.
8-897	8	42	52	43	4	Stevenson et al 2012 (Figure 7/Table 10) has tropospheric O3 RFs for the 4 RCPs - I am not quite sure if this is where the quoted 'ACCMIP' numbers originate or if they are from the Shindell et al 2012 paper (or another). But there should be a clear reference here. [David Stevenson, United Kingdom]	Accepted, revised.
8-898	8	42	52			ACCMIP forcing is compared with RCP database forcing, but no information is given on how ozone RF was calculated in the RCP database, and there is no indication of which is more trustworthy. I guess that ACCMIP is more reliable, but this needs to be assessed. [Nathan Gillett, Canada]	Accepted, revised.
8-899	8	43	12		17	Again, which is more reliable, ACCMIP or the RCP database? How were the forcings calculated in the RCP database? [Nathan Gillett, Canada]	Accepted, revised.
8-900	8	43	14	43	14	units missing for 0.3 [Räisänen Petri, Finland]	Accepted, revised.
8-901	8	43	25	43	26	Remove "become wealthy and hence" as it is not necessary and is debatable whether it is the only way it could come true. [Robert Portmann, United States of America]	Rejected. This is an explicit assumption in the IAMs, which directly link wealth to air quality regulations. Hence we maintain this.
8-902	8	43	38		39	Why does this follow? Is this because the integrated assessment modellers do a good job of estimating the forcing due to WMGHGs but not that due to other forcings? [Nathan Gillett, Canada]	Accepted, revised.
8-903	8	43	45			much stronger' seems like an overstatement here. In Fig 8.20b the aerosol AF over the same period is about 0.8 W/m^2. [Nathan Gillett, Canada]	Noted. Sorry, we cannot find what this refers to.
8-904	8	43	49			"indicated" should be "indicating". [Adrian Simmons, United Kingdom]	Accepted, revised.
8-905	8	44	5	44	5	I can't see ranges shown in Fig 8.20 (I would like to). I assume these are bar charts of best estimates, aren't they? [Jonathan Gregory, United Kingdom]	Noted. Error bars are definitely in final figure (they were there earlier, but for some reason didn't show up in the pdf).
8-906	8	44	22	44	22	I’m not sure I agree with the characterization of the CO2 RF as spatially homogeneous. This is not what Fig. 5a of Taylor et al. (2011) shows. See also CO2 RF plots at http://data.giss.nasa.gov/efficacy/. For example 2xCO2 AF according to the GISS model has a zonal range of ~2 to 4.8 Wm-2 and an RF from 2.3 to 5.3 Wm-2. [Lazaros Oreopoulos, United States of America]	Accepted. The sentence has been edited to reflect the distribution in the literature.
8-907	8	44	24	44	27	Do you mean short-lived components of WMGHGs or the NTCFs here? [Kate Willett, United Kingdom]	Accepted. NTCF's - this is now clarified.
8-908	8	44	29			What are NTCFs? (Maybe they are defined earlier, I haven't read the whole chapter) [David Stevenson, United Kingdom]	NTCF’s are defined in Box 3.1, we now refer to the Box for definition in the previous paragraph where the acronym is first used.
8-909	8	44	51	44	58	Also in p. 8-45, lines 1-10: The text here uses the non-compliant with AR5 (e.g., see Fig. 7.2) terminology of “direct”, “indirect” and “semi-direct” aerosol effect. [Lazaros Oreopoulos, United States of America]	Accepted, this has been "translated" to the new terminology.
8-910	8	44	53	44	53	A recently published paper is highly relevant to the discussion on the spatial structure of adjusted forcing, and should be cited. Its main finding is that a forcing imposed persistently over a specific region may alter the radiative balance elsewhere through atmospheric circulation, thus giving rise to a nonlocal component of the adjusted forcing. The reference is Ming, Y. and V. Ramaswamy (2012), Nonlocal component of radiative flux perturbation, Geophys. Res. Lett., 39, L22706, doi:10.1029/2012GL054050. [Yi Ming, United States of America]	Accepted, this paper is now cited in section 8.6.2.
8-911	8	44				Section 8.6: Maps indicating changes in emissions over time of various forcing agents used in historic ACCMIP simulations and for the different RCP scenarios would be helpful in interpreting the regionally varying	Accepted. These are being added to the Supplementary material.

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						changes in RF. Maybe these maps are located elsewhere (not in Ch. 8) in the report? If so, please refer to the specific location. [Government of United States of America]	
8-912	8	44				The spatial distribution of aerosol and ozone RF (or AF) is documented in substantial detail (Figures 8.22-8.25), but no figure is provided for WMGHGs (the most important anthropogenic forcing agents) or total anthropogenic RF or AF (to which climate responds). Please, consider adding such a figure. [Räisänen Petri, Finland]	Accepted. A figure showing total ARF is added (to Figure 8.22?)
8-913	8	45	12			Do you mean the patterns are similar? The magnitudes of surface forcing can be much larger [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Rejected, for scattering aerosols the TOA and surface forcing are indeed similar.
8-914	8	45	14	45	15	Ming et al. (2010) should be cited. [Yi Ming, United States of America]	Rejected. Due to space limitations only representative publications are included.
8-915	8	45	15	45	17	RE: the impact of absorbing aerosols on atmospheric circulation and cloud formation, Persad et al. (2012) should be cited. [Yi Ming, United States of America]	Rejected. Persad et al is cited on the previous page in a similar context and the citations need not be repeated here.
8-916	8	45	17		18	What is 'AF at the … surface'. Isn't AF defined at TOA? It would be helpful if AF were formally defined in the chapter. Then use another term for AF at the surface. [Nathan Gillett, Canada]	Accepted. This is now explicitly defined.
8-917	8	45	24			gas-phase only models (Stevenson et al 2012)' - whilst this paper is about tropospheric O3 RF, several of the models employed also simulate aerosol, and include aerosol-gas phase interactions. It is incorrect to call the models 'gas-phase only'. [David Stevenson, United Kingdom]	Noted. This is rephrased to clarify that some but not all of the models are gas-phase only.
8-918	8	45	27			Stevenson et al 2012 does not explicitly discuss inter-model standard deviations of ozone changes. Stevenson et al (2006) does (Figs.3,4,5). Young et al (2012) does discuss SDs of O3 distributions, but not SDs of the changes (I don't think - possibly it is within the supplementary material). So I am unsure you can make this statement with respect to ACCMIP results. [David Stevenson, United Kingdom]	Accepted. Young et al. is a better reference, so we have used it instead.
8-919	8	45	30	45	31	How do these medium levels in confidence in spatial patterns of aerosol and ozone RF equate with the high levels of confidence given in Table 8.6 for overall RF. There appears to be an inconsistency here, particularly for aerosols. In addition, historical forcing patterns are given a low confidence level (Page 46, lines 36-37). Some discussion about these points should be added to the text. [Katharine Law, France]	Noted, and now explanation is provided. The size of the model standard deviation (>1/2 the mean value) is the reason to degrade the confidence.
8-920	8	45	38	45	38	Replace “atmospheric absorption” with “aerosol atmospheric AF”, to be consistent with the y-axis label of the corresponding panel. [Lazaros Oreopoulos, United States of America]	Accepted. The Figure label text has been changed.
8-921	8	45	42	45	44	The figure caption for Fig. 8.23 is incomplete. It does not explicitly state what the “preindustrial to present-day RF” refers to. Also, it must be perhaps clarified why the first row of panels in Fig. 8.23 is identical to the first row in Fig. 8.22, while the third row of panels in Fig. 8.23 is not the same as the second row in Fig. 8.22. [Lazaros Oreopoulos, United States of America]	Accepted. The years for the simulations are now specified in Fig 8.23 and Fig 8.22. An explanation for difference in carbonaceous fields is added to Fig8.23 caption.
8-922	8	45	50	46	7	This part is a good example where the findings are put into a societal context. [Government of NORWAY]	Noted.
8-923	8	45	50	46	13	Please quantify these changes in emissions and provide references. [Loretta Mickley, United States of America]	Accepted. Reference is now made to supplementary figures showing regional trends in emissions and regional maps of emissions. Reference for historical emissions is added.
8-924	8	45	50			Don't the different WMGHGs have somewhat different forcing patterns? So wouldn't some change in forcing pattern have occurred over time as the relative contributions from the different WMGHGs changed? [Nathan Gillett, Canada]	Accepted. The WMGHG's have fairly similar forcing distributions, but we have added a qualifier to the statement.
8-925	8	46	20	46	20	I would use the term land desertification (mostly due to overuse of natural rangelands and unsuitable agricultural practices) here [European Union]	Accepted, desertification added.
8-926	8	46	28		29	When I read this I initially thought the reductions referred to were reductions in SO2 emisions, but I see now that the text is referring to reductions in surface SW. Clarify this. 'Ongoing reductions' implies a progressive	Acceped, this has been clarified.

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						decrease in time to me, but I don't think this is what is meant. [Nathan Gillett, Canada]	
8-927	8	46	39	48	55	Section 8.6.2.2: I find this tiny section on the huge topic on the relations between climate response and climate forcing to be misplaced. There are several chapters later in the report that treats this important topic adequately. The discussion here is, in my opinion, based on a very narrow selection of studies and one specific conceptual view. I suggest skipping this part from Chapter 8, and leave this important topic to the later chapters that are dedicated for climate response analysis. [Trond Iversen, United Kingdom of Great Britain & Northern Ireland]	Rejected. Better introduction and explanation of the purpose of the section are added. The goal here is to address the relatively new research topic of how regional forcing distribution influences response. The robustness is based on multiple models getting similar results, however the evidence of signal relative to internal variability is not yet robust. Cross-references to other chapters (10, 11) are added. The list of topics is fairly comprehensive, and the studies referenced are representative, but both of these are limited by page availability.
8-928	8	46	39	48	55	Much of what is discussed on this section seems to belong in other chapters. There also is repetition of what is in this Chapter 8, for example in the discussion of e-folding times of stratospheric aerosols from volcanic eruptions. [Adrian Simmons, United Kingdom]	Noted. There is improved cross-referencing to other chapters. However this is the only place in the IPCC that addresses the specific topic of how regionally varying forcing influences climate. An introduction has been added to clarify the purpose of the section. The text on volcanic e-folding differs from what is earlier in the chapter, but a reference to the earlier text is provided here as well.
8-929	8	46	39			Section 8.6.2.2. I see the value in this discussion, but I am not sure it belongs in this chapter. The response to forcing is really the central subject of chapter 10. Could you consider moving this section there, where I would logically expect to find it as a reader? [Jonathan Gregory, United Kingdom]	Rejected. The purpose of this section is better introduced now, which is to explore the effects of regional variability of forcing.
8-930	8	46	41	46	50	Section 8.6.2.2 should contain some discussion of how trends in absorbing aerosols and ozone likely lead to a more diffuse climate response than trends in scattering aerosols, as well as discussion of how feedbacks involving soil moisture can amplify the local climate response to changing aerosols. (E.g., in regions with moist soil, the climate response to declining aerosols is diffuse; in regions with dry soils, the response is more localized.) See Discussion section of Mickley et al. (2012). Mickley, L.J., E.M. Leibensperger, D.J. Jacob, and D. Rind, Regional warming from aerosol removal over the United States: Results from a transient 2010-2050 climate simulation, Atmos. Env., 46, 545-553, 2012. [Loretta Mickley, United States of America]	Rejected. These points are interesting however we do not have space to include detailed results based on a single study.
8-931	8	46	49	46	50	Observations suggest a link between aerosol trends and regional temperature change. See following papers: (1) Ruckstuhl, C., Philipona, R., Behrens, K., Coen, M.C., Durr, B., Heimo, A., Matzler, C., Nyeki, S., Ohmura, A., Vuilleumier, L., Weller, M., Wehrli, C., Zelenka, A., Aerosol and cloud effects on solar brightening and the recent rapid warming. Geophys. Res. Lett. 35, L12708, 2008. (2) Philipona, R., Behrens, K., Ruckstuhl, C., 2009. How declining aerosols and rising greenhouse gases forced rapid warming in Europe since the 1980s. Geophys. Res. Lett. 36, L02806, 2008. (3) Krishnan, R., and Ramanathan, V., Evidence of surface cooling from absorbing aerosols. Geophy. Res. Lett. 29, 2002. [Loretta Mickley, United States of America]	Accepted, the 2 more recent references are included in the following paragraph.
8-932	8	46	50	46	50	Joshi et al (In review in J Clim 2012) examine how spatially varying forcing affects the land-sea warming contrast (full reference for the manuscript is in WG1 chapter 12) [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Rejected. We were not able to find this paper.
8-933	8	46	54		55	Shindell et al. (2009) doesn't mention the Arctic. Should this be Shindell and Faluvegi (2009)? First this is an attribution study of observed temperature changes, and is assessed in 10.3.1.1.4. Second, if retained, the discussion here should note that this study uses an inverse approach to infer the aerosol response, rather than a forward model to predict the aerosol response from the forcing. There is therefore a danger that some part of the change due to internal variability might be attributed to aerosols in this approach. [Nathan Gillett, Canada]	Accepted, the reference should have been Shindell and Faluveig (2009). The text has been revised to note the inverse method, and a pointer to section 10.3.1.1.4 has been added.
8-934	8	46				Section 8.6.2.2. I found several problems with this section. First, much of the section describes the attribution of observed climate changes to particular forcings. In several cases attribution assessments are made which lack appropriate uncertainty qualifiers, are too confident, and/or are at odds with assessments made in chapter	Noted. Chapter 8 is addressing a topic that is generally not mature enough for formal detection and attribution and yet has demonstrably robust results

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						10. Second, I found that the scope of this section was not clearly defined. There are a large number of climate variables, and a large number of forcings and the section just considers a subset of the combinations of these. I strongly suggest that this section should be recast to focus exclusively on mechanisms of response to spatially inhomogeneous forcing, and that it should not cover the attribution of observed climate changes, but focus on simulations and theoretical explanations. I think the material covered in the first two paragraphs on this section belongs here, but much of the rest of the subject matter is covered in chapter 10, and should not also be covered here. [Nathan Gillett, Canada]	based on multiple model sensitivity studies. This is now better explained. Confidence estimates have been revised to be more consistent with those available from Chapter 10.
8-935	8	46				Section 8.6.2.2: It would be useful to have section 8.6.2.2 lead off section 8.6 as motivation for examining spatial distribution of the forcing. [Government of United States of America]	Noted. We have added a brief statement to the beginning of 8.6 motivating the need to consider spatial pattern and pointing to section 8.6.2.2.
8-936	8	47	4	47	4	here evapotranspiration is correct [European Union]	Noted. We assume page 48 line 4 is meant.
8-937	8	47	5	47	10	In their model study, Mickley et al. (2012) found that the spatial pattern of surface warming over the United States strongly corresponded to the pattern of aerosol forcing. The warming was amplified by positive feedbacks involving soil moisture and low cloud cover. Mickley, L.J., E.M. Leibensperger, D.J. Jacob, and D. Rind, Regional warming from aerosol removal over the United States: Results from a transient 2010-2050 climate simulation, Atmos. Env., 46, 545-553, 2012. [Loretta Mickley, United States of America]	Accepted. This study is now included.
8-938	8	47	8	47	8	Leibensperger et al. (2008) was not about the climate response to local aerosols. [Loretta Mickley, United States of America]	Accepted, this reference has been removed.
8-939	8	47	8	47	8	Both Leibensperger et al. (2012a) and (2012b) should be cited here. Leibensperger, E.M., L.J. Mickley, D.J. Jacob, W.-T. Chen, J.H. Seinfeld, A. Nenes, P.J. Adams, D.G. Streets, N. Kumar, and D. Rind, Climatic effects of 1950-2050 changes in US anthropogenic aerosols - Part 1: Aerosol trends and radiative forcing, Atmos. Chem. Phys., 12, 3333-3348, 2012a. Leibensperger, E.M., L.J. Mickley, D.J. Jacob, W.-T. Chen, J.H. Seinfeld, A. Nenes, P.J. Adams, D.G. Streets, N. Kumar, and D. Rind, Climatic effects of 1950-2050 changes in US anthropogenic aerosols - Part 2: Climate response, Atmos. Chem. Phys., 12, 3349-3362, 2012b. [Loretta Mickley, United States of America]	Accepted, both studies are cited.
8-940	8	47	8	47	8	More should be made of the Leibensperger et al. (2012b) study. In their model study, Leibensperger et al. (2012b) found that trends in U.S. aerosol sources may have contributed to the observed cooling over the central United States during the mid-twentieth century, a phenomenon sometimes known as the U.S. "warming hole." Previous climate model studies attributed the warming hole to variations in SSTs in the tropical Pacific (Robinson et al., 2002; Kunkel et al., 2006; Wang et al., 2009). Leibensperger, E.M., L.J. Mickley, D.J. Jacob, W.-T. Chen, J.H. Seinfeld, A. Nenes, P.J. Adams, D.G. Streets, N. Kumar, and D. Rind, Climatic effects of 1950-2050 changes in US anthropogenic aerosols - Part 2: Climate response, Atmos. Chem. Phys., 12, 3349-3362, 2012b. Robinson, W. A., R. Reudy, and J.E. Hansen, General circulation model simulations of recent cooling in the east-central United States, J. Geophys. Res.-Atmos., 107, 4748–4761, 2002. Pan, Z. T., et al., Altered hydrologic feedback in a warming climate introduces a “warming hole”, Geophys. Res. Lett., 31, 2002. Kunkel, K., X.Z. Liang, J. Zhu, and Y. Lin, Can CGCMs simulate the twentieth-century “warming hole” in the central United States?, J. Climate, 19, 4137–4153, 2006. [Loretta Mickley, United States of America]	Rejected. Although interesting, we lack space for extensive discussion.
8-941	8	47	12	48	34	Not sure hydrology belongs in a forcing chapter; or large scale circulation. Suggest chapter stay focused on forcing. [Stephen E Schwartz, United States of America]	Rejected, the assigned section-topic is the climate response to spatially varying forcing; the hydrology is a climate response. See also chapter response to comment 927.
8-942	8	47	12		26	This paragraph is on the topic of the attribution of observed circulation and precip trends, and belongs in chapter 10. Also it seems overconfidence on the topic of the attribution of observed precip trends to aerosols compared to 10.3.2.2. [Nathan Gillett, Canada]	Rejected. As explained above (e.g. comment 927) the section is on how spatially heterogeneous forcing influences climate, and this would include aerosol effects on hydrology. Reference to discussions in Ch 10 and 11 are added, as well as attribution conclusion from 10.3.2.2.

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8-943	8	47	16	47	16	Ming et al. (2007) is the wrong reference. It should be Ming and Ramaswamy (2009, 2011). The references are: Ming, Y., and V. Ramaswamy, 2009: Nonlinear Climate and Hydrological Responses to Aerosol Effects. Journal of Climate, 22, 1329-1339. Ming, Y., and V. Ramaswamy, 2011: A Model Investigation of Aerosol-induced Changes in Tropical Circulation. Journal of Climate, doi:10.1175/2011JCLI4108.1. [Yi Ming, United States of America]	Accepted. The 2nd of these references is used.
8-944	8	47	16	47	17	Ming and Ramaswamy (2011) also discussed the changes in the meridional streamfunction and Hadley and Walker circulation caused by aerosols, and should be cited along with Williams et al. (2001). [Yi Ming, United States of America]	Accepted. This sentence is rewritten to include Ming and Ramaswamy (2011).
8-945	8	47	23	47	25	Worthwhile to mention that the current generation of clean technology reduces the emission of sulphur and fine particulate matter, but leads to an unanticipated increase in the direct emission of ultrafine particles (1-10 nm median diameter) which are highly effective precursors of cloud condensation nuclei (CCN). These additional ultrafine particles might probably modify cloud microphysics, as well as precipitation intensity and distribution on a regional scale downwind of emission sources. See Junkermann et al, 2011, The climate penalty of clean fossil fuel combustion. Atm Chem Phys 11, 12917-12924 [European Union]	Rejected. The focus here is on aerosol-hydrology influences, and not on how technologies affect aerosol number (which is still very uncertain). The discussion would be more appropriate in Ch7 or in WGIII.
8-946	8	47	28			This statement attributing changes in clouds to aerosols is made without any likelihood qualifier. And isn't this just a model prediction, not an affect which has been demonstrated in the observations? [Nathan Gillett, Canada]	Accepted. This has been restated so that it no longer reads as an attribution statement, and an "evidence" qualifier has been added, as well as additional cross-references to other chapters.
8-947	8	47	35			The Wang et al., 2009 reference is cited twice. Should something else should be cited here instead of the repeated reference to the same study? [Government of United States of America]	Noted, the 2[nd] reference has been removed
8-948	8	47	40	47	44	The authors should consider clarifying this sentence - as written, it is hard to read. Is it saying that this long list of other factors "increased greenhouse gases, cooling stratospheric temperatures, strengthening the polar vortex and shifting the westerly jet poleward" has a similar impact on surface temp; but maybe of opposite sign to ozone loss? Not totally sure. Please clarify [Government of United States of America]	Accepted, this sentence has been rewritten – ozone depletion and increased GHG's affect stratospheric temperatures similarly, with similar dynamic tropospheric response.
8-949	8	47	40		41	It is not the case that ozone depletion affects SH surface temperatures similarly to increased GHGs. GHGs drive broad scale warming, whereas ozone depletion causes a small surface cooling at high lats. [Nathan Gillett, Canada]	Accepted, this sentence was worded poorly and has been rewritten, see also response to comment 948
8-950	8	47	44		46	This is a model result but is written as though it has been demonstrated in obs. [Nathan Gillett, Canada]	Accepted, this has been clarified
8-951	8	47	48			There are no detection and attribution studies which find a detectable influence of land cover change on observed climate.Matthews et al. (2004) attempted to do this, but could not detect the effect of land cover change. This is a modelling result only. H. D. Matthews, A. J. Weaver, K. J. Meissner, N. P. Gillett, M. Eby, Natural and anthropogenic climate change: incorporating historical land cover change, vegetation dynamics and the global carbon cycle, Clim. Dyn., 22, 461-479, 2004. [Nathan Gillett, Canada]	Accepted, the reference and result have been added, and significance clarified.
8-952	8	47	56	47	56	Not "CO2 effects vegetation forcing…" but CO2 affects vegetation forcing [Robert Kandel, France]	Accepted.
8-953	8	47	56	48	9	This is an interesting discussion of how CO2 increases can result in decreased evapotranspiration from plants due to stomatal closures resulting in drying and warming. Is this effect fairly localised or widespread? Is there any observational evidence of this to date or is there a time in the future when this is expected to be non-negligible? Where else is this discussed within WG1? [European Union]	Accepted. We specify that the effect is local, refer to a section in Ch 11 that include references on the phenomenon. The discussion already mentions regions where it has greatest effect. As stated in the text, the uncertainties are large, so difficult to estimate the long-term impacts at this time.
8-954	8	47	57	47	57	transpiration not evapotranspiration [European Union]	Accepted, change is made.
8-955	8	48	1	48	1	Joshi and Gregory (GRL, 2008) modelled this effect [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted, this reference has been added.
8-956	8	48	23	48	23	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	Rejected. The other references discuss this particular topic more thoroughly, and space is limited for adding more.

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						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]	
8-957	8	48	25	48	27	Better to specify: Solar spectral (UV) irradiance variations … and on line 27 Such solar forcing [Robert Kandel, France]	Accepted
8-958	8	48	25	48	34	This section is interesting but difficult to follow. Can you go into slightly more detail - how do variations in solar irradiance induce ozone responses? What sort of solar irradiance variations are sufficient to do this - sun spot cycles or larger scale cycles? [European Union]	Accepted. We shall explain it better.
8-959	8	48	27		29	My recently submitted study on this topic found no significant solar response in the NAM and a positive solar response in the SAM on average in 37 CMIP5 models. N. P. Gillett and J. C. Fyfe. Annular mode changes in the CMIP5 simulations, Geophs. Res. Lett., submitted, 2012. [Nathan Gillett, Canada]	Taken into account: We will check your paper and change the text accordingly.
8-960	8	48	28	48	28	It should be cited the study of the solar influence on the Northern Annular Mode by: de la Torre et al. (2006) Solar influence on Northern Annular Mode spatial structure and QBO modulation, Adv. Space Res., 37, 1635-1639. doi: 10.1016/j.asr.2005.05.018 [Juan Antonio Añel Cabanelas, United Kingdom]	Taken into account: We will add this paper.
8-961	8	48	34			Refers to section 8.3.1.6 which does not exist (this is not a quibble about typos - it means I can't cross-check to what you are referring in this important statement.) [Joanna Haigh, United Kingdom]	Taken into account: The numbering of the sections has changed. The correct sections are 8.4.1.3.1 and 8.4.1.4. We will change the tecxt accordingly.
8-962	8	48	45		47	There is no significant NAM increase following volcanic eruptions in the CMIP5 models (Gillett and Fyfe, 2012; Driscoll et al., 2012). Given the large number of models involved and their increased sophistication, in my view this calls into question previous results on this topic, although I imagine this is a controversial point. N. P. Gillett and J. C. Fyfe. Annular mode changes in the CMIP5 simulations, Geophs. Res. Lett., submitted, 2012. Driscoll, S., Bozzo, A., Gray, L. J., Robock, A., & Stenchikov, G. (2012). Coupled model intercomparison project 5 (CMIP5) simulations of climate following volcanic eruptions. Journal of Geophysical Research, 117(D17), D17105. [Nathan Gillett, Canada]	Noted. We have added the qualifier "may" to the sentence. Simulations that isolate volcanos from other forcings may be required for signal.
8-963	8	49	1	50	22	Suggest future forcing to modeling chapter [Stephen E Schwartz, United States of America]	Rejected. It is difficult to move the discussion on geophysical disributions of radiative forcings to Chapter 12 because dispersion of discussion in Chpate 12 should be avoided and because the geophysical distributions from preinustrial to the present are also shown in this chapter.
8-964	8	49	1			In my view this section was less well written compared to the rest of the chapter. It deserves extra attention when the TOD is prepared. [Nathan Gillett, Canada]	Taken into account. Section has been revised and improved.
8-965	8	49	3	49	7	Are ozone increases seen over polluted regions, particularly in winter months, as a result of less NOx, less ozone titration? Analysis of observed trends (see chapter 2) suggests this might already be occuring? [Katharine Law, France]	Noted. This paragraph states general future projection.
8-966	8	49	3		12	This paragraph on Air Quality should be removed or reconciled with that in Chapter 11 (they are close). It belongs here if it deals with pollutant emissions driving the future RF, so emphasize that instead of AQ. Also 'air pollution' (line 5) is too vague as the huge CH4 increases lead to global scale surface O3 increases (see Chapter 11). [Michael Prather, United States of America]	Accepted. Basically viewpoint of Air Quality will be weakend in this section.
8-967	8	49	4		7	Doesn't tropospheric ozone increase to 2100 in RCP 8.5? Figure 8.4 seems to show this. [Nathan Gillett, Canada]	Accepted. This sentence will be revised. As shown in Figure 8.2, CH4 emission is predicted to increase only in RCP8.5.
8-968	8	49	7	49	10	"On the other hand, the emission scenarios for TAR and AR4, which was based on Special Report on Emissions Scenarios (SRES), had less optimistic future projections for air pollution, at least for some	Taken into account. This sentence has been revised as follows: "The RCPs therefore contrast with the

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						scenarios and current policies do not lead to the low emissions shown in the RCPs (Pozzer et al., 2012)." Please explain why all the RCPs are anticipating low emissions of air pollution, when current policies are not the reason. [Government of NORWAY]	emission scenarios for TAR and AR4, which were based on Special Report on Emissions Scenarios (SRES) and have future projections of larger increase in the near-term climate forcers (NTCFs). It has been questioned whether such low emission of NTCF is possible in the future given the current policies (Pozzer et al., 2012)." The primary reason for the difference between SRES and RCPs is what the policies are not included in SRES.
8-969	8	49	21		29	The text implies that Figure 8.2 shows regional emissions, but it doesn't. [Nathan Gillett, Canada]	Accepted. This is given in Supplementary Material Figure S8.1 in the latest draft.
8-970	8	49	23			Given the present natural emissions of soil-dust, SO2 concentrations will not raech very high in India as the SO2 is significantly scavenged by CaCO3 rich soil-dust (Kulshrestha et al., 2012). Ref: Kulshrestha U. Acid Rain. Encyclopedia of Environmental Management, Taylor and Francis (in press). [Umesh Kulshrestha, India]	Rejected. This sentence just describes the trend of SO2 emission.
8-971	8	49	33	49	33	Ozone precursors are also affected by climate change - there is very little discussion about this topic (is it in another chapter?) - changes in lightning NOx, soil NOx, biogenic VOCs etc. [Katharine Law, France]	Taken into account. There are several other sections to take care of this incluidng chapter 7.
8-972	8	49	36	49	36	"results" should be "result" [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Noted. This sentence is deleted in the latest draft.
8-973	8	49	37	49	37	"with acting as cloud"- I think there's a word or phrase missing? [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Noted. This sentence is deleted in the latest draft.
8-974	8	49	38		39	The text says that SOA may 'greatly increase' without any discussion of how large or how important this increase is likely to be, and whether it is accounted for in model simulations. [Nathan Gillett, Canada]	Accepted. This sentence has been revised as follows: "The spatial pattern of the aerosol forcing may be influenced by natural aerosols due to … SOA from vegetation changes (Tsigaridis and Kanakidou, 2007)."
8-975	8	49	42	49	42	I think that the 'maximum latitude of air pollution' is a slightly misleading phrase implying that there is a latitude beyond which no air pollution occurs. Reordering to 'latitude of maximum air pollution' may help. [Kate Willett, United Kingdom]	Accepted. It will be revised in the Final Draft.
8-976	8	49	42			Replace "air pollution" with "emissions of short-lived pollutants." otherwise discussed in 11. Rest of paragraph fits here. [Michael Prather, United States of America]	Accepted. It will be revised in the Final Draft.
8-977	8	50	1		4	The text here says that GHG-induced stratospheric cooling will enhance ozone recovery in the SH high latitudes. This is odds with the WMO climate assessment which showed a negligible impact of climate change on SH high latitude ozone (a slight decrease in ozone due to climate change) (WMO, 2007, Fig 3-10). The dominant climate effect on ozone in the lower stratosphere is through circulation changes. It is only in the upper stratosphere where the GHG-induced cooling is more important. [Nathan Gillett, Canada]	Accepted. This opinion will be incuded based on the WMO report in the Final Draft.
8-978	8	50	11	50	15	Something must be said about the robustness of the AF simulations. These are of very low confidence, aren't they? The geographical distribution also looks quite noisy which to me is another indicator of the uncertain nature of these simulations. [Lazaros Oreopoulos, United States of America]	Taken into account. The confidence level will be described based on Table 8.6 in the Final Draft.
8-979	8	50	14	50	15	This sentence is not clear as written; the authors should consider revising to read more clearly. [Government of United States of America]	Accepted. This sentence will be revised to clarify in the Final Draft.
8-980	8	50	14		15	This sentence makes no sense. [Nathan Gillett, Canada]	Accepted. This sentence will be revised to clarify in the Final Draft.
8-981	8	50	24	62	37	I limit my commeting to issues related to metrics. Overalll I think the metrics section is good and has improved since the first order draft. I only have some minor comments. [Daniel Johansson, Sweden]	Noted. Thanks.

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8-982	8	50	24	62	46	The authors have done a great job in summarising current knowledge from the physical science perspective on metrics. My comments are intending to further improve a section that I consider to be already in pretty good shape. My only substantial concern is the way that the issue of value-based choices gets mixed into the notion of structural uncertainties. Specific comments will clarify this concern and propose a solution. [Andy Reisinger, New Zealand]	Taken into account. Thank you. Regarding the treatment of value-based choices: this has been re-written and a different terminology is used.
8-983	8	50	24			Chapter 8 discuss different forcing definitions and put particular emphasis on adjusted forcing (AF). The section on emission metrics (8.7) does not mention how adjusted forcing could be used for metrics. Although AF has not so far been used to derive emission metrics (to my knowledge) a short discussion of how the use of AF would affect emission metrics would be useful. [Terje Berntsen, Norway]	Taken into account. Mentioned in Supplementary Material as a possiblity.
8-984	8	50	24			Section 8.7: In several parts of the section on common metrics the text is unbalanced and seems to focus on describing all the negative aspects of the GWP as a common metric, whereas the description on the alternative common metrics (mostly GTP) mainly focusses on the positive aspects and generally ignores the weaknesses and uncertainties associated with the alternatives. The whole section should be revised so that it presents a more balanced overview of the subject of common metrics. [Government of Denmark]	Taken into account. In SOD, much attention was given to GWP since 1) this is the adopted metric in Kyoto etc 2) and most of the literature is on this metric. 3) IPCC has always updated GWP 4) and GTP is not adopted in any legal contexts. Balance is improved by changing wording. And more attention is given to limitations of GTPs and alternatives.
8-985	8	50	24			Suggest chapter restrict itself to forcing, not get into metrics. Concern elaborated below. But basically this is a forcing chapter. Not a response chapter. [Stephen E Schwartz, United States of America]	Rejected. This is given by scoping.
8-986	8	50	24			Section 8.4: a summary table of the different emission metrics existing in the literature may help to clarify and shorten the discussion in this section. Such a table could have columns like time horizon, discount rate, point on the emission/forcing/temperature/impact chain, reference paper, etc. The WG3 AR5 draft is doing something very similar (Table 3-3) so there may be value in cross-checking their work (and even having the same table in both WG reports wouldn't do any harm). [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Rejected. A good suggestion which we have considered seriously at earlier stages and found that this will be difficult and may lead to problems related to how the readers percieve our assessment of metrics.
8-987	8	50	24			Section 8.7: I think it is a good job overall nicely encompassing a breath of issues. But there are still many details that need to be refined, in my view. [Katsumasa Tanaka, Switzerland]	Noted.
8-988	8	50	26			Section 8.7.1: The wording of the section has to be reworked. Currently, in many places, the language seems either non-scientific, policy-prescriptive or strongly twisted. Specifically, the wording blatantly obviously indicates that the authors wanted to stress insufficiencies, limitations and biases of the GWP concept, and to portray other metrics as "promising" or similar. The underlying literature however seems not to support such a biased representation against GWPs, when taking into account that many other metrics simply did not have the standing yet to warrant scientific publications that are focussed on their shortcomings. [Government of Germany]	Taken into account. We have improved the presentation of the limitations/shortcomings and carefully consider the balance. We base our assessment on the literature of published papers.
8-989	8	50	28			Section 8.7.1.1: I think that this section needs to point out why this IPCC report is largely devoted to the GWP (Section 8.7.1.2) and the GTP (Section 8.7.1.3) as this section proceeds those for the GWP and the GTP. The current manuscript does discuss multiple choices at different levels in the design of emission metrics, but rationales behind the focus on the GWP and the GTP are not given. [Katsumasa Tanaka, Switzerland]	Taken into account.
8-990	8	50	30	50	37	This definition of metrics is not consistent with the one in the Glossary, please harmonize. [Government of Germany]	Taken into account. The comment is passed on to the Glossary writing team.
8-991	8	50	30	58	10	Section 8.7.1 Metrics are mentioned very briefly in the SPM (pg11 lns 25-38) but no specific metrics are listed. The key metrics discussed in chapter 8 (e.g. GTP and GWP) should at least be mentioned by name in the SPM metric section. [European Union]	Noted. Comment passed on to one of the SPM authors from chapter 8.
8-992	8	50	30		37	This paragraph could be improved to introduce non-specialists to metrics. Put ln 34-37 at the the start of the paragraph. [Nathan Gillett, Canada]	Taken into account. Presentation is improved but lines 34-37 are not moved to the start as we dont think that will help.
8-993	8	50	34	50	34	It does not have to be complex models. A range of papers (Azar and Johansson, 2012, Climatic Change; Reisinger et al, 2010, Geophysical Research Letters; Tanaka et al, 2009, Climatic Change) use simple models	Rejected. We are here talking about INPUT to the metrics.

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						to derive metric values. [Katsumasa Tanaka, Switzerland]	
8-994	8	50	40	50	40	Replace "used" by "constructed" or "defined" in the caption of figure 8.26. "Used" assumes the metrics have already been defined. The diagram illustrates choices that can be made in their definition rather than how a specifc metric could be used. [Peter Van Velthoven, Netherlands]	Accepted. "defined" used.
8-995	8	50	46	50	46	Delete "somewhat misleadingly". The following sentences do clearly highlight the limitation of any metric, i.e. That there is no complete equivalence, so please refrain from non-factual language. [Government of Germany]	Accepted as suggested.
8-996	8	50	46	50	46	What exactly does "misleading" imply? [Katsumasa Tanaka, Switzerland]	Accepted: "somewhat misleadingly" is deleted.
8-997	8	50	47	50	48	The first half-sentence is difficult to understand, please improve. [Government of Germany]	Taken into account.
8-998	8	50	47	50	48	This part of the sentence (Ideally, the climate effects should be the same regardless of composition of the equivalent CO2 emissions) is misleading and should be removed. [Katsumasa Tanaka, Switzerland]	Rejected.
8-999	8	50	50			add "especially over extended time periods" after "regard to other effects", as the differing lifetimes and time horizons are the most important source of differences between metrics, and so this should be highlighted here. [Andy Reisinger, New Zealand]	Accepted.
8-1000	8	50	53	50	58	Life Cycle Assessments are brought up above (Line 37) as an example of metric applications. How does the argument here apply to the use of metrics in Life Cycle Assessments? [Katsumasa Tanaka, Switzerland]	Rejected. We think the words "...depend on which aspects of climate chnage that are most important to a particular application..." in the existing text answers the question raised.
8-1001	8	50	53	51	25	It would seem appropriate to mention that some metric choices benefit some nations in emissions reduction negotiations compared to others within this section of text. [European Union]	Rejected. We restrict our assessment to the scientific aspects and will refrain from discussing policy aspects such as this. The reader may also infer the consequences for calculation of CO2 equivalents based on different choices,
8-1002	8	50				Section 8.7: In the context of the whole report, the primary role of chapter 8 is to quantify the values of changes in forcing (present day compared to pre-industrial), the associated uncertainties, and to compare against values from previous reports. The authors did a very good job at this. It is unclear why chapter 8 is stuck with the Emission Metric section. This section contains a lot of useful information, but is not user friendly. It coudl be shortened by half and reorganized to mroe clearly flow, by using this order: Section 8.7.1: Introduction and concept Section 8.7.2: Survey of values for various metrics (current 8.7.2.1 and 8.7.2.2) Section 8.7.3: Examples of possible applications (the rest of the old section 8.7.2). The authors did not provide an assessment of what values should be adopted. In the discussion, it should be made very clear that figures 8.31 - 8.34 are illustrative examples of how (the process) the information can be organized and used, and the values are not meant to be used in actual policy decisions. Thus, it is appropriate that none of the numerical values are mentioned in the chapter summary. [Government of United States of America]	Noted. Inclusion of Emission metrics in chapter 8 was given by the scoping of AR5 WG1. We do not indicate that values should be used in policy decisions. What we do here is a scientific assessment of metric concepts and results in the literature. We have improved the presentation and hope the chapter hae become more user friendly.
8-1003	8	51	4	51	25	Box 8.2 now has a useful and concise focus by listing the different choices that need to be made for a metric. I propose to make it a bit easier to digest though by further separating out the choices; at present, the type of impact, and whether its an integrated or end-point metric, are mixed in one paragraph, which can be confusing. I suggest that the box have separate paragraphs that detail each of the following choices separately: time frames; time horizon (simply, do you look 20 or 500 years ahead, or infinitely?); impact or climate property of concern (RF, delta T, SLR etc, and is it rate of change or absolute value); integrated or	Accepted.

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						end-point; and spatial dimension. Much of the exising text can be used for this, it's just a case of giving it an even clearer structure. [Andy Reisinger, New Zealand]	
8-1004	8	51	6	51	6	Define " backward looking", or delete it. The backward looking perspective is not discussed elsewhere in the section so there is really no point bringing it up here. [Daniel Johansson, Sweden]	Taken into account.
8-1005	8	51	6	51	8	The explanation does not seem to address time frames, but model types. From the remaining text, no backward-looking concepts appear, or the difference between backward or forward looking does not appear again as important while different model types are required changes that are not mentioned. [Government of Germany]	Taken into account. Text clarified.
8-1006	8	51	6			Explain the difference between these perspectives. [Nathan Gillett, Canada]	Taken into account. Text clarified.
8-1007	8	51	10	51	17	It remains very unclear what 'end-point' should be in this para and to what it relates. Instead of providing appropriate explanations, the text quotes some examples which is not very helpful. The start should be 'types of impact'. Only the first two sentences deal with types of impacts, the remaining para with other changes. Those should be addressed in separate subparas. E.g., the sentence starting in line 13 would fit to the heading 'time frames' (better than the first para of this box). [Government of Germany]	Taken into account. Text clarified.
8-1008	8	51	10	51	17	Move the second sentence "This choice..Article 2)" in line 11-13 to either line 10 after "end point:" or to the end of line 17. Presently, it seems to refer only to the choice between RF. Delta-T or sea level change, while the second part of this paragraph also lists items that also constitute such alternative choices, e.g. "rate of change". [Peter Van Velthoven, Netherlands]	Taken into account.
8-1009	8	51	10			"RF, ΔT" – Unnecessary use of acronyms (i.e. 'radiative forcing' and 'temperature change' are easily read English) [Government of New Zealand]	Taken into account as suggested
8-1010	8	51	11	51	11	Remove "also" [Terje Berntsen, Norway]	Taken into account as suggested
8-1011	8	51	11	51	12	"Dangerous anthropogenic interference with the climate system" is addressed by climate policies (not directly by emission metrics). [Katsumasa Tanaka, Switzerland]	Noted. Unclear comment. But we think choice of metric is related to DAI as addressed by climate policies.
8-1012	8	51	12	51	13	With these two sentences following each other it sounds as if level (e.g., degrees Celsius) or rate of change are socioeconomic damages. I think changing the order of these two sentences will fix the problem. [Terje Berntsen, Norway]	Taken into account
8-1013	8	51	15	51	17	How the impact is calculated is a different question and does not have to be discussed in this box on metrics. [Katsumasa Tanaka, Switzerland]	Taken into account. Text changed.
8-1014	8	51	19	51	20	It remains unclear how the explanation is related to metrics. Please clarify why this is a relevant choice required for am metric and what the choice really is. [Government of Germany]	Taken into account. Text clarified.
8-1015	8	51	19	51	21	Rephrase this section: In a box that says "choices required when using emission metrics" this paragraph here assumes that emission metrics can be sensibly applied to emissions of short-lived climate forcers that have a spatially and seasonally very heterogenic response. No previous IPCC report even provided quantitative values for GWPs for short-lived climate forcers such as black carbon, so this section here implicitly makes the value judgement that it might be sensible to do so and only the "spatial dimension for emissions and response" is a choice, not the possibility per se. Thus, please rephrase by a) making clear that if one were to extent the metric concept to short lived climate forcers, this would be going beyond the traditional application of metrics given that the effect of the emissions depends on the season, location and even time of day (aviation induced cirrus, for example) of the emissions. Metrics are first of developed for things that can to some degree be compared; whether short-lived climate forcers can be compared to well-mixed GHGs, the domain so far for metrics (see IPCC AR4) is an open question. b) Beyond this fundamental question, whether metrics can be meaningfully extended to short-lived climate forcers, highlight not only the "spatial dimension" in vague terms but all the dimensions that would be leading to rather heterogeneous effects which would have to be considered in case that "equivalent" metrics would want to be applied to short-lived climate forcers with atmospheric residence times below a year, i.e. the location, season, weather conditions, co-emissions.	Rejected. This comment reflect that metrics are being used in several contexts; i.e. in the scientific literature for comparing emissions and effects, in policy assessments, emission trading and in international agreements (as discussed at page 50, line 34-37). Metrics for SLCF are already used extensively in the scientifc literature, and here we do an assessment of these metrics and estimates. Such an assessment does not imply any value judgement or recommendation for inclusion in climate policies. It is also worth noting that AR4 presented metrics for SLCFs: fig 2.2. shows integrated RF for long and shortlived components which is the same as AGWPs. The GWP table 2.14 there also contains some gases with short lifetimes. Finally, AR4 WG1 section 2.10.3.

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						[Government of Germany]	presents GWPs for components with indirect effects (CO, NOx, VOC and H2) and table 2.15 gives an overview of published estimates. This is a part of a scientific assessment and a similar assessment was presented in AR5 SOD.
8-1016	8	51	23	51	25	The sentence in line 23-34 implies that there is a choice of model, which is not mentioned (see comment to first para). Delete the last sentence from line 24-25: none of the choices can be based on science alone, they all involve judgements. [Government of Germany]	Noted. We agree that choices of time horizon and climate impact cannot be based on science alone, and this is what we have written.
8-1017	8	51	23	51	25	Here I am admittedly supporting on my own work, but Tanaka et al. (2010, Carbon Management) is a paper that fits in this argument involving choices and value judgment and can be cited here. But the paper is officially not peer-reviewed and may thus be not allowed to be cited in the IPCC report, while the paper has been cited 11 times (Google Scholar) and I believe that the argument of the paper stays at a concervative side. [Katsumasa Tanaka, Switzerland]	Noted. As you say, this paper is not peer reviewed. In addition we avoid references in this box.
8-1018	8	51	29	51	29	It would be better to start this section with a heading 'types of metrics' followed by an explanation of all types, instead of an explanation of GWP and GTP, followed by an uncertainty discussion and than followed by other types of metrics. [Government of Germany]	Rejected. We understand that the reviewer see other possible structures here, but we keep the current structure since the suggestion structure would require more space and since we think 8.7.1.1 and the Box do the job.
8-1019	8	51	29	52	27	The expansion of the subsection on GWP concept is helpful. It at least addresses some of the issues over change in GWP of a substance with time being due entirely to CO2 impulse profile. I would continue to urge presentation of tables and figures of AGWP's of various substances vs time, in addn to any presentation of tables of GWP. [Stephen E Schwartz, United States of America]	Noted. Thank you.
8-1020	8	51	29			GWP concept. I urge now, as I have done for several previous 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 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 Well Known . . . to a Few People: Attribution of Excess Atmospheric CO2 and Resulting Global Temperature Change to Fossil Fuel and Land Use Change Emissions. Schwartz, S. E. 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. Or at least AGWP's be presented in parallel in all instances. I am thus pleased to see the table of AGWP's in Appendix. 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, United States of America]	Noted. As suggested during review of FOD we focus more on AGWP and explain why GWP depends on CO2.
8-1021	8	51	29			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 Lowe1, C Huntingford2, S C B Raper3, C D Jones4, S K Liddicoat4 and L K Gohar1 1 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 HARE1 and MALTE MEINSHAUSEN2,3 HOW MUCH WARMING ARE WE COMMITTED TO AND HOW	Noted.

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						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. WEAVERLifetime of Anthropogenic Climate Change: Millennial Time Scales of Potential CO2 and Surface Temperature Perturbations VOLUME 22 JOURNAL OF CLIMATE 15 MAY 2009 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. Allen1, David J. Frame1,2, Chris Huntingford3, Chris D. Jones4, Jason A. Lowe5, Malte Meinshausen6 & Nicolai Meinshausen7 Warming caused by cumulative carbon emissions towards the trillionth tonne Nature Vol 45830 April 2009 doi:10.1038/nature0801 [Stephen E Schwartz, United States of America]	
8-1022	8	51	31	51	41	The text presents two quotes from the First IPCC Assessment Report. That seems to only serve as a critique of GWP. However, the first quote seems irrelevant, since the First IPCC Assessment Report the IPCC has published three further Assessment Reports, all still considering GWP as the best option for a common metric. The last part of the quote "…A simple approach has been adopted…", is presented but no reflexion is made about the strengths of having a simple approach rather than some of the very complex alternatives that is described later in the chapter. The second quote focusses on the time horizon used. However, the quote merely states the obvious and the reason for including is difficult to understand, if not only again serving to criticise GWP. It should be considered what value these quotes bring to the text, and if kept more discussion should be included in line with the comments made above. [Government of Denmark]	Taken into account. More discussion is included and more attaention is given to balanced focus. The quotes were inserted to remind readers and users about how this metric was introduced as a tentative approach more than 20 years ago. We agree that the simplicity is an important factor to consider but in this context (i.e. scientific assessment) we find it necessary to focus on the scientific aspects of the metric. The weighting of scientific performance vs performance in a policy context is important and needed but this is not possible to do in this context. Regarding time horizon; we find it necessary to remind the users about the basis for the 3 common horizons and the traditional choice of 100 years. Many applications adopt CO2-equivalents based on 100 years without giving attention to this choice and its impacts on the results.
8-1023	8	51	31	51	41	This para provides somewhat negative information on the history of the GWP. A comparable type of information must be provided for the GTP. In the current version, the text is biased. [Government of Germany]	Taken into account. Wording is modified and similar attention is given to the limitations of GTP. The reason for the focus on GWP is its massive application in various assessments and analyses and the risk of implicit choices.
8-1024	8	51	31	52	24	Section 8.7.1.2 There is no definition for AGWP anywhere in Ch 8 before it appears in Figure 8.28 as AGWP. This section is a little hard to follow for the non-expert, especially as the reader has no idea what AGWP is meant to be. The description of GTP pg 52 ln 31-40 is better. [European Union]	taken into account. We have improved the text within the frames given. It may be argued that since this is a well know concept presented in previous IPCC reports it may not be needed to repeat all this here.The first sentence in the section is a short definition/explanation.
8-1025	8	51	32	51	32	The definition of the GWP is not restricted to gases. [Terje Berntsen, Norway]	Accepted. Changed "gas" to "component"
8-1026	8	51	33	51	35	I think it better to quote this later in this section. I would imagine it is confusing for those who try to understand what the GWP is for the first time. [Katsumasa Tanaka, Switzerland]	Rejected. Since this is a well know concept presented in previous IPCC reports it may not be needed to repeat all this here.
8-1027	8	51	39	51	39	"usually" is used in two consecutive sentences. [Katsumasa Tanaka, Switzerland]	taken into account. Text changed.

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8-1028	8	51	39	51	41	The quotation of Houghton does not explain the selection of different time horizons in a good way, delete the reference and continue after 500 years with 'representing different choices of time frames in an exemplary way'. [Government of Germany]	Rejected. It is not meant to explain it. Just to show how arbitrary these choices were.
8-1029	8	51	39			“It is usually integrated over 20, 100 or 500 years though according to Houghton et al. (1990) these time horizons were ‘presented as candidates for discussion and should not be considered as having any special significance”. Use of word ‘according to’ seems inappropriate/confusing. This is where these numbers come from and that is what the article said. Previous authors had used different time scales. Suggest something along the lines of: “It is usually integrated over 20, 100 or 500 years consistent with Houghton et al. (1990)., note however that Houghton et al (1990). presented these time horizons as ‘candidates for discussion [that] should not be considered as having any special significance”. [Government of New Zealand]	Taken into account.
8-1030	8	51	43	51	47	In the paragraph, it is stated that the choice of time horizon has a large impact on the GWP values and the usual choice of 100 years is arbitrary without any scientific backing. While this is all correct, it seems very biased that this is raised in the chapter on GWP, since exactly the same issues are relevant for GTP. Either this issue should also be included in the GTP section or the issue of choosing a time horizon should be addressed in a separate section. [Government of Denmark]	Taken into account. Text changed.
8-1031	8	51	43	51	47	Delete this paragraph. Repetition from the box and does not add specific information on GWP. There is no similar discussion on time frames for the other metrics but the message would be the same. Here it is presented in a way as if the choice of time frame only matters for GWP. [Government of Germany]	Taken into account by stressing that this is also the case for GTP.
8-1032	8	51	46	51	47	See my comment for Chapter 8, Page 51, Line 23-25. [Katsumasa Tanaka, Switzerland]	Noted. As you say, this paper is not peer reviewed.
8-1033	8	51	49	51	49	The fact that the AGWP for short-lived components do not increase with longer time-horizon and thus the change in the GWP reflects the increase in the AGWP for CO2 is also a property of the gas (e.g. methane) itself. [Terje Berntsen, Norway]	Rejected. We write "mainly" so we think this should be ok.
8-1034	8	51	49	51	50	This is shown in Figure 8.28, but are there also papers that support this statement? [Katsumasa Tanaka, Switzerland]	Noted. This is according to the simple formulae and no supporting papers are needed for this obvious observation.
8-1035	8	51	49	51	54	The same type of information must be provided for the GTP. How about GTP for short and long-lived species after 5 decades and for longer periods? Please be balanced. [Government of Germany]	Rejected. We don't have space for that. And the issue is different for GTP, which may also be seen from fig. 8.27.
8-1036	8	51	52	51	52	"Practically" or "virtually" rather than "purely"? [Katsumasa Tanaka, Switzerland]	Taken into account. "Purely" has been deleted.
8-1037	8	51				Section 8.7.1.2: The authors should consider adding a short paragraph in this section discussing the units of GWP, and that these units are optimized for emissions comparisons rather than biogeochemical feedbacks. An issue is that, for terrestrial CO2 vs. CH4 responses to climate change where a comparison is frequently made between the GWP of a kg of soil C that can become either CO2 or CH4 based on the decomposition pathway, the ratio of the warming responses is not the GWP, but the GWP multiplied by the ratio of the gas molar masses (i.e. 9.1 instead of 25, for the 100-year time horizon). This mistake is commonly made by both scientists and media and it would be helpful to spell out explicitly here that the units are not stoichiometric and so a conversion needs to be made if making a stoichiometric comparison. [Government of United States of America]	Rejected. We think it is clear as formulated.
8-1038	8	52	1	52	1	"Pulse emissions of relevant agents" rather than just "pulses". [Katsumasa Tanaka, Switzerland]	taken into account. Have inserted "emission".
8-1039	8	52	1	53	53	The authors should consider moving the figures to where they are discussed in the text. In other words, the figures on these pages seem out of order. Both are introduced before the concept they illustrate is discussed in the text. Fig. 8.27 should appear after section 8.7.1.3 has at least begun. Fig. 8.28 should appear after AGWP is discussed. This appears to be in Section 8.7.1.4, although it's difficult to find a definition of AGWP. The first mention of AGWP appears to be on page 53, line 20. But it is not explained/defined there. [Government of United States of America]	Taken into account. Figures are moved.
8-1040	8	52	3	52	3	"Artificial" or "synthetic" gases? [Katsumasa Tanaka, Switzerland]	Rejected. Nothing about "synthetic" or "articifical"

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							here. On page 62 we use "synthetic" for a group of gases, which is explained.
8-1041	8	52	6	52	6	The same type of information must be provided for the GTP. How about GTP for short and long-lived species after 5 decades and for longer periods? Please be balanced and use neutral language. [Government of Germany]	Rejected. Same comment as 8-1035. Not relevant to line 6 at page 52. See response to 8-1035.
8-1042	8	52	6	52	7	I don't think that the radiative forcing is "the total energy added to the climate system". [Katsumasa Tanaka, Switzerland]	Noted. We think it is fine as written.
8-1043	8	52	6	52	11	The paragraph correctly states that GWP is an indicator of radiative forcing over time. However, GWP is then criticised for exactly that, and it is stated that the equivalence is “weaker or non-existent with regard to many other relevant climate end-points”. There are no specifics regarding the “other relevant climate end-points” and also no quantification on the “weaker” equivalence. This should be addressed in far more specifics. [Government of Denmark]	Taken into account. Text is changed. We think it is important to explain what is equivalent and where there is not equivalence. This is not a critique, just a clarification.
8-1044	8	52	6			The GWP is a measure the integrated/averaged RF over time; "magnitude' is an odd description. [Michael Prather, United States of America]	Accepted. "magnitude" is deleted.
8-1045	8	52	7	52	8	Delete the sentences starting with even until 'other climatic end-points'. This has already been said in a better and less biased way in the first sentence. In the section on GTP, you don't elaborate on what the GTP concept does not include. If it is defined related to the magnitude of RF, it is clear why the relationship to other climate response parameters are weaker. [Government of Germany]	Taken into account. Similar remarks added for GTP.
8-1046	8	52	7	52	10	This sentence needs to be rephrased to be more neutral in language. A judgement is implied in this sentence as to what is weak or strong equivalence. [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account; text changed.
8-1047	8	52	7	52	11	The text states that "the only thing that is equivalent is the integrated RF over 100 years, and the equivalence is weaker or non-existent with regard to many other relevant climatic end-points." This statement is true, but it is equally true for every other climate metric, and hence should be stated in the general discussion of metrics, for example in the introductory section 8.7.1.1. As an example, if GTP50 would be used, the equivalence wouldn't hold for temperature changes any other than the 50 years. Making this kind of statement for GWP only is biased. [Tommi Ekholm, Finland]	Taken into account. This is a good point and this was already made clear in the last sentence of the 2nd para of the introduction.
8-1048	8	52	13	52	13	The line starts with the senetence "There haven been several attemts to interpret the GWP". I think it should be written something like "There have been several attemts to give other interpretions of the GWP". GWP has an interpretation that is stated in line 6-7 on the same page, i.e. the ratio of the "total energy added to the climate system" for one gas X to that of the reference gas given a specific time horizon). [Daniel Johansson, Sweden]	Taken into account.
8-1049	8	52	14	52	15	The finding "the GWP of a species converges to the ratio of the equilibrium temperature response due to a sustained emission of the species and CO2" (even though it might be found in the literature) must be wrong because efficacies modulate the radiative forcing to temperature relationship. Cite Hansen et al. 2005 on efficacies of climate forcings. [Government of Germany]	Taken into account.
8-1050	8	52	15	52	16	I suggest to change to: Since the temperature response at time t of a sustained emission is equivalent to the integrated temperature …………….. [Terje Berntsen, Norway]	Accepted as suggested
8-1051	8	52	15	52	18	I do not follow the logic here. Please clarify. [Katsumasa Tanaka, Switzerland]	Taken into account; text changed.
8-1052	8	52	15		17	I believe that this theorem was proven for general linear systems (including all impacts) also in Prather (2002, GRL, 29:2063) [Michael Prather, United States of America]	taken into account. Reference added.
8-1053	8	52	19			Actually, the theorem above hold for all emitted/forcing species no matter what the lifetime. It holds hold for NTCFs exactly if one carefully defines the steady-state lifetime and pattern (of all perturbations including climate), which changes with different location of emissions. This is stated later 8/54/52. [Michael Prather, United States of America]	Taken into account; text changed.
8-1054	8	52	21	51	24	The statement about 'relative cumulative forcing index' seems to be somewhat contradictory to the	Taken into account. A forward reference to this point

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						conclusions of Peters et al (2011a) and Azar and Johansson (2012) that the GWP approximates the iGTP. I think this should be stated here (not necessarily calling it the iGTP, but simply noting that even though the GWP as defined is fairly abstract (which the 'relievely cumulative forcing index' conveys well), it actually has a fairly close and more meaningful physical interpretation as 'integrated relative warming index'. This is stated later in the metrics discussion but I think it is more appropriate here. The end of that sentence could then provide a forward reference to section 8.7.1.3 which discusses GTPs in detail. Otherwise readers are left with an unjustified impression that the GWP is so far removed from issues of concern as to be useless, whereas I think Peters et al (2011a) and Azar and Johansson (2012) have shown the opposite. [Andy Reisinger, New Zealand]	is added, as suggested.
8-1055	8	52	21	52	21	The radiative forcing is a physical quantity, but what does "misperception of the physical impacts" mean? [Katsumasa Tanaka, Switzerland]	Taken into account; text changed.
8-1056	8	52	22	52	22	I agree that "Relative cumulative forcing index" would potentially be a better name that captures more precisely what the GWP is about. But does this suggest that we should be using the new name in the future? [Katsumasa Tanaka, Switzerland]	Noted. This was meant as an explanantion of what the GWP does.
8-1057	8	52	22	52	24	Is the GTP independent of the background atmosphere, etc.? Please provide the same detail of information on GTP and GWP. The current text lists a number of problems for the GWP and is silent about those of the GTP. [Government of Germany]	Taken into account. More on limitations of GTP is added. But please note that we already have pointed to the additional uncertainty in GTP (line 5, page 53)
8-1058	8	52	23	53	9	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, United States of America]	Rejected. Given cleary by scoping.
8-1059	8	52	32	52	32	Here it is mentioned that the GTP concept is related to 'a chosen point in time', but in the section it is not explained what this means for the use of the GTP concept for purposes to compare mitigation efforts in relation to a time integration as for GWP. [Government of Germany]	Taken into account. Explanation is added.
8-1060	8	52	36	52	37	The definition of AGTP is unclear, it reads 'AGTP is the absolute 'Global Temperature Change Potential' giving temperature change per unit emission. As this is key for understanding, an improved definition should be provided. [Government of Germany]	Taken into account. Presentation improved.
8-1061	8	52	40	52	40	Similar to the GWP concept, it should clearly be explained that the time horizon determined by the proximity to a target year, needs some value judgement of proximity to target year. [Government of Germany]	Taken into account. We have added an explanation: "as calulated by using scenarios and climate models".
8-1062	8	52	41			I would insert a sentence here that says: "A key difference between the GWP and GTP is that the GWP is an integrated metric, whereas the GTP is an end-point metric that compares warming only at one specific point in time." [Andy Reisinger, New Zealand]	We agree that this is important to make clear. This is already stated at line 3 and 4 in the section 8.7.1.3. In addition, this is the key message of figure 8.27.
8-1063	8	52	42	52	44	It should be better explained, that the AGTP seems to require a choice of emission scenario and it should also be explained whether the linear assumption is correct. [Government of Germany]	Taken into account. We have added more explanation of application of GTP; i.e. Emission x GTP(H) for a fixed time horizon (similar to the use of GWP) and an application with a changing time horizon for a given scenario. Regarding linearity: This is assumed for all applications of metrics and is not specific to this application.
8-1064	8	52	45			Isn't this equation only valid for infinitesimal perturbations? Doesn't it assume that the forcing is linear in the concentrations? [Nathan Gillett, Canada]	Taken into account. Due to space limitation we could not write much on this. We believe however that these points are made clear elsewhere in the text.
8-1065	8	52	51	52	51	This sentence needs to be revised. The climate sensitivity is used as if it were a physical process ("By accounting for the climate sensitivity and the exchange of heat between the atmosphere and the ocean"). But it is rather a measure of the performance of a climate model. I identified this problem in some other places in	rejected. This is a physical concept

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						this section. [Katsumasa Tanaka, Switzerland]	
8-1066	8	52	52	52	52	Suggest this is reworded to be more neutral to "…includes physical processes that the GWP does not." [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted as suggested.
8-1067	8	53	1	53	9	The paragraph states that the increased uncertainty of using GTP instead of GWP is a typical trade-off when “moving to an impact of greater relevance”. In addition to the strange formulation, no reflexions are made regarding the acceptable increase in uncertainty in connection with the perceived added benefits. This should be addressed in far more detail since it is not automatically preferable to accept far greater uncertainties if the only added benefit is that the model is far more complex. More and more complex models are not an objective in itself, if it means a significant increase in uncertainty. [Government of Denmark]	Rejected. A good question but the literature does not give us a basis for going more into this.
8-1068	8	53	1	53	9	Does this paragraph fit better into Section 8.7.1.4? [Katsumasa Tanaka, Switzerland]	Rejected. This is worth considering, but we kept it here in order to stress some of the limiations and shortcomings related to GTP together with the presentation of the concept; i.e. to make this more visible to the reader and improve the balance between treatment of GWP and GTP. We have also added references to the more detailed discussion below.
8-1069	8	53	2	53	2	The text states that "the GWPs for NTCF are higher due to the integrative nature of the metric." This statement is very generic, please indicate the role of timeframe towards this conclusion in order to avoid misinterpretations. The statement perhaps implicitly assumes that the same (possibly long, e.g. 100 yerar) timeframe is used both for GWP and GTP for this comparison. [Tommi Ekholm, Finland]	Taken into account. Text changed. Yes, it is implicitely assumed over the same timescales. We have added "over the same time frames"
8-1070	8	53	4	53	5	This is also shown by Reisinger et al. (2010, Geophysical Research Letters). [Katsumasa Tanaka, Switzerland]	Noted. (Added reference to more discussion on this in next section).
8-1071	8	53	6	53	7	I do not understand this sentence. [Katsumasa Tanaka, Switzerland]	Taken into account. Sentence changed.
8-1072	8	53	7	53	9	GWP values are also influenced by the background atmospheric composition etc. [Katsumasa Tanaka, Switzerland]	Taken into account. Text changed.
8-1073	8	53	7			Revise the beginning of the sentence to read "The GTP is influenced, as is the GWP, by the background...". Just to signal that the GTP is actually quite simillar in some respects to the GWP. [Andy Reisinger, New Zealand]	Accepted. We have changed to "as for GWP, the GTP is…."
8-1074	8	53	8	53	8	The climate feedbacks are included in the climate sensitivity that is discussed above. Does feedbacks in this sentence refer to chemical feedbacks? If yes, this should be explicitly stated. [Terje Berntsen, Norway]	rejected. Yes, climate feedback are included already in lambda, but what we have in mind here are climate-carbon feedbacks. This will be discussed in the following section.
8-1075	8	53	11	55	10	Uncertainties and limitations should be discussed for all types of metrics in a systematic and balanced way and it would be better to introduce all types of metrics (section new metric concepts) before. [Government of Germany]	Rejected. We agree that this would in principle be a more logical structure. However, we decided to use this structure given the space available and the instructions from scoping that we should focus on GWP and GTP.
8-1076	8	53	11	55	10	I have a major concern about the way in which this section lumps CHOICES in with structural uncertainties. I don't think it is helpful to mix those two. The notion of uncertainty only makes sense really if one assumes that there is a true value 'out there' and one has to discover it. But choices are different - there is no question of there being a true value for the relative weight of CH4 vs CO2. Hence I propose that this section is re-structured (without changing much of the detailed wording) to discuss (1) choices, (2) structural uncertainties, and (3) scientific uncertainties. The latter are pretty much as discussed. Structural uncertainties include treatment of carbon cycle feedbacks and temperature coupling, inclusion of other indirect effects, coupling of CH4 to aerosols (as per Shindell et al), choices of response functions, and perhaps even the choice of background atmosphere. But please keep choices separate, i.e. all the issues listed in Box 8.2. This would allow you to compare the relative influence that scientific and structural uncertainties and choices have, to help	Taken into account. Terminology changed here . The user related choices are separated out and we avoid calling these uncertainties.

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						signal where the biggest differences arise. The latter would be a policy relevant but currently missing conclusion. [Andy Reisinger, New Zealand]	
8-1077	8	53	13	53	16	I do not clearly understand the definitions of structural and scientific uncertainties discussed here. For example, the uncertainty in climate-carbon cycle feedbacks would fit into scientific uncertainties according to the definitions given here ("the range of values that can be calculated for a given metric due to incomplete knowledge of processes from emissions to climate change and impacts"). But climate-carbon cycle feedbacks are discussed later in the part for structural uncertainties (p.54, line 17-22). The sentences here are the same with p.7 of IPCC (2009) and requires a revision for clarifications. As a matter of fact, I faced a similar difficulty in classifying the type of uncertainties involving in the definitions and computations of metrics (see Figure 1 and its caption of Tanaka et al. (2010, Carbon Management)). In my case, the separation was between the uncertainties related to the representation of natural earth system and those related to the climate policy. A separation between scientific and policy uncertainties (rathern than scientific and structural uncertainties) might be a better way forward (it is discussed in this way in p.51, lines 23-25). It might be safer to abondan the classification of uncertainties at all in this report, or at least the difficulty in classifying the type of uncertainties could be acknowledged. [Katsumasa Tanaka, Switzerland]	Taken into account. We will change the terminology here and separate out the user related choices and avoid calling these uncertainties.
8-1078	8	53	14	53	14	The word "consequences" used to define the structural uncertainties is confusing to me. [Katsumasa Tanaka, Switzerland]	taken into account. Wording and terminology changed.
8-1079	8	53	20	53	20	AGWP appears for the first time and has not been defined before. [Government of Germany]	Taken into account. But the definition was given in fig 8.27 already.
8-1080	8	53	20	53	24	The CO2 impulse response function is the adjustment time. The wording makes it sound like the IRF is something different. The authors should consider revising the text accordingly. [Government of United States of America]	Rejected. We think the sentence "….impulse response function (IRF) that describes the development in atmospheric concentration that follows an emission pulse (Joos et al., 2012); see Box 6.2 and Supplementary Material). " gives a good explanation. And for CO2 there is not one adjustment time.
8-1081	8	53	21	53	22	The discussion here is limited to the IRF. The IRF is a way of representing the carbon cycle, but it can be represented by a range of other models. In my view, the limitation of the linear IRF approach is not properly acknowledged in some recent papers. For example, see my comment (Earth Syst. Dynam. Discuss., 3, C436–C441, 2012, www.earth-syst-dynam-discuss.net/3/C436/2012/). My comment on p.50, line 34 is also related. [Katsumasa Tanaka, Switzerland]	Rejected. We agree that IRF is one way of representing the C cycle and that limitations could be discussed. But unfortunately, we do not have space for that here.
8-1082	8	53	27	53	27	What does RE mean here? [European Union]	Taken into account. Explained now
8-1083	8	53	27	53	27	I do not think RE has been defined. Think is refers to radiative efficiency… [Daniel Johansson, Sweden]	Taken into account. Explained now
8-1084	8	53	27	53	27	"RE" has not(?) been defined. Radiative efficiency? [Räisänen Petri, Finland]	Taken into account. Explained now
8-1085	8	53	27	58	27	The abbreviation RE is used, without being defined. Does it mean "radiative effect"? This term appears to be spelled out in most of the document. Also appears on page 55, line 3, and on page 58, line 20 and line 23. The authors should consider not using the definition or, if it remains, spelling it out. [Government of United States of America]	Taken into account. Explained now
8-1086	8	53	33	53	33	See my comment for Chapter 8, Page 52, Line 51. [Katsumasa Tanaka, Switzerland]	Rejected. Same response as to your comment above.
8-1087	8	53	35	53	35	Do you mean Collins et al. 2012 instead of 2010? [Robert Portmann, United States of America]	Noted. We mean Collins et al. 2010 (JGR) were they use the formualtion from Boucher and Reddy. (Which is also used on Collins et al. 2012)
8-1088	8	53	36	53	36	Is this perhaps "enhance" rather than "increase"? [Katsumasa Tanaka, Switzerland]	Rejected. Enhance also has an element of "improved", so we think increased is better.
8-1089	8	53	44	53	46	I disagree. The impulse response function approach is not appropriate when one explores the uncertainty in GTP values. For example, when the climate sensitivity is varied from its reference value, the impulse response function would be scaled just linearly with the assumed climate sensitivity, a limitation of the linear approach.	Rejected. The IRF_dT does not scale linearly and since lambda also affects the time constants and thus also the temporal development of the response. Thus

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						See Figure 2.6 (p.82) of http://www.mpimet.mpg.de/fileadmin/publikationen/Reports/BzE_40.pdf Climate models are more appropriate tools to probe the uncertainty in climate sensitivity. [Katsumasa Tanaka, Switzerland]	the IRF_dT is key in exploring the uncertainties (in addition to IRF_CO2 and radiative efficiency).
8-1090	8	53	48			Is the time horizon really structural uncertainty or a policy choice? [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Terminology changed here . The user related choices are separated out and we avoid calling these uncertainties.
8-1091	8	53	52	53	52	what are 'some transport sectors'? please clarify. [Government of Germany]	Taken into account. Specified.
8-1092	8	54	1	54	1	Remove "usually". [Katsumasa Tanaka, Switzerland]	Rejected. We think "usually" is appropriate here.
8-1093	8	54	1	54	2	Please also discuss the additional uncertainty introduced with the choice of a discount rate. [Government of Germany]	Rejected. This is not an uncertainty as such, but is a value based choice that will have a large effect on the values. If space we may mention this, with a reference to discussions in WGIII.
8-1094	8	54	4	54	15	It is counter intuitive to treat indirect effects as a structural uncertainty. It may boil down to what one means by "atmospheric concentrations" in Figure 8.26. If one means atmospheric concentration of the emitted material, then whether one include the effects of the daughter products is a structural uncertainty. However, some of the feedbacks on the response time should be a counted as a scientific uncertainty. If one means atmospheric concentrations of all species in the atmosphere, then it should be all counted as scientific uncertainty. The authors should consider revising teh text to clarify this. [Government of United States of America]	Taken into account. Terminology here changed.
8-1095	8	54	4	54	15	This para is very useful, but to be an assessment, I feel that some conclusion is necessary as to how far the feedbacks can be considered based on current scientific knowledge. Drawing on the familiar example of the wing beat of a butterfly causing a hurricane, there has to be a limit as to what detail in the treatment of feedbacks one can plausibly expect from current science. At least add a concluding sentence that says that in a complex and interconnected system such as the atmosphere, feedbacks can become infinitely complex, and that generally, scientific uncertainty of the magnitude and even direction of feedback increases the further one departs from the primary perturbation, resulting in a trade-off between completeness and robustness, and hence utility for decision-making. [Andy Reisinger, New Zealand]	Taken into account.
8-1096	8	54	5	54	7	This sentence isn't very clear. [Government of United Kingdom of Great Britain & Northern Ireland]	Rejected since we do not see how this can be reformulated to make it clearer.
8-1097	8	54	17	54	22	This paragraph describes the strong dependency of climate-carbon feedback of both GWP and GTP. According to the sources cited GTP is significantly more dependent on this that GWP. It should be clarified which effects (if any) have been included in the data presented in appendix 8.A. [Government of Denmark]	Accepted. Yes, this has been made very clear.
8-1098	8	54	21	54	22	Should this be "climate-carbon cycle feedbacks"? [Katsumasa Tanaka, Switzerland]	Rejected. Yes, but that is clear from the first part of the sentence
8-1099	8	54	22			This tale of added CO2 for a CH4 perturbation (quite logical, as a warming from CH4 releases CO2) and its importance could be explained as adding along-term impact (CO2) on top of a short-term perturbant (CH4). We have many examples of this, the NOx (<1 week) kick CH4 (10 y). The problem with the text is that it picks out the case where CH4 has disappeared and then of course the added CO2 is dominant (GWP-100 or -500). The key case here is one of time scales as well as feedbacks. [Michael Prather, United States of America]	Noted. And partially taken into account. Discussion changed.
8-1100	8	54	22			It's not strictly correct that Reisinger et al (2010) "found" the carbon cycle feedback as we did not address this explicitly in our study, it's rather an implicit feature resulting from our model design. Perhaps rephrase "Enhancement of methane's GTP due to carbon-climate feedbacks may also explain the higher GTP values found by Reisinger et al. (2010)." [Andy Reisinger, New Zealand]	Accepted as suggested.
8-1101	8	54	24	54	24	The current wording "the inclusion of indirect effects and feedbacks in metric values has been inconsistent in the IPCC reports". This is another example of a very biased choice of language. A more neutral and factual way to describe the evolving science and the addition of more and more indirect effects would be to say just that: i.e. while initial IPCC reports were taking into account a limited number of indirect effects and feedbacks,	Noted. And partially taken into account. Text and discussion changed.

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						the literature evolved over time and so did the IPCC Assessment reports, i.e. by including many of the feedbacks. [Government of Germany]	
8-1102	8	54	24	54	33	It is stated that since TAR the carbon-climate feedback has been included in the modelling. The paragraph further goes on to state that the inclusion of the feedback in the modelling has caused the results in TAR, AR4 and AR5 to be biased towards CO2. This seems like a strange conclusion, presumably the feedback is “real” and hence, while it might be uncertain, provides a more realistic estimate than to ignore the carbon-cycle feedback. One would assume that the methodology used in TAR, AR4 and AR5 is more correct than the SAR methodology. The text in this paragraph should be clarified. [Government of Denmark]	Taken into account. The text has been improved to clarify that the inclusion of climate-carbon feedbacks only for the reference gas and not the non-CO2 gas (which is an improvememnt in itself) lead to inconsistent values in AR4.
8-1103	8	54	24	54	33	The inclusion of indirect effects and feedbacks are only discussed for GWP not for other metrics. Please discuss whether this is relevant for other metrics in a similar way. [Government of Germany]	Taken into account. We have mentioned this for GTP, but will make that more clear.
8-1104	8	54	27	54	27	The wording "adopted by the Kyoto Protocol" is not fully correct and maybe the authors are unaware that the updated IPCC AR4 GWP values are the basis for any second commitment period of the Kyoto Protocol. Please revise. [Government of Germany]	Taken into account. Will mention that AR4 values will be used in the 2nd period of the Kyoto Protocol.
8-1105	8	54	30	54	30	"climate-carbon cycle feedbacks" rather than "carbon cycle-climate feedback"? Also, I believe that such feedbacks are usually plural because there are climate feedbacks on the ocean and land CO2 uptake. [Katsumasa Tanaka, Switzerland]	Taken into account.
8-1106	8	54	30	54	32	I get confused here. AR4, TAR, and AR5 model includes carbon -climate feedback - why should then there be a bias?? [Fortunat Joos, Switzerland]	Taken into account. The text explains this better. The problem is that inclusion of this for CO2 leads to an inconsistency since this is not done for the non-CO2 gases in the numerator.
8-1107	8	54	31	54	31	A "bias" in this context suggests a wilful distortion of a metric. Again, the authors should choose less judgmental wording, as others would argue that the GWPs give much more weight to non-CO2 gases than other metrics, e.g. GTPs with a long time horizon. Thus, a) choose more neutral language to state that this particular inclusion of carbon-cycle feedbacks might imply a higher relative importance and b) clarify that GWPs are not in general "underestimating the relative importance of non-CO2 gases" but only if "integrative radiative forcing" is regarded as the yardstick for the comparison of importance. [Government of Germany]	Taken into account. Text has been changed. Explanationsadded to the extent space allows us.
8-1108	8	54	35	54	46	Discuss the scenario dependency of metrics not only for GWPs but as well for GTPs (which can have a much larger scenario dependency). [Government of Germany]	Taken into account to the extent that there is basis for this in the literature. Most studies have done this for GWP and less is available for GTP.
8-1109	8	54	35	54	46	This para reads very much like a shopping list, without clear structure or conclusions. Please see if you cannot arrive at a conclusion along the lines of "If future global emissions are close to an RCP2.6 pathway, then ...; but if emissions follow a higher trajectory, then....". Also, to be an assessment, some confidence statements should be applied. [Andy Reisinger, New Zealand]	Taken into account. Text improved.
8-1110	8	54	41		44	The text here is confusing. Isn't the AGWP of CO2 lower in 2100 than 2000? (the text seems to say the opposite). What does 'following a constant background' mean? Surely if the background is constant the AGWP will not change at all. Also what is RCP3D? [Nathan Gillett, Canada]	Taken into account. Text improved.
8-1111	8	54	48	54	50	The sensitivity is explored in Figure 6 of Joos et al. (2012, ACPD). But the nonlinearity is known in other fields for a long term. For example, the saturation of oceanic CO2 uptake under rising atmospheric CO2 concentration, which is mainly responsible for the pulse-size sensitivity, has been known since 70s (Revelle, R., W. Munk (1977) The carbon dioxide cycle and the biosphere. In Energy and climate, studies in geophysics. pp.140-158. National Academy Press, Washington, D,C., USA.). [Katsumasa Tanaka, Switzerland]	Noted.
8-1112	8	54	52	8	52	What is NTCF [M Daniel Schwarzkopf, United States of America]	Rejected. Defined earlier in chapter
8-1113	8	54	52	54	52	Fro NTCF the metric value also depend on time of emission, not only location. [Terje Berntsen, Norway]	Taken into account. Added.
8-1114	8	54	52	54	56	Similar to comment on page 8-51 line 19: In a box that says "choices required when using emission metrics" this paragraph here assumes that emission metrics can be sensibly applied to emissions of short-lived climate forcers that have a spatially and seasonally very heterogenic response. No previous IPCC report even	Noted. This comment reflects that metrics are being used in several contexts and for different applications; i.e. in the scientific literature for comparing emissions

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						provided quantitative values for GWPs for short-lived climate forcers such as black carbon, so this section here implicitly makes the value judgement that it might be sensible to do so and only the "spatial dimension for emissions and response" is a choice, not the possibility per se. Thus, please rephrase by a) making clear that if one were to extent the metric concept to short lived climate forcers, this would be going beyond the traditional application of metrics given that the effect of the emissions depends on the season (black carbon), location and even time of day (aviation induced cirrus, for example) of the emissions. Metrics are first of developed for things that can to some degree be compared; whether short-lived climate forcers can be compared to well-mixed GHGs, the domain so far for metrics (see IPCC AR4) is an open question. b) Beyond this fundamental question, whether metrics can be meaningfully extended to short-lived climate forcers, highlight not only the "spatial dimension" in vague terms but all the dimensions that would be leading to rather heterogenous effects which would have to be considered in case that "equivalent" metrics would want to be applied to short-lived climate forcers with atmospheric residence times below a year, i.e. the location, season, weather conditions, co-emissions. [Government of Germany]	and effects, in policy assessments, emission trading and in international agreements (as discussed at page 50, line 34-37). Metrics for SLCF are already used extensively in the scientifc literature, and here we do an assessment of these metrics and estimates. Such an assessment do not imply any value judgement or recommendation for inclusion in climate policies. It is also worth noting that AR4 presented metrics fro SLCFs: fig 2.2. shows integrated RF for long and shortlived components which is the same as AGWPs. The GWP table 2.14 in AR4 also contains some gases with short lifetimes. Finally, AR4 WG1 section 2.10.3. presents GWPs for components with indirect effects (CO, NOx, VOC and H2) and table 2.15 gives an overview of published estimates. This is a part of a scientific assessment performed in AR4 and a similar assessment was presented here in AR5 SOD.
8-1115	8	54	52	54	56	Short-lived species might be a better term that NTCF. "SLCF" is a commonly used term to define these short-lived climate forcers. [Government of United States of America]	Rejected. It has been decided to use the term NTCF in AR5 WG1.
8-1116	8	54	54			For a stationary linear system, the calculation by pulse or steady-state is identical - please note they should give the same results. [Michael Prather, United States of America]	Taken into account in discussion earlier in the text.
8-1117	8	54	55	54	56	It would be beneficial for the readers if the text clarifies why adopting sustained emissions are a strong assumption (i.e. why pulse emissions are more frequently used in emission metrics). [Katsumasa Tanaka, Switzerland]	Rejected. We agree that this could be usefull but due to space limitations we cannot explain this further.
8-1118	8	54	55			It is not the change in future emissions per se, but the change in future composition and climate. [Michael Prather, United States of America]	Taken into account. But due to space limitation we could not discuss this in detail. We have however, removed the figures using sustained emissions from the main text.
8-1119	8	55	1	55	10	I felt this para belongs with the discussion of scientific uncertainties, to start at page 53 line 48, before structural uncertainties are discussed. All uncertainties mentioned in this para are scientific, not structural (although the approaches taken by the different studies are structurall different, but each individual confidence interval is not). [Andy Reisinger, New Zealand]	Taken into account. Para is moved and rewritten.
8-1120	8	55	1	55	10	I think it important to clarify that Reisinger et al. (2010, Geophysical Research Letters) explored the uncertainties arising from the differences between models as well as those estimated from historical observations. [Katsumasa Tanaka, Switzerland]	Rejected. Given space restriction we think this is not worth mentioning.
8-1121	8	55	4			Prather etal 2012 GRL calculated an uncertainty in the GWP-100 for CH4 of +-13% (1 sigma) but that included only adjustment times and RF factors, not a changing future atmosphere. What was included in the values quoted here? If scenarios then a better explanation may be needed. [Michael Prather, United States of America]	taken into account and improved.
8-1122	8	55	6	55	8	Split in two sentences. One for GWP and one for GTP. [Terje Berntsen, Norway]	taken into account and improved.
8-1123	8	55	9	55	10	Sorry, but I think this conclusion is a cop-out. It simply repeats the literature but does not assess it. The authors should, based on their assessment of the scientific rigour of the studies cited in the para, offer their assessment of what the uncertainty is (with a single number, not a rather wide range), and offer a confidence level. Something like: "Based on the range of studies, with their differing approaches and types of uncertainty, we assess the current scientific uncertainties for CH4 to be ±40% for GWP100 and ±70% for GTP100 (high confidence)." Or something similar that results in a usable figure and doesn't simply repeat the range contained in various publications. At present the authors are saying "any of the above figures and studies might be true", which is ok for a review but I think it is dodging the task of doing an assessment. You should	Accepted. Text changed with more focus on assessment of uncertainties.

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						also make clear whether these are 90% confidence intervals or a likely range - use the codified IPCC uncertainty lexicon. [Andy Reisinger, New Zealand]	
8-1124	8	55	10	55	10	Uncertainty in the uncertainty? I would recommend that you make your best judgement of the uncertainty and give one number. If the range is for asymmetric uncertainty, this must be clearly stated. [Terje Berntsen, Norway]	Taken into account. More own judgment and assessment given.
8-1125	8	55	11			I feel that Table 8.1.A should be called out here prominently, because it concludes this discussion and it is the one of the key policy-relevant outputs from this section - updated metric values if policy makers chose to continue to use those specific metrics. The call-out to the table should also include a brief reflection on the assessed uncertainty of the various metric values, building on the conclusions of the para on page 55 lines 1-10, and ideally offering some inferences regarding uncertainties for other gases where those have not been assessed explicitly. I feel this table ought to be part of the chapter, not in an appendix: many billions of dollars ride on the numbers that it contains. A perhaps unpleasant thought, but that's what it is, and the authors should not hide this table away. [Andy Reisinger, New Zealand]	Taken into account by giving a brief reflection in section 8.7.2.1 of what the table is telling. But we think the best solution is to have this in the appendix. This avoids breaking up the text and also makes it easier to read the table.
8-1126	8	55	12	55	12	As presented, the economic metrics do not seem to replace physical metrics completely, but only add additional parameters. Without an equivalence of different gases in physical terms, the cost-effectiveness framework or cost-benefit framework could not be applied in multigas-framework (which is basic for cost-effectiveness). Thus, this seems to be supplementary concepts instead of concepts replacing the physical metrics. This should be made clearer. The text completely misses a discussion of uncertainties of the new metric concepts. [Government of Germany]	Taken into account. Text has been edited to reflect the fact that the economic metrics include physical measures as well as economic ones. The issue of uncertainties is too large to take on comprehensively in this section, but we mention in the fourth paragraph that economic dimensions introduce additional uncertainties.
8-1127	8	55	12			Section 8.7.1.5: The discussion between the fixed time horizon and the time-dependent time horizon could be introduced somewhere appropriate. The concept of the changing time horizon toward the policy target year is discussed in Berntsen et al. (2010, Climatic Change Letters). Its numerical consequences are tested in Tanaka et al. (2012, Climatic Change Letters, under revision). [Katsumasa Tanaka, Switzerland]	Rejected (partly diue to space limitations).
8-1128	8	55	12			Section 8.7.1.5: Somewhere in this section, it can be briefly introduced how metrics are used in the cost-benefit setting. See Marten and Newbold (2012, Energy Policy, 51, 957–972). [Katsumasa Tanaka, Switzerland]	Taken into account. We have added reference to this paper.
8-1129	8	55	14			Even more strongly I feel that discussion of economics does not belong in this chapter or perhaps even in the report. Deal with economics in some other WG. [Stephen E Schwartz, United States of America]	Rejected. Discussion of alternative metrics is given by scoping.
8-1130	8	55	17	55	18	The current wording "theoretically appropriate metrics" seems misleading, as if other metrics that do not include economic dimensions were not appropriate, even not theoretically. Please rephrase. It is highly questionable that a physical metric has to include any economic dimension, as the choice of an appropriate target completely depends on the value judgement of what the appropriate target would be. For a stabilisation of CO2 equivalence concentrations over different time horizons, the GWP metric seems very appropriate, for minimising peak temperatures, a tailored GTP metric seems to be more appropriate, for a mix of policy goals, a blended metric seems to be most appropriate, but only if the goal itself includes an "economic dimension", the metric should "theoretically" include an economic dimension as well. Thus, it would be advisable, if the authors do not make implicit value judgement of what appropriate goals were, and simply discuss which metric would be appropriate for which policy goal. [Government of Germany]	Taken into account. We have clarified that metrics with economic dimensions are designed to directly address an agreed upon policy goal, and that physcial indexes can only be an approximation to them (although other factors may be judged important such as additional uncertainties introduced by economic indexes)
8-1131	8	55	20	55	21	It should be mentioned that this requires an agreed temperature target as a pre-condition. [Government of Germany]	taken into account.
8-1132	8	55	20	55	32	A judgement is being made in this paragraph about what is "appropriate". Please rephrase. Perhaps: "an appropriate emissions metric may be…" and "… an appropriate index may be …" [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to comment 8-1130.
8-1133	8	55	20	55	32	It might be worthwhile to bring out more clearly, based on Tol et al (2008, or 2012?), that if the goal is a cost-effectiveness one (such as minimising an externally derived peak temperature), then the (time-dependent) GTP is more consistent to use, whereas if the goal is more a cost-benefit one, then GWPs are more consistent. [Andy Reisinger, New Zealand]	Noted. We believe this point is already made clear in the text.

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8-1134	8	55	20	55	32	If possible, it would be useful here to refer to a relevant chapter of WGIII AR5. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted as suggested.
8-1135	8	55	21	55	21	Again, not restricted to gases [Terje Berntsen, Norway]	Taken into account and changed to "components".
8-1136	8	55	24	55	24	The GCP concept seems to require an agreed least cost scenario to be established. This pre-condition should be mentioned more clearly. [Government of Germany]	Taken into account. Text clarified.
8-1137	8	55	29	55	29	Suggest that you write down the acronyms for Global Damage Potential and Relative Damage Cost, i.e. GDP and RDC, in the same way as it is done for GCP and CETO in line 22 & 23, so as to be consistent we have acronyms are treated in the text. [Daniel Johansson, Sweden]	taken into account.
8-1138	8	55	30	55	30	I suggest to replace "measures" with "metrics" , to avoid confusion with "emission reduction measures" [Terje Berntsen, Norway]	Taken into account. Text changed.
8-1139	8	55	34	55	34	GTP could just as easily be used as an example as GWP, and therefore both should be included or the text should be modified to simply speak of physical metrics. [Government of Denmark]	Rejected. It is relevant to mention GWP since it is the most common metric used.
8-1140	8	55	34	55	34	It is somewhat difficult to believe that the IPCC authors here make a value judgement call on what the "most appropriate" metric would be. Please refrain from making policy prescriptive statements. [Government of Germany]	Taken into account. See response to comment 8-1130.
8-1141	8	55	34	55	35	Again, a judgement has been made as to what is appropriate. Rephrase. [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to comment 8-1130.
8-1142	8	55	35	55	36	Reisinger et al (2012) as published in Climatic Change, didn't really look at country level but only a regional levels of livestock production. A separate study, pulished as a peer-reviewed technical report, did look at implications for New Zealand specifically - this might be more appropriate and relevant to cite in this particular context. Perhaps add "regional" to "project or country" level and cite both the Climatic Change paper and the technical report. To my knowledge, Godal and Fuglestvedt and Reisinger et al (Climatic Change and Technical report) are the only three studies that have tried to quantify regional or national cost implications. [Andy Reisinger, New Zealand]	Rejected since we cannot refer to reports.
8-1143	8	55	36	55	39	Need to add "assuming full participation of all sectors and regions in a global mitigation effort" - this is a crucial underpinning assumption of all those model studies. Please add Reisinger et al (2012, Climatic Change) here since we explicitly tested the importance of alternative 'imperfect' metrics, and came to those conclusions, and also Smith et al (2012 in Climatic Change). [Andy Reisinger, New Zealand]	Taken into account. Text changed and references added.
8-1144	8	55	39	55	39	More recent papers to cite here, that do not include some of the simplifications in these earlier works, include: Smith et al (2012) and Reisinger et al (2012), both published in Climatic Change (pus on-line August and October respectively) [Steven Smith, United States of America]	taken into account. References added.
8-1145	8	55	41	55	42	I believe economists will highly disagree with this statement. One could claim that physical metrics just ignore these uncertainties. I believe it is similar to claiming that the direct radiative forcing of aerosols is better than the adjusted forcing because it is less uncertain. [Terje Berntsen, Norway]	Taken into account. Text changed.
8-1146	8	55	41	55	42	The sentence that physical metrics remain attractive should be changed into 'are still necesary for the establishment of economic metrics and the introduction of mitigation and damage costs add further uncertainties. [Government of Germany]	Taken into account: See response to comment 8-1145.
8-1147	8	55	41	55	42	The wording "remain attractive" implies (as the wording in the above paragraphs) that the author or authors have a strong personal view on the appropriate metric to be chosen. However, whether to use "physical metrics" or metrics that include an economic dimension is a value judgement and there can be good reasons for both these broad options. Please refrain from using any implicit language that goes beyond a neutral and factual description of the different concepts. [Government of Germany]	Taken into account: See response to comment 8-1145.
8-1148	8	55	45	55	47	My analysis (Tanaka et al., 2012, Climatic Change Letters (in revision)) shows that this is not the case. GTP is quantitatively very different from GCP, if not qualitatively. [Katsumasa Tanaka, Switzerland]	Taken into account. Text changed and reference added.

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8-1149	8	55	47	55	47	Figure 8.29 does not fit into the discussion in this paragraph. [Katsumasa Tanaka, Switzerland]	Rejected. After consdiering this we found that it would fit best here since it is related to some metrics from economics.
8-1150	8	55	47	55	50	Figure 8.29 seems to fit much better to the GTP section than here. The variation of GTP in time is not clearly explained in the GTP section and this should be moved and expanded there. From the graph and the para it remains unclear why GTP is related to the cost-effectiveness framework and GWP to the cost-benefit framework. This should be better explained. [Government of Germany]	Rejected. After considering this we found that it would fit best here since it is related to some metrics from economics.
8-1151	8	55	56	55	57	This sentence does not fit into the flow of argument in the two proceeding sentences. [Katsumasa Tanaka, Switzerland]	Taken into account. Sentence moved.
8-1152	8	56	2	56	2	It is more precise to say "post-target temperature effects" rather than "long-term temperature effects". [Katsumasa Tanaka, Switzerland]	Taken into account as suggested
8-1153	8	56	5	56	6	I think it fair to state that the CETP approximates the GCP. This is first shown by Johansson (2012, Climatic Change) and later confirmed by Tanaka et al. (2012, Climatic Change Letters (in revision)). [Katsumasa Tanaka, Switzerland]	Noted. This is what we already have written..
8-1154	8	56	13	56	14	The point of Manning and Reisinger is not so much that we talk about broader time spans than the GTP normally does, but that we tried to step away from any prescribed time horizon and look at entire emissions trajectories. Note that this approach is based on the Forcing Equivalence Index originally proposed by Tom Wigley. [Andy Reisinger, New Zealand]	Taken into account. Text is revised.
8-1155	8	56	13	56	14	I do not understand this sentence very well. Also, this sentence does not look relevant to the papers cited here (Manning and Reisinger, 2011; Tanaka et al., 2009). Differences between FEI (Manning and Reisinger, 2011) and TEMP (Tanaka et al., 2009) are fundamental and discussed in Tanaka et al. (2012, Climatic Change Letters (in revision)). The discussion for TEMP (Tanaka et al., 2009) was more precise in FOD, as far as I can recall. [Katsumasa Tanaka, Switzerland]	Taken into account. Text changed.
8-1156	8	56	21	56	22	In the GTP section, it should be clearer mentioned what is stated here, which is that the GTP concept cannot account for effects over a broader time horizon. [Government of Germany]	Taken into account.
8-1157	8	56	21	56	30	This para suggests a simple physical interpretation of the GWP. This is at odds with the negative statement on p 52, lines 6-10, that "the GWP does not translate directly into any climatic response parameter". [Government of Germany]	Noted. But we find the statement adequate since we write "directly", and in the para here we discuss how GWP can be interpreted.
8-1158	8	56	21	56	30	MGTP, IGTP, and iGTP are essentially the same. This can be made explicit to avoid confusion. [Katsumasa Tanaka, Switzerland]	Rejected. Due to spacelimitations we had to be very short here. But we have explained this earlier in the text and we give references to these papers.
8-1159	8	56	29	55	29	I think this is too vague. I would suggest to change "may lead to" to "provide" [Terje Berntsen, Norway]	Accepted as suggested
8-1160	8	56	29	56	30	I think that sentence may be too cryptic for most readers - spell out what that means. [Andy Reisinger, New Zealand]	Taken into account. Text changed.
8-1161	8	56	32	56	32	The term "Biogenic emissions" are too wide as it includes all natural sources of CO2. The discussion here is about anthropogenic combustion of biomass (i.e. biofuels). [Terje Berntsen, Norway]	Taken into account. Removed “biogenic” and added “from the combustion of biomass”.
8-1162	8	56	32	56	32	The text states that "biogenic emissions of CO2 are not allocated to national emission totals". This is not entirely accurate, as for example changes due to afforestation, reforestation and deforestation are taken into account in the Kyoto Protocol. See Article 3.3 and 3.4 of the Protocol. [Tommi Ekholm, Finland]	Taken into account. Text changed.
8-1163	8	56	32	56	43	Metrics: since agriculture is a major emitter of GHG it may be useful to discuss the greenhouse gas intensity (GHGI) which scales GHG emissions by crop yield. That is an extremely useful definition, specifically if food production and mitigation options are discussed. Mosier, A.R., Halvorson, A.D., Reule, C.A. & Liu, X.J.J. 2006. Net global warming potential and greenhouse gas intensity in irrigated cropping systems in north-eastern Colorado. Journal of Environmental Quality, 35, 1584–1598 [European Union]	Rejected. Thanks for the comment, and we are aware of this paper and agree with your thoughts. The paper introduces biomass use into the metric and thus differs from the pure concept of comparing two species, and moves more towards LCA. In that context, this paper is best referenced in the WGIII

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							section where there is more detail
8-1164	8	56	32	56	43	It should be discussed how this approach conflicts with other approaches to account for time-lag between oxidation and regrowth. In this para it is only mentioned that regrowth does not occur immediately, however it is not mentioned that often oxidation either occurs instanteneous, which is taken into account in the accounting of harvested wood products. The concept seems to erroneously replace harvest with oxidation. [Government of Germany]	Taken into account. We have rephrased the start of this paragraph to focus on bioenergy and not biomass more broadly. Cherubini et al 2011 clearly assume that the bioenergy is used in the year of harvest, but Cherubini et al 2012 (referenced) relax this assumption to look at decay functions as is this case for harvested wood products (HWPs). We have made the 2012 reference more prominent at the start.
8-1165	8	56	34	56	34	"Oxidation" is technically correct, but possibly confusing. Use "emission" instead. [Terje Berntsen, Norway]	Taken into account. “oxidation” replaced with “combustion”
8-1166	8	56	34	56	34	"it causes RF" needs to be re-phrased although I know what this means. [Katsumasa Tanaka, Switzerland]	Taken into account. Text changed.
8-1167	8	56	35	56	43	Cherubini et al. are not the only ones who have been developing GWPbio metrics. See also e.g. Pingoud et al., "Global warming potential factors and warming payback time as climate indicators of forest biomass use", Mitig Adapt Strateg Glob Change 17, pp- 369-386 (2012), and provide a more comprehensive literature survey. [Tommi Ekholm, Finland]	Rejected. Thanks for the comment, and we are aware of this paper and agree with your thoughts. The paper introduces biomass use into the metric and thus differs from the pure concept of comparing two species, and moves more towards LCA. In that context, this paper is best referenced in the WGIII section where there is more detail
8-1168	8	56	36			Values of what? [Nathan Gillett, Canada]	Taken into account. Added "GWPbio".
8-1169	8	56	37			The idea of calculating GWPs for biomass is so new extension that other study groups arriving roughly to the same results from somewhat different starting points should be also mentioned. It gives weight and robustness for the idea. Please, add after the words "Cherubini et al. (2011) " the words "and Pingoud et al. (2012)"… 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 17: 369–386. Springer. doi-link: 10.1007/s11027-011-9331-9. [Ilkka Savolainen, Finland]	Rejected. Thanks for the comment, and we are aware of this paper and agree with your thoughts. The paper introduces biomass use into the metric and thus differs from the pure concept of comparing two species, and moves more towards LCA. In that context, this paper is best referenced in the WGIII section where there is more detail
8-1170	8	56	39	56	39	Please introduce the GTPbio concept briefly. Now the text jumps suddenly from GWPbio to GTPbio. [Tommi Ekholm, Finland]	Rejected. Given the GTP has been discussed extensively in early sections, it should be apparent that the GTPbio is the extension of the GTP to biomass”
8-1171	8	56	47			SLCF is used, but not defined. This appears to be the only use in the document. Is NTCF used elsewhere for the same thing? Consistenct should be ensured and, historically, that terms seems to have been SLCF. [Government of United States of America]	Accepted. Changed to NTCFs used in rest of chapter/report.
8-1172	8	56	49			At the end of the paragraph, please add the reference (Kirkinen et al. 2008). Kirkinen et al. (2008) considers the integrated radiative forcings as fuction of time (AGWPs) in relation to fuel energy for several biogenic and fossil fuels. 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. This section is on biofuels, and the cited paper only mentions albedo once: “The concept of RF can also be extended to cover changes of albedo”. See WGIII for discussion of this issue.
8-1173	8	56	56	56	56	Consider change wording. Indicates a one-one relation between forcing region and respons region. [Terje Berntsen, Norway]	taken into account. Reformulated.
8-1174	8	57	5		12	I think more attention should be given to the multi-component approach. This Smith et al. (2012) approach has many advantages which it is not given full credit for here. The metrics derived are less sensitive to arbitrary parameter choices. The different nature of long-lived and short-lived forcings is made explicit. The cumulative emissions approach lends itself directly to a policy goal of limiting global mean temperature increase to a particular amount. The paragraph refers to 'peak warming in the distant future', but global warming at any time is approximately proportional to the cumulative emissions up to that time, so it is not true that the method is	Rejected.Good comment. But given the space we have it will not be possible to give more attention to the multi-gas approach. I have suggested to WGIII that they discuss this in more detail.

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						just focused on the far distant future. Moreover the objection that impacts other than peak warming are not addressed is common to most metrics, and certainly GTP and GWP do not go any further than this. [Nathan Gillett, Canada]	
8-1175	8	57	10	57	12	The final sentence on the discussion of metrics proposed by Smith et al. is unclear to me and could be refined. To state that they "do not account for the timing of emissions relative to the target year" assumes some sort of target year, which is not the case in Smith et al. The metrics are in fact designed for a peak temperature policy (such as limiting peak warming to 2degC) rather than a time-bound policy (such as limiting warming by 2100 to 2degC). If "target year" means any given year of interest (e.g. 2100) then this ought to be clarified. I would also note that this loss of timing information comes at the gain of reducing the structural uncertainty from time horizon choice discussed at the end of p53. The final part of the sentence about not capturing impacts beyond peak warming is already covered by the general statement on p50 lines 49-50 ("a metric that establishes equivalence with regard to one effect cannot guarantee equivalence with regard to other effects") and applies equally to every other metric. As a result I suggest editing this sentence to "This approach uses time-invariant metrics that largely avoid the structural uncertainties arising from time horizon choice, but it does not provide information on the contribution of emissions to warming in a specific target year." [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Taken into account, Text changed.
8-1176	8	57	11	57	12	Remove "of course". [Katsumasa Tanaka, Switzerland]	Accepted as suggested.
8-1177	8	57	14			On section synthesis 8.7.1.6 with the particular wording example (line 53 on page 8-57): "As pointed out in several studies... The time invariant GWP is not well suited for a policy context with a global concentration, forcing or temperature target. The GWP and variants may be more suitable." This seems to be a gross distortion of the literature and a policy-prescriptive value judgement for several reasons: a) "a policy context with a global ... target" is something very different from a specific policy goal that is 1:1 reflected by a chosen metric. The current policy context is arguably the 2C target. Thus, only a metric that would reflect exactly this target would theoretically be "suited". This would arguably be a GTP metric that defines exactly the future emission scenario and hence can infer the point in time at which global temperatures peak and work backwards the time-variant time horizon of this GTP. (BTW: Even that is impossible due to the uncertainties of when exactly peak temperatures will be reached). The authors might want to state such a 1:1 theoretical connection in their discussion for various metrics, rather than a broad endorsement of the GTP metric as such over a GWP metric. The authors should however then as well discuss that a "policy context" of a 2C target might still allow for a mixture of different parallel policy goals, that could for example be better represented by an integrative metric such as the GWPs, i.e. to give some weight to more near-term temperature rates of changes as well. The current text of the whole synthesis section fails to clearly discuss the relationship between 'metric', the specific 'policy goal that the metric shall/does represent' and the mixture of larger policy contexts and metrics that are a) existing in the real world and b) very well internally consistent, if the larger mixture of may policy objectives is taken into account. In any case, please refrain from any policy-prescriptive language. The meeting report from the IPCC Oslo meeting on metrics was largely very well balanced in its wording. This AR5 metric section is not. [Government of Germany]	Taken into account. The text is changed in order ro improve balance. But we find the comment somewhat unclear.
8-1178	8	57	16	57	16	I disagree with this statement because the definitions for the scientific and structural uncertainties are not clear to me. See my comment on p.53 line 13-16. [Katsumasa Tanaka, Switzerland]	Taken into account. The text is reformulated
8-1179	8	57	16			As per my main comment earlier, I think this should be three elements: value-judgements, structural and scientific uncertainties. Whether one uses an integrated or end-point metric is not an uncertainty, it's a choice. [Andy Reisinger, New Zealand]	Taken into account. The text is reformulated
8-1180	8	57	24	57	25	It should be discussed how the implementation of regional/local metrics will affect the comparability at the global scale including the significance for climate mitigation projects. Also, it should be further discussed whether the observed regional differences are statistically significant or they are within the uncertainty ranges. [Government of Denmark]	Rejected. Good points, but we don't have space for a discussion of these issues.
8-1181	8	57	24	57	27	At the beginning it was said that different concepts serve different purposes, here the language implies that the alternatives to GWP introduce some positive features, however the more dynamic temporal contributions may pose much more problems for policy makers than a static as it is highly undesirable if 'exchange rates'	Noted. We agree that these issues would require more discussion but we don't have space for that. We will however, include a clarification regarding

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						change strongly over short time horizons. [Government of Germany]	changing metric values over time. (i.e. that some of these are related to changing background levels, but that the change driven by proximity to a target year can be given in a way that will not make this upredictable, but rather available for planners and policymakers.)
8-1182	8	57	25	57	25	The text states that "Among the alternatives, the GTP concept has the broadest application." Is the meaning of this sentence that the GTP is used more often in research than other alternative metrics? If so, is the broad application a merit for a metric? The sentence sounds like an advocacy for GTP, but popularity should not be a reason to advocate a metric. Popularity has nothing to do with the physics or economics of climate change. Please remove the sentence. [Tommi Ekholm, Finland]	Taken into account. Sentence removed
8-1183	8	57	25	57	27	This is my own work again, but TEMP (Tanaka et al., 2009) is a metric that directly addresses the time-varient nature toward the target. [Katsumasa Tanaka, Switzerland]	Taken into account. We have reformulated the text. We now write "time variant metrics" without mentioning any specific metrics.
8-1184	8	57	27			Reprase: "accounts for proximity to a prescribed temperature target." [Andy Reisinger, New Zealand]	Accepted as suggested.
8-1185	8	57	29		35	This paragraph gives a strong endorsement of the RTP concept. But is the RTP approach really that useful in the mitigation policy context? After all climate change is a global problem, so even though different forcings may have regionally-varying affects, it does not make sense to consider the temperature response solely in one region when making climate policy. And to be useful in this context one needs a single metric value for each forcer, not a matrix of values. [Nathan Gillett, Canada]	Noted. We think it is useful to consider the these types of metrics also as they expand the current concepts to include differences in regional responses (not only to regional of emissions). The RTP concept may help to convey information that is not visible to the users (e.g. policy makers) when global mean response metrics are used. In addition, this concept is also potentially useful for research and scientific assessments/analyses. We will however, slighlty modify the text and remove "promising".
8-1186	8	57	32	57	32	Replace "temperature pattern" with "climate response patter" [Terje Berntsen, Norway]	Accepted as suggested.
8-1187	8	57	32	57	35	The section "Many species, especially NTCF, produce ... other variables than temperature" is implying the fundamental assumption that metrics to compare short-lived climate forcers by some index of their strongly regionally and seasonally heterogeneous effect is something desirable or "promising" as the authors call it. The section should be kept neutral in the sense that the view that emissions with somewhat incomparable effects (because of the strong regional and seasonal, etc. aspects) cannot be meaningfully compared, so metrics for comparison are to a certain degree meaningless or simply just a value judgement that weighs very different effects against each other. This is why the concept of metrics traditionally (and probably for good reason) is limited to well-mixed GHGs which have, irrespective of their timing and location of the emissions, some strong spatial and some temporal overlap in their effects. Without this overlap in the effects, physical metrics become dependent on value judgements and the authors should be very clear about this strong limitation (if not disqualifying characteristic) of any attempt to design RTPs. Thus, the authors should refrain from using language such as "promising" etc. and use more factual neutral wording. [Government of Germany]	Taken into account. The word "promising" is not used.
8-1188	8	57	33	57	35	It should be mentioned that RTP (Abbreviation not introduced, only ARTP) is related to spatial pattern in responses. It remains very unclear, why this is promising as the spatial pattern are not important for the majority of GHGs. It should be reconsidered how important and for what specific uses this is really important. [Government of Germany]	Taken into account. "promising" is removed, and we may add some more explanation.
8-1189	8	57	33			RTP is used, but not defined. This appears to be the only use in the document. Does it mean Regional Temperature Potential? Either way, the authors should consider defining it (and maybe a different term used anyway in this Synthesis section, since RTP was not previously discussed?) [Government of United States of America]	Taken into account.
8-1190	8	57	37	57	37	The text states that "The GWPs suffer from inconsistent treatment of indirect effects/feedbacks." This is a	Taken into account. Text is changed.

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						universal statement, and I believe that the inconsistency is not an inherent feature of the GWP concept (apologies if I'm mistaken). Please adjust the text, e.g. "The reported values of GWP in AR4 and AR5 suffer from ..." If the AR5 reports also GTP or other metric values, please also indicate whether this inconsistency holds for these metrics. [Tommi Ekholm, Finland]	
8-1191	8	57	37	57	37	The wording "The GWPs suffer from inconsistent treatment of indirect effects/feedbacks". The only evidence that the authors presented was that early IPCC reports include less indirect effects and feedbacks - reflecting therefore the literature at that time. This "evolving science" seems hardly a justifiable basis for calling a certain metric concept into question, stating that this metric concept "suffers from inconsistencies". Any metric concept would have done according to that measure, so the current language seems misleading. Please rephrase. [Government of Germany]	Taken into account. Text is changed.
8-1192	8	57	37	57	37	This refers to the GWP values published in the previous IPCC reports rather than GWP values in general. Different papers use different models to compute metrics including the GWP. [Katsumasa Tanaka, Switzerland]	Taken into account. Text is changed.
8-1193	8	57	37	57	39	I am not sure if this is a fair statement. I imagine that climate-carbon cycle feedbacks are not widely appreciated or even known at the time of SAR. As far as I am aware, Cox et al. (2000, Nature) brought attention to such feedbacks into the climate research community. [Katsumasa Tanaka, Switzerland]	Taken into account. Text changed. But the point made here was more the assessments after SAR did the present GWPs that treated the feedbacks in an inconsistent manner.
8-1194	8	57	37	57	42	It is not only the GWP that suffers from this. E.g. the GTP also uses the IRF for CO2. [Terje Berntsen, Norway]	Taken into account. Text is changed.
8-1195	8	57	37	57	42	In the paragraph GWP are singled out as having inconsistent treatment of indirect effects/feedbacks. However, based on the previous pages exactly the same is the case for GTP. The text should be modified to reflect this. On page 54 (line 17-22) results of climate-carbon feedback on the CH4 GWP and GTP are reported, but judging from the text in this paragraph this has not been taken into account. It should be made more explicit for both GWP and GTP what indirect effects/feedbacks have been included in deriving the values presented in appendix 8.A. [Government of Denmark]	Taken into account. Text is changed.
8-1196	8	57	37	57	42	The text does not mention the treatment of indirect effects/ feedbacks in the GTP and other concepts at all. Should be discussed in a more balanced way. [Government of Germany]	Taken into account. Text is changed.
8-1197	8	57	37	57	42	Make clear that it is not only GWPs, but certainly GTPs as well, and the effect of including climate-carbon cycle feedbacks on the response to non-CO2 emissions may be much stronger there. I.e. avoid unintentionally characterising GWPs as having problems that could be solved by switching to another metric. [Andy Reisinger, New Zealand]	Taken into account. Text is changed.
8-1198	8	57	38	57	39	The current wording seems to be unaware of the fact that for the second commitment period of the Kyoto Protocol, the updated GWP values as provided by IPCC AR4 are chosen. [Government of Germany]	Rejected. We considered mentioning that KP2 has adopted AR4 GWP. But after searching for clear and solid references for this we found out that we should not mention this.
8-1199	8	57	44	57	44	The choice of time horizon is also very relevant for GTP. This should be reflected in the text. [Government of Denmark]	Accepted as suggested.
8-1200	8	57	44	57	44	This statement holds also for GTP. [Government of Germany]	Accepted as suggested.
8-1201	8	57	44	57	49	Discounting implies an arbitrary choice of discount rate. It seems incorrect that this choice is preferred to other choices made related to other metrics. It does not seem to be an alternative which is not value based, what the current text seems to imply. The text also does not take into account that in using GWPs and revising them in period shorter than 10 years, the argument put forward here that they have a large number up to the time horizon and 0 thereafter is not really a valid one, as the chosen 100 time horizon is very long in relation to policy decisions, for which the metric concept is used. The text should clearly discuss the enormous problems that would arise for all current uses of metrics (e.g. emissions trading CDM), if the 'exchange rate' is continuously changing. [Government of Germany]	Taken into account. Will make it clearer that discounting is also value based. We don't have space for discussing how a metric with changing values over time would function in policy contexts as this is beyond the scope of a scientific assessment. But note that the change driven by proximity to a target year can be given in a way that will not make this upredictable, but rather available for planners and policymakers.

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8-1202	8	57	45	57	49	Again: Please provide the same detail of information on GTP and GWP. The current text lists a number of problems for the GWP and is silent about those of the GTP. [Government of Germany]	Taken into account. GTP added here.
8-1203	8	57	45			"often seen as": replace with "is equally", because that's what it is. It simply shifts the point of controversy, but certainly doesn't make it go away - witness the discussions around the results from the Stern review! [Andy Reisinger, New Zealand]	Accepted as suggested.
8-1204	8	57	52	57	55	These two sencences use existing literature selectively and make vague and speculative statements, and thus show considerable bias. The text states that "the time invariant GWP is not well suited for a policy context with a global concentration, forcing or temperature target." GWP100 is perfectly suited for policy that targets integrated RF for 100 years. In addition, the notion of being "well suited" is vague. O'Neill (2003), Aaheim et al. (2006) and Johansson et al. (2006) (see the full citations from the WG1 SOD) have estimated that the use of GWP100 would increase global discounted mitigation costs by 2% to 4% when compared to a cost-efficient case (which is nevertheless a quite theoretical case). I think this is very modest increase, and I would describe the metric to be "well suited" towards the policies assessed in these papers. Please include the conclusions from these three papers. The text states also that "The GTP and variants may be more suitable." They may be, or they may not be. This is speculation, and is not suitable for a scientific synthesis on metrics in an IPCC AR. [Tommi Ekholm, Finland]	Taken into account. We have modified the wording.
8-1205	8	57	52	57	55	Here, the text is not only unbalanced in favour of the GTP, it even becomes policy prescriptive. Please remove any recommendations or assumptions about suitability of emission metrics for policy purposes. [Government of Germany]	Taken into account. Text changed.
8-1206	8	57	52	58	2	The text is policy prescriptive and biased. Certain metrics are better suited to particular uses and particular policy objectives. Here the impression is given, that GTP is generally better than GWP which contradicts the first sentence of the para. It should be mentioned what problems the GTP concept implies for the most important uses such as in the UNFCCC framework or for emissions trading, for which a time invariant metric may be much better suited. It is also important to address transparency and simplicity in the approach to determine metrics, given the fact that IPCC lacks behind in time and that metrics are necessary for substances that IPCC ARs do not yet mention at all. [Government of Germany]	Taken into account. Text changed.
8-1207	8	57	54	57	55	I would remove this sentence. Specifying any metric for being suitable for policy is not supported by the literature. [Katsumasa Tanaka, Switzerland]	Taken into account. Text changed, based on our assessment.
8-1208	8	57				This section is really asking for a Table classifying different metrics types [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Rejeceted. We have considered this possibility seriously at earlier stages and found that this will be difficult and may lead to more problems with how the readers percieve our assessment of metrics.
8-1209	8	58	1	58	2	There will always be a lot of other factors for policymakers to consider (apart from climate change) when different mitigation options are compared. The last part of the sentence seems to indicate that you suggest to include non-climate effects in the metric itself. I believe that the metrics should include climate impacts an donly that, and suggest to remove the last part of the sentence. [Terje Berntsen, Norway]	taken into account
8-1210	8	58	2			At the end of this para, I think it is crucially important to add that in a scenario of complete participation, the differences in economic costs are relatively small between GWPs and cost-minimising abatement pathways. This is discussed already earlier, and it is a key part of this synthesis, because the authors need to avoid painting a picture of GWPs being "not well suited" and "inconsistent" with economic objectives, when we know that the amount of inconsistency in economic terms is actually quite small. In other words, this synthesis needs to provide a sense of scale to make it policy relevant, and not discuss only theoretical concepts and their internal consistency. [Andy Reisinger, New Zealand]	Taken into account to some degree eralier in the text. But it was not possible to go much into this here. We agree that it would be good to indicate how serious the limitations are in the big picture (costs etc), but that is beyond the scope of our chapter. THis is also something that belongs to WGIII.
8-1211	8	58	4	58	10	It is disappointing that AR5 does not provide more insights into the link between metrics and applications. It is very clear for which purposes metrics are currently applied, but the applications and the impacts of different metrics on the existing applications are ignored in the text and the question is given back to policy makers. It is a very simple exercise to look at and discuss the existing applications of metrics. The second question also seems less relevant given the situation that policy makers are not able to address all types of indirect effects	Taken into account. But much of this is beyond the scope of ch8 in WGI, and belongs to WGIII. Thus, we could not go deep into this. We have also added a paragraph in the introduction stating what the limits of our assessment are, and that there are additional

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						and feedbacks. The third aspect transparency and simplicity is important and is not discussed in the chapter before related to the different concepts. The value judgements are stressed above all for the GWP in this chapter, but not in the same way for other concepts. [Government of Germany]	important aspects that need attention.
8-1212	8	58	4	58	10	I would add a fourth point to the issues on which dialogue is needed: the need to define 'good enough' and 'sufficiently fit for purpose', as compared to 'correct' in an absolute sense. Almost all metrics are perfectly suited to only one application, and hence 'wrong' for any other application. There is a real risk that the better is the enemy of he good, and that the use of perfectly good metrics is shunned because there is something 'better' out there for any individual policy goal. Given the multitude of policy goals, a better sense of what is good enough in an imperfect and not always clearly communicated policy environment must be developed through an increased dialogue. Contact me for details on a paper that is currently under review that may offer some further basis for including this point. [Andy Reisinger, New Zealand]	Taken into account. These are good points, but due to space limitations we cannot go much into this. And it is beyond the scope of this chapter to weight the scientfic performance of metrics vs the policy aspects, and to assess what is good enough. But this point is now brought up in text added to introduction.This discussion belongs to WGIII.
8-1213	8	58	4	58	10	I like the idea of fostering dialogues between policymakers and scientists to design emissions metrics to meet policy needs. I think that #1 is by far the most important. #2 and #3 seem to be scientific questions with just weak policy relevance. I am not sure to what extent policymakers provide useful inputs for #2 and #3. [Katsumasa Tanaka, Switzerland]	noted.
8-1214	8	58	5	58	5	I suggest that the text explicitly states the importance of the target parameter and the time horizon -- these are required inputs from policymakers so that scientists can design an emission metric (Berntsen et al., 2010, Climatic Change Letters). The other point worth considering is whether the policy is addressed in the cost-effectiveness or cost-benefit setting, which influences the construction of metrics (Tol et al., 2012, Environmental Research Letters). [Katsumasa Tanaka, Switzerland]	Rejected, partly due to space limitations but also since we find that this has been said clearly already earlier in the text.
8-1215	8	58	12	62	16	This section is really useful, but I would encourage the authors to discuss with WGIII whether some of this material might not be better suited to their assessment. I.e. the different emissions trends under different metrics, and the contributions of different sectors, is core WGIII territory and should be covered there. [Andy Reisinger, New Zealand]	Rejected. Contact is established with WGIII. But their treatment is mainly related to discussion of concepts and as far as I have seen there are no places in WGIII were this would fit in. This is something to bring up in the scoping of AR6.
8-1216	8	58	14			“Metrics for WMGHGs” – suggest using full language rather than acronyms for titles (makes for a difficult read otherwise) [Government of New Zealand]	Accepted as suggested.
8-1217	8	58	16	58	17	Table 8.A.1 also contains data on gases that are not well mixed in the atmosphere, in some cases with lifetimes less than 1 week. [James Franklin, Belgium]	Taken into account. Text changed.
8-1218	8	58	17			As per previous comment, this table should not be in an appendix but be a core part of this chapter. Also, give details on how Hodnebrog et al calculated the metrics, and how this relates to the issues covered earlier, i.e. treatment of feedbacks, issues arising from the use of pulse response functions rather than full modelling. Plus, to be consistent with the IPCC guidance on uncertainties and given the importance of the numbers in this table, some statement on uncertainty and confidence is needed. [Andy Reisinger, New Zealand]	Noted. We have decided to keep this table in the appendix, as we think it will be easier to read this way, and since it will not break up the main text. We will add, to the extent space allows, more on uncertainties.
8-1219	8	58	39	58	45	In particular with regard to Figure 8.32: why use year 2000 emissions here when Figure 8.31 used year 2008 emissions? More importantly, in Figure 8.32b, did the authors use the same single metric value to calculate the temperature response from each species, or did they take into account that the background concentrations will be changing over tim and hence the metric values for some gases should also change over time? If the authors assumed the same metric value, this means the figure is illustrative only but needs a clear caveat (with order of magnitude) of how mucn changing background concentrations might change this picture about the warming effect over time. [Andy Reisinger, New Zealand]	Taken into account. Emissions will be updated to most recent year possible. Fig 8.32b will be removed.
8-1220	8	58	42			The CO2-eq. Emissions of NTCF are given in figure 8.31 an dshows that there is quite some difefrence between teh components. I suggest that the subsections of 8.7.2.2. follows the magnitude of their CO2-eq. Emissions. Now it starts with a fairly detailed discussion of NOx which turn out to be a very minor part of the NTCFs. [Terje Berntsen, Norway]	Rejected. The net values in terms of CO2 eqivalents may hide larger individual contributions that are important for our understanding of man-made perturbations. In addition, the chapter also stress the limiation of metrics and using global mean values.
8-1221	8	58	42			“Metrics for NTCF” – suggest using full language rather than acronyms for titles (makes for a difficult read otherwise) [Government of New Zealand]	Accepted as suggested.

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8-1222	8	58	51			Original reference for this phenomenon is Wild et al, GRL 28, 1719-1722, 2001. [Michael Prather, United States of America]	Rejected. Generic text here without references.
8-1223	8	58	52	58	52	NOx also affects CO2 through nitrate deposition (fertilization effect) [Terje Berntsen, Norway]	Taken into account.
8-1224	8	58		62		The discussion of new metrics is confusing. I fail to see what policy makers will gain from there metrics. [M Daniel Schwarzkopf, United States of America]	Noted.
8-1225	8	58				Section 8.7.2.2: The section may be better and more appropriately titled, "Metrics for some short-lived species". SLCF - and not NTCF - has been the commonly accepted nomenclature to date. If a concerted effort is going to be made to introduce this new term for the old one (and there are justifiable reasons for doing so), the authors should be very explicit about this. Additionally, the authors may wish to consider stripping the very-short-lived species from Table 8A1, form a new table and include in this section as metrics for non well-mixed greenhouse gases. [Government of United States of America]	Taken into account: Title changed. And caption of table 8.A.1 is changed.
8-1226	8	59	1	59	1	due the should be "due to the" [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Accepted as suggested.
8-1227	8	59	7	59	8	It is not clear whether the effect of O3 on CO2 uptake by vegetation is included in the estimate of NOx GTP and GWP. Please clarify. [Government of United States of America]	Taken into account.
8-1228	8	59	15	59	15	Insert reference to "O. Hertel, C. A. Skjøth, S. Reis, A. Bleeker, R. Harrison, J. N. Cape, D. Fowler, U. Skiba, D. Simpson, T. Jickells, M. Kulmala, S. Gyldenkærne, L. L. Sørensen, J. W. Erisman, and M. A. Sutton (2011) Governing processes for reactive nitrogen compounds in the atmosphere in relation to ecosystem, climatic and human health impacts. Biogeosciences Discuss., 9, 9349-9423, 2012 - under review for Biogeosciences" [Stefan Reis, United Kingdom of Great Britain & Northern Ireland]	Rejected. Not relevant in this context.
8-1229	8	59	21	59	21	Add the word "positive" before "long-term ozone effects" [Terje Berntsen, Norway]	Accepted as suggested.
8-1230	8	59	29	59	35	It should be noted that the metric values for VOCs in general do not include SOA formation [Terje Berntsen, Norway]	Accepted as suggested.
8-1231	8	59	37	60	5	Would be better if the first paragraph (l.38-44), which covers regional dependence on BC metric value, was placed at the end of this section below the paragraphs on the global metric value. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted as suggested.
8-1232	8	59	48	59	57	Note that metric values for BC have been substantially revised in Bond er al. 2012 (re-submitted to JGR, nov. 2012) [Terje Berntsen, Norway]	Noted. This is refleced in table with BC values
8-1233	8	59	56			What does it mean that the values in Table 8.A.6 'are not consistent with AR5 values'? This is the AR5. Or should 'AR5' be 'AR4'? [Nathan Gillett, Canada]	Taken into account. Text clarified.
8-1234	8	59	57	60	1	Please provide proper references to the "two studies" mentioned here. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted as suggested.
8-1235	8	60	7	60	16	This section would likely be clearer if the information were presented in a table. The authors should consider including one. [Government of United States of America]	Rejected. After having considered this possibility we believe that a table would not improve the overview. And text is moved to Supplementary Material.
8-1236	8	60	33	60	50	The figures and discussion here are from one piece of research only which leaves the question of structural uncertainty rather open. Can you briefly say which models were used to estimate these GWP/GTP and temperature responses and whether this example should be treated as broadly representative of all other estimates (if there are any) because there is widespread consensus or whether this is just one of many, varied estimates. [European Union]	Taken into account. The number of figures will be reduced. And documentation is addedd in Supplementary Material.
8-1237	8	60				The evaluation of GWP(10) and GTP(10) values (e.g., in Figure 8.31) does not make sense as it conflicts with the IPCC definition of climate as average weather over time scales of 20 years or more [IPCC, 2007]. Evaluation of GWP(10) values is a significant departure from previous IPCC reports which report values for 20 years and longer. The points made in this section can be made equally well without referring to GWP(10) values, and I would recommend removing the GWP(10) and GTP(10) values.	Rejected. The time horizons applied here should not be confused with timescales for climate statistics and average weather. The time horizons indicate over whic time span we consider the perturbation, and is not related to what is possible to observe. E.g.

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						While it is well understood that there is no single "correct" time horizon over which to assess radiative forcing impacts, it is equally well understood that there are some "incorrect" time horizons (e.g., 1, 5, or 10 years). See the discussion in Environ. Sci. Technol., 45, 3169, 2011 for more details. [Timothy Wallington, United States of America]	according to the definition of GWP, we use 1 kg as marginal emissions; and integrate the effect over e.g. 100 years. This is not meant to be any observable quanitity.
8-1238	8	61	4	62	50	Grewe and Dahlmann (2012, Springer Verlag, "Atmopsheric physics" Schumann U., editor) pointed out that the discussion of the different metrics is very misleading. [Volker Grewe, Germany]	Noted. But unclear comment.
8-1239	8	61	4	62	50	Reason: Different metrics aim at different aspects of climate change: Short-term climate change vs. Long-term climate change; Cilmate change of a todays emission versus climate change induced by a scenario. [Volker Grewe, Germany]	Noted.
8-1240	8	61	4	62	50	It would be much better to discuss aspects of climate change and the respective metric. E.g. if we are aiming at minimizing 2050 temperature change then GTP2050 is the right choice. Etc. Different questions lead to different answers! Just looking at different answers does NOT imply that science is uncertain, but that different questions were posed! [Volker Grewe, Germany]	Noted. We agree with much of this. We assess some of the metrics available in the literature; metrics that can be used to help answer some questions.
8-1241	8	61	5	62	37	This is an interesting section as it does discuss sectorial contributions to radiative forcing. However the sectorial perspective does not give that context to discuss the full implication of anthropogenic climate change, as it is humans and societies that are responsible for the systems and structures that contribute to GHG emissions. Limiting the discussions to sectors, and thus technologies as the solutions, loses out an important part of the story. One cannot point at the sectors without pointing to individuals and chosen development pathways. This means that the discussions about humans as agents of change needs to come upfront somewhere and preferably already in the introduction. [Government of NORWAY]	Rejected. We relate the emissions and their effects to i) components and ii) sectors/sources. It is beyond the scope of WG1 Ch8 to go further back in the cause effect chain and discuss what drives human activity and behaviour. This may however be a topic that WGIII or SyR could cover. (And please note, we are not discussing technolgies as solutions; this is WGIII material).
8-1242	8	61	7	61	10	Some might argue that the use of NTCF is not appropriate since the definition includes CH4. The motivation is not WMGHGs vs NTCF. It is really co-located emissions for sectors. For WMGHGs, things just add globally. For short-lived, the local forcing may be cancelling as well. The authors should consider clarifying or elaborating upon this point. [Government of United States of America]	Taken into account. Given space restriction we could not discuss this very much, but text is changed.
8-1243	8	61	7			Lee et al. (2010) have also pointed out that model intercomparison studies with identical backgournd conditions show more similar results (their fig. 13). [Volker Grewe, Germany]	Noted.
8-1244	8	61	8	61	8	Insert reference to "Smith P, Nabuurs G-J, Janssens IA, Reis S, Marland G, Soussana JF, Christensen TR, Heath L, Apps M, Alexeyev V, Fang J, Gattuso J-P, Guerschman JP, Huang Y, Jobbagy E, Murdiyarso D, Ni J, Nobre A, Peng C, Walcroft A, Qiang S, Wang & Pan Y, Zhou GS (2008) Sectoral approaches to improve regional carbon budgets. Climatic Change 88,209–249, DOI 10.1007/s10584-007-9378-5" [Stefan Reis, United Kingdom of Great Britain & Northern Ireland]	Rejected. The text does not need references here as this is a general statement.
8-1245	8	61	12	61	14	When mentioning progress in undestanding, it would be worth also mentioning that Dahlmann et al. (2011) have investigated the change form individual sectors and - which is important- how much changes in emissions, chemistry anc radiation changes contribute to these RF changes. (Atmos. Environment) Surface and upper air emissions have a very different nature. [Volker Grewe, Germany]	Taken into account. Reference is added..
8-1246	8	61	12	61	14	E.g. between 1960 and 2010 an increase in upper air emissions lead to a more effective ozone production, whereas for surface emissions the effectiveness for ozone production was decreased (their figure 8). [Volker Grewe, Germany]	Accepted. Reference inserted.
8-1247	8	61	13	61	14	I suppose a reference to Unger et al. (2010) could be added here. [Terje Berntsen, Norway]	Accepted as suggested.
8-1248	8	61	21	61	30	The figures and discussion here are from one piece of research only which leaves the question of structural uncertainty rather open. Can you briefly say which models were used to estimate these temperature responses and whether this example should be treated as broadly representative of all other estimates (if there are any) because there is widespread consensus or whether this is just one of many, varied estimates. [European Union]	Partially taken into account: Unclear comment. Misplaced? If it is referred to the figures showing emissions weighted by metrics and teh sector figures: Documentation is given in the Supplementary Material.

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8-1249	8	61	25	61	25	Unbalanced) [Terje Berntsen, Norway]	Taken into account. Corrected.
8-1250	8	61	35			With respect to aviation NOx, Stevenson and Derwent (2009) found a strong dependence on background NOx, with low background NOx regions most sensitive. Whilst some dependence on latitude was also found, the background NOx was more important (equation 1 of that paper). [David Stevenson, United Kingdom]	Taken into account. Mentioned.
8-1251	8	61	37			Grewe and Stenke (2008), ACP 8, have provided a detailed analysis of regional differences in the impact of a NOx and H2O emission on RF-O3 and RF-CH4. [Volker Grewe, Germany]	Rejected. Unfortunately, due to space restrictions we have to limit our references and citations to a few studies only.
8-1252	8	61	50	61	52	Here, at least two recent studies (Righi et al., 2011, Peters et al., 2012) which investigated global aerosol indirect effects in detailed aerosol climate model simulations should be included. Both studies do not substantiate the very high indirect effect forcing estimates stemming from shipping emissions presented in Lauer et al. (2077). Righi et al. (2011) used the exact same model setup as Lauer et al. (2007), scaled the emissions to represent those of 2006 (2000 in Lauer et al. 2007) and found a maximum forcing of about -0.4 Wm-2, compared to -0.6 Wm-2 in Lauer et al (2007). [Karsten Peters, Australia]	Taken into account. Text modified and these studies are cited.
8-1253	8	61	50	61	52	Peters et al. (2012) used the global aerosol climate model ECHAM-HAM to investigate aerosol indirect effects stemming from shipping emissions and investigated the models' response towards uncertainties in the emissions themselves and their parameterisation in the model. Their results, corresponding to emission levels of the year 2000, suggest a maximum indirect forcing of -0.32 +- 0.01 Wm-2, i.e. about half of the upper estimate presented in Lauer et al. (2007). In both studies, the magnitude of the aerosol indirect effect from shipping emissions largely depends on the microphysical properties of the aerosol assumed at the point of emission (such as size distribution and/or hygroscopicity). [Karsten Peters, Australia]	Taken into account. Text modified and the studiy is cited.
8-1254	8	61	50	61	52	Righi, M., Klinger, C., Eyring, V., Hendricks, J., Lauer, A., and Petzold, A.: Climate Impact of Biofuels in Shipping: Global Model Studies of the Aerosol Indirect Effect, Environ. Sci. Technol., 45, 3519–3525, doi:10.1021/es1036157, 2011. [Karsten Peters, Australia]	Taken into account
8-1255	8	61	50	61	52	Peters, K., Stier, P., Quaas, J., and Graßl, H.: Aerosol indirect effects from shipping emissions: sensitivity studies with the global aerosol-climate model ECHAM-HAM, Atmos. Chem. Phys., 12, 5985-6007, doi:10.5194/acp-12-5985-2012, 2012. [Karsten Peters, Australia]	Taken into account
8-1256	8	61				It is unclear if indirect effect, particularly the aerosol indirect effects etc. are consistent with the other RFs in the chapter. Youshould maybe highlight where publcations differ. Bond et al. have updated BC GTPs and GWPs that may want discussion [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Checked for consistency. BC metrics from Bond et al. discussed.
8-1257	8	61				Impacts by sector. Delete in entirety. Does not belong in a forcing chapter. [Stephen E Schwartz, United States of America]	Rejected. Given by scoping.
8-1258	8	62	10		12	The meaning of this sentence wasn't clear to me. What is 'tagging'? What does 'perturbation response' mean here? [Nathan Gillett, Canada]	taken into account. Sentence deleted.
8-1259	8	62	26	62	30	This text needs to make clear if emission control measures are assumed to control SO2 and PM emissions for power plants and industry. If emissions control measures are assumed such as FGD and particle filters these sectors may not lead to cooling on the short run as suggested by the figure. This is important to get absolutely clear also for the technical summary since this may lead to the wrong policy conclusions. [European Union]	Rejected. We have used emission data from EDGAR and we could not go into the underlying factors controlling the emissions. Documentation is given in Supplementary Material.
8-1260	8	62	40	62	42	Please state the year on which the emissions pulse is based (I suggest use 2008), and the database from which the emission are assumed. Why is the envelope for Figure 8.33 different to that from Figure 8.32? It should be the same, and would then nicely illustrate how one can look at attributing emissions to gas or to sector, but the overall outcome remains the same. [Andy Reisinger, New Zealand]	Taken into account. Updated emission to most recent year possible
8-1261	8	62	45	62	46	To avoid any perception of bias, it would be useful to have Figure 8.34 produced for both a 20 year and 100 year time horizon, which would then nicely illustrate how the relative importance of different sectors changes. I realise that this in part duplicates informatio contained in Figure 8.33, but so does the current Figure 8.34; so adding a 100 year dimension would add value and not create a new problem. I don't know if Aamas et al 2012 contains a 100-year horizon, but it would be legitimate to create the 100-year figure simply using whatever	Taken into account

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						methodology they used in their paper. [Andy Reisinger, New Zealand]	
8-1262	8	62	50	62	50	I think the response to this FAQ is very nicely crafted. [Francis Zwiers, Canada]	Noted. Thanks
8-1263	8	62	50			FAQ 8.1: Overall we felt this FAQ reads wells, but is currently more in the style of a text book response, and could benefit from drawing upon the latest quantitative results coming from the Chapter 8 assessment. Quantitative evidence to support the text should be added where possible. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. We did not find any quantitative information from Chapter 8 that could be included here while keeping the text at the appropriate scientific level
8-1264	8	62	52	62	53	I suggest replacing "water vapour is an essential component of the Earth's climate" with "water vapour PLAYS AN ESSENTIAL ROLE IN the Earth's climate". [David Wratt, New Zealand]	Accepted
8-1265	8	62	52	62	56	FAQ 8.1: suggest to mention the distinction between natural and anthropogenic greenhouse effect to avoid confusion. We also suggest to clarify that the "largest contributor to the greenhouse effect" is referring to the natural greenhouse effect. While this is included in the text, it might not be obvious to the reader. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
8-1266	8	62	52			Since this is for non-scientists, perhaps preface 'greenhouse effect' with 'natural'. Many people will automatically assume that 'greenhouse effect' means 'anthropogenic greenhouse effect'. [Nathan Gillett, Canada]	Accepted
8-1267	8	62	53			Can one really say that water vapor in the atmosphere is mostly controlled by air temperature? Air temperature is certainly a factor influencing the carrying capacity of the atmosphere and the onset of condensation. But water vapour enters the atmosphere primarily from evaporation over the oceans, and sea-surface temperature is the key factor here. Indeed, in Chapter two (page 2-46, last line, to page 2-47, first line) it is noted that the interannual variability and longer-term trends in integrated water vapour are closely tied to changes in SST at the global scale. Total column water vapour correlates particularly well with the saturation specific humidity computed using sea-surface temperature. Some rewriting is needed here. [Adrian Simmons, United Kingdom]	Rejected. It is clear that air temperature controls water vapor in the atmosphere much more than sst. SST does not impose any vertical gradient in the air water vapor content while this gradient is huge
8-1268	8	62	54	52	54	I suggest replacing "For this reason , scientists consider it a feedback, rather than a forcing to climate change" with "For this reason, scientists consider CHANGES IN WATER VAPOUR CONCENTRATION TO BE a feedback rather than …".(Reason: It is the changes in water vapour concentration which constitute the feedback, not the water vapour itself). [David Wratt, New Zealand]	Taken into account. The sentence was changed to "For that reason, scientists consider it a feedback AGENT rather than a forcing to climate change.
8-1269	8	62		63		FAQ 8.1 Should aviation also be mentioned here as an anthropogenic source of water vapour? [Kate Willett, United Kingdom]	Rejected : Included in the "marginally through the combustion of fossil fuel."
8-1270	8	62				Evaluation of the impacts from the different sectors is confounded by the large changes in emissions of various pollutants which are underway at present. Emissions of NOx, PM, and VOCs (but not CO2) are declining rapidly from road transport and analyses can become outdated quickly (see Science, 327, 268, 2010 for a comment on this). Emissions of NOx, PM, and VOCs from road transport in 2010 were substantially smaller that those in 2000, and emissions in 2020 are very likely to be substantially smaller again than those in 2010. The same may be true for emissions in other sectors. A caveat recognizing the need to be mindful of such criteria emission trends in assessments of sector contributions in the future would be appropriate. [Timothy Wallington, United States of America]	Noted
8-1271	8	63	1	63	3	We understand that this statement is related to the natural greenhouse effect. If this is correct, the word "natural" should be added. [Government of Germany]	Accepted
8-1272	8	63	5	63	5	Rather than write “marginally through the combustion of fossil fuel” I would suggest writing “also through the combustion of fossil fuel, especially natural gas." As argued forcefully in this FAQ 8.1 response, these “injections” or emissions of water vapor are insignificant with regard to atmospheric water vapor content, but some readers (not only those in bad faith) may find “marginally” hard to swallow if one considers that burning CH4 produces two H2O molecules for each CO2 molecule released into the atmosphere. [Robert Kandel, France]	Rejected. Marginally is important to clearly state that the flux generated by fossil fuel burning is small compared to that of the other processes;
8-1273	8	63	12	63	13	The anthropogenic CO2 flux into the atmosphere is also relatively small compared to natural fluxes, so I think	Taken into account. The fact that anthropogenic water

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						there is a need to explain a bit more clearly why water vapour does not accumulate like CO2 (the answer is not just that the anthropogenic flux is small). [Francis Zwiers, Canada]	vapor emission does not impact significantly the concentration is due to both facts -natural flux small compared to anthropogenic, and small residence time
8-1274	8	63	18	63	19	This FAQ is very well written. Only one small clarification is sought. It's not clear from these lines and even this whole paragraph whether stratospheric water vapour is considered an anthropogenic or natural forcing agent. Figure 8.17 identifies it as an anthropogenic agent through its formation from anthropogenic methane oxidation. This information should be added here. [Government of Canada]	Accepted "water vapour in the stratosphere (above 10 km altitude) is considered to be a radiative forcing agent, with measurable impacts on the greenhouse effect changes"
8-1275	8	63	19			After 'forcing agent' insert 'since it has a long residence time and is directly enhanced by anthropogenic activities'. [Nathan Gillett, Canada]	Rejected. This is explained in the following sentences and we do not want to make this initial sentence too long
8-1276	8	63	26	63	27	I think this could be made a bit more accessible to lay readers. Suggest replacing the sentence that begins "A typical polar air atmospheric column" with "A typical 'column' of air extending from the surface to the stratosphere in polar regions may contain only a few kilograms of water vapour per square meter, while a similar column of air in the tropics may contain up to 100 kilograms." [Francis Zwiers, Canada]	Accepted
8-1277	8	63	26			Air does not hold water vapor; the equilibrium vapor pressure of water is achieved (at equilibriium) whether or not air is present. [Stephen E Schwartz, United States of America]	Taken into account
8-1278	8	63	26			Craig Bohren hates this phrase ('warmer air holds more water vapour') and I tend to agree with him that we should try and eradicate it. [David Stevenson, United Kingdom]	Taken into account
8-1279	8	63	27	63	27	"a tropical air mass might hold up to 100 kilograms". This value sounds very extreme (has it ever been measured)? 50 kg would be more typical. [Räisänen Petri, Finland]	Taken into account. 100 is to large. 50 is too low. May be changed to 70 or 75
8-1280	8	63	29	63	30	The bit in parentheses I think would only help to confuse lay readers, so I think it might be best to delete it. [Francis Zwiers, Canada]	Accepted
8-1281	8	63	29		30	Delete '(with less precipitation than evaporation during the transition period)'. This must be a very small term compared to the rate of precip and the rate of evaporation, so I don't think it's worth mentioning. Also, I'm sure this is also small compared to the variability in atmospheric water vapour. [Nathan Gillett, Canada]	Accepted
8-1282	8	63	30	53	30	Replace "…and therefore to more warming" with "and therefore LEADS to more warming". [David Wratt, New Zealand]	Accepted
8-1283	8	63	30	63	30	And therefore leads to more warming. [Robert Kandel, France]	Accepted
8-1284	8	63	30	63	30	Add "leads" in "and therefore leads to more warming" [Urs Neu, Switzerland]	Accepted
8-1285	8	63	30			ppor wording with "too more warming" [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted
8-1286	8	63	31	63	31	I suggest replacing "It is included" with "It occurs". I assume that this is a consequence of other processes that are explicitly modelled, and not something that is explicitly included as a process. [Francis Zwiers, Canada]	Accepted
8-1287	8	63	34			cannot is too strong here - you mean within the ipcc forcing/feedback framework [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Taken into account. changed to "should not"
8-1288	8	63	42		45	The phrasing here could be improved. [Nathan Gillett, Canada]	Taken into account
8-1289	8	63	43	63	44	It is stated that "water vapour [feedback] amplifies any initial forcing by a typical factor of three". I believe "factor of three" is somewhat exaggeration. Taking the results of IPCC AR4 models analyzed by Soden and Held (2006) (J. Climate, 19, 3354-3360) as guidance, the mean value of Planck feedback is -3.21 Wm-2 K-1, and the mean of water vapor feedback 1.80 W m-2 K-1. From this one can infer that the water vapor feedback would increase warming typically by a factor of 3.21 / (3.21-1.80) = 2.3, with values of 1.9...2.9 for the individual models. Thus, unless new information has surfaced that shows that the WV feedback is stronger than that for AR4 models, factor of three is too much. It is important to get this kind of things right. [Räisänen Petri, Finland]	Taken into account. The values depend on the processes that are included. We nevertheless agree that 3 indicates little uncertainty. We have changed the formulation to "between 2 and 3'

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8-1290	8	63	44	63	44	"factor of three" should be "factor of two to three" to be consistent with line 3 [Manoj Joshi, United Kingdom of Great Britain & Northern Ireland]	Rejected. Line 3 refers to the current levels. Here we discuss the amplification factor. Different things
8-1291	8	63	44			How do you arrive at a particular ratio (of 3)? You are comparing the W m-2 K-1 from water vapour with the climate feedback parameter (W m-2 K-1) that applies in the absence of water vapour. The latter depends on what processes are included. It's rather arbitrary, I would say. [Jonathan Gregory, United Kingdom]	Accepted. See 8-1292
8-1292	8	63	47			FAQ 8.1, Figure 1: In general we would prefer not to combine schematic and quantitative figures into a single figure, however, for an FAQ this may be a reasonable approach. Nevertheless, some further visual separation of the quantitative (i.e., The graph of the increase in water vapour) from the non-quantitative part of the figure would be useful. For example, adding a white background to the graph, and labelling this as 'a', and the schematic as 'b'. The caption can then more clearly refer to the two components of the figure. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Figure has been modified as suggested, and we also expanded the caption for a better description of the figure.
8-1293	8	63	55	65	5	I am uneasy about this section. It misses the point that the committed warming from emissions of long-lived substances such as CO2 are independent of any cooling influence of transient substances such as aerosols. The aerosol cooling is hiding the problem; it is last week's aerosol (precursor) emissions that are negating the warming influence of some decades worth of CO2 emissions. This is the Faustian bargain. Reducing emissions of aerosol (precursors) does not make the warming influence of prior CO2 emissions worse, it just reveals it. This does not come through in the FAQ. [Stephen E Schwartz, United States of America]	Taken into account. the text explicitly states uses the phrasing "exacerbate warming" to reflect the point made by the reviewer.
8-1294	8	63	55			FAQ 8.2: A summary could be included in the TS for better understanding of the relationship between pollutants and climate change. [Government of Japan]	Noted
8-1295	8	63	55			FAQ 8.2: This FAQ response is primarily qualitative, and could benefit from drawing upon the latest quantitative results coming from the Chapter 8 assessment. Quantitative evidence to support the text should be added where possible, and should also form the basis for a more substantial and useful chapeau. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: to the chapeau were made, as suggested by another reviewer
8-1296	8	63	55			FAQ 8.2: Care needed with language to avoid staying outside the WGI mandate, egg, lines 27 - 43: "It is unclear if developing countries will curb sulphur emissions...." [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: sentence is removed
8-1297	8	63	55			FAQ 8.2: Figure 1: This figure is currently not called out in the text. The figure should be firmly embedded into the text. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: done
8-1298	8	63	57	63	57	In comparison to other FAQs, an extension of the lead paragraph is recommended: Add e.g. "While e.g. a reduction of sulphur dioxide emissions generally leads to more warming, lowering of black carbon emissions rather has a cooling effect. Nitrogen oxide emission control has both a cooling (through reducing of tropospheric ozone) and a warming effect (due to its impact on methane lifetime and aerosol production)." [Urs Neu, Switzerland]	Accepted: added
8-1299	8	64	2	64	56	This box needs to mention co-emitted pollutants. This means that the net effect depends on the source, and assumed aerosol forcings, particularly indirect forcings, which are quite uncertain. For some source sectors the impact of pollutant controls is fairly certain (e.g. diesel vehicles) while for others are much more uncertain, including even the sign of the net effect (e.g., residential biomass). [Steven Smith, United States of America]	Accepted: this is now explicitly mentioned
8-1300	8	64	3	64	3	Aerosols ARE airborne solid or liquid particles therefore as written this sentence is confusing. [Government of Canada]	Accepted: text changed
8-1301	8	64	3	64	3	Suggest replacing "aerosols, and … or PM" with "and aerosols, including solid or liquid particulate matter, or PM". (Isn't PM normally considered to a constituent of aerosols). [Francis Zwiers, Canada]	Accepted: text changed
8-1302	8	64	4	64	5	Maybe better to say "many countries" than to say "most major urban centres" - certainly where I live, the implementation and enforcement of AQ regulations is not a municipal responsibility. [Francis Zwiers, Canada]	Rejected: the text is left unchanged as it is still the goal of the urban centre to control pollution, regardless of how the decisions is ultimately made
8-1303	8	64	6			Say here at the start that many of these pollutants have significant effects on climate, some acting to warm the climate and others to cool it. [Nathan Gillett, Canada]	Accepted: done

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8-1304	8	64	7	64	10	This paragraph should start by explaining why (some) air pollutants affect climate. Suggest text such as "Some air pollutants are also radiatively active, meaning they interact with shortwave solar radiation and/or longwave radiation either directly or indirectly. In this way, as with other radiative forcing agents, they impact on climate. Of the common air pollutants listed above, tropospheric ozone and aerosols are of particular concern for their impacts on climate." [Government of Canada]	Rejected: This is done later on in the text.
8-1305	8	64	7	64	10	Tropospheric ozone is also produced downwind of sources from PAN decomposition. Therefore, ozone produced locally or downwind over emission regions can impact ozone levels over downwind receptor regions. HTAP (2010) estimated that 30-40% of ozone is imported from outside a particular region. In addition, ozone RF is not local to the emissions, it is maximum over regions with warm surface temperatures and cold tropopause temperatures. This text needs revising. [Katharine Law, France]	Accepted: added usually
8-1306	8	64	9	64	9	What does 'potent' in this context mean? It sounds like referring to their potency as air pollutants but the rest of the sentence refers to their RF effects. Suggest replacing 'are most potent" with "have highest atmospheric concentrations". [Government of Canada]	Rejected: we want to bring forward that this is more than just concentrations but also impact. Will keep potent
8-1307	8	64	9	64	9	Change "These pollutants are most potent near their point of origin" to "These pollutants are *usually* most potent near their point of origin." [Loretta Mickley, United States of America]	Accepted: added
8-1308	8	64	9	64	10	I think words that can be easily confounded in meaning (it is never clear whether signficant means statistically significant, should be avoided. Also the concept of radiative forcing is technically challenging for lay readers. Perhaps this can be simplified to be more lay reader friendly. Here is a suggestion: "These pollutants are most potent near their points of origin, which can cause quite substantial local or regional effects on climate, even if the globally averaged effect is small." [Francis Zwiers, Canada]	Accepted: text changed
8-1309	8	64	12	64	12	Suggest inserting "some" ahead of "consequences", since the previous sentence points out that forcing may be localized. [Francis Zwiers, Canada]	Accepted: added
8-1310	8	64	12	64	14	Models do include a number of the important air pollutants that affect climate so this sentence is misleading. Suggest rewriting as ".. some couplings between these short-lived pollutants and climate are still poorly understood, quantified and modeled, which makes it difficult to fully understand and project those consequences." [Government of Canada]	Accepted: statement is replaced
8-1311	8	64	13	64	14	Suggest replacing "some climate models do not even include them" with "and they are not included in all climate models". The emphasis that is given by "even" I think casts unneccessary doubt on climate models. [Francis Zwiers, Canada]	Accepted: statement is replaced
8-1312	8	64	16	64	20	The first sentence here sets up well the paragraph to talk about the importance of composition. Then ozone should be defined as a GHG since this whole FAQ really focuses on ozone and aerosols. Suggest saying "Ozone, as a GHG, will primarily impact climate through radiation, while aerosols affect radiation transfer and can also affect climate....". Suggest also that the basic division between scattering and absorbing aerosols be described and then it could be mentioned how this simple division is made complicated by 'aging' of aerosols in the atmosphere when they are mixed with other atmospheric pollutants and constituents. Policymakers are becoming familiar with these concepts and this FAQ should help strengthen that understanding. Finally, the specific information about sulphate particles could be merged with the paragraph(s) below that also are specific to sulphur. [Government of Canada]	Rejected: There is enough information in the chapter on the role of ozone as a GHG.
8-1313	8	64	22	64	25	This paragraph could add a couple sentences to explain more clearly how scrubbers "might exacerbate global warming" due to how they work and how they may make the power plant less efficient. Or, because they reduce SO_2 emissions, which reduce sulfate aerosol formation in the atmosphere. And since sulfate aerosols have a negative RF (i.e., they cool the planet), scrubbers may have the unintended climate effect of exacerbating warming, despite the the well-known environmental (i.e., acid rain) and public health (i.e., PM) benefits. The authors might consider clarifying this point. [Government of United States of America]	Rejected: this is mentioned in paragraph #6
8-1314	8	64	22		23	Lu et al. (2011) show progressive decreases in SO_2 emissions from China since 2006. Has there really been an upward trend in SO_2 observed over the past decade? (I imagine this is quite sensitive to the start and end dates).Lu, Z., Q. Zhang, and D. Streets (2011), Sulfur dioxide and primary carbonaceous aerosol emissions in china and india, 1996–2010, Atmos. Chem. Phys., 11 (18), 9839–9864. [Nathan Gillett, Canada]	Rejected: The statement is over "last few decades" for which the staement is correct

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8-1315	8	64	24	64	25	Delete subjective wording such as "the inherent irony". [Government of Canada]	Accepted: removed
8-1316	8	64	24			irony is not really appropriate here think - you only mean offsetting in terms of globally averaged climate warming here and UNFCCC talks about dangerous interference. Futhermore it doesn't square with earlier discussion that says there is no rebound of aerosol forcing due to geographic redistribution [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted: removed
8-1317	8	64	27	64	27	This statement "It is unclear whether developing countries will curb sulphur emissions to the same extent as …" is misleading. Evidence to date (e.g., Smith et al. 2005, Klimont et al 2012) actually indicates quite strongly that developing countries are controlling SO2 emissions at a much faster rate, relative to income, than developed countries. Smith, Steven J., Pitcher, H., and Wigley, T.M.L. (2005) Future Sulfur Dioxide Emissions. Climatic Change 73(3) pp. 267-318. [Steven Smith, United States of America]	Accepted: sentence removed
8-1318	8	64	27	64	28	No information has been presented yet to support this sentence. Need to first describe how the U.S. and Europe have curbed sulphur emissions since the 1970s. Also, why single out the U.S.? At least be consistent and refer to regions (e.g. North America and Europe). [Government of Canada]	Accepted: sentence removed
8-1319	8	64	32	64	32	I suggest: “has likely caused”. [Lazaros Oreopoulos, United States of America]	Accepted: added
8-1320	8	64	32	64	34	see also Junkermann et al 2011 The climate penalty of clean fossil fuel combustion. Atm Chem Phys 11, 12917-12924 [European Union]	Rejected: interesting reference but no work is supposed to be cited in an FAQ
8-1321	8	64	32	64	35	"Hydrophilic aerosols" will need explaining. The statement that anthropogenic emissions (of aerosols) rose sharply during the second half of the 20th century requires some support, especially given the statement in the paragraph above about declines in emissions from the U.S. and Europe. [Government of Canada]	standard use of hydrophilic: ignored. Ch 8 has extensive documentation of emission changes: ignored.
8-1322	8	64	35	64	35	We propose to replace the phrase "may stimulate more warming" by "may have the consequence of more warming", for example. [Government of Germany]	Accepted: statement has been removed
8-1323	8	64	37	64	39	We propose to mention the possible impact of BC on clouds which is very uncertain and could even counteract the explained cooling effect due to BC reductions. [Government of Germany]	Accepted: this is the reason for the dashed arrow on BC.
8-1324	8	64	37	64	40	This very short paragraph about BC is wholly inadequate for this topic. It is very oversimplistic. The various ways BC can impact climate should be mentioned, along with their uncertainties. BC will need to be defined also, at the beginning of this paragraph as this is the first mention of the substance. [Government of Canada]	Rejected: the FAQ is not the place for a lengthy discussion of BC. This is done in Chapter 7.
8-1325	8	64	37			I didn't understand the blackcarbon arrows - thay are not that well explained [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Accepted: We have added a sentence on interactions with clouds to explain the second arrow.
8-1326	8	64	41	64	49	This paragraph needs to begin by first explaining that tropospheric ozone is not directly emitted but is formed in the atmosphere from precursor emissions. This is implied in the first sentence, but should be made explicit. [Government of Canada]	Rejected: ozone is thoroughly discussed in chapter.
8-1327	8	64	45	64	46	This will be hard for lay readers to understand - would the following work as a replacement for the part of the sentence beginning with "because": "because of chemical reactions between different compounds in the emissions from the targetted sector, and possibly also emissions from other sectors, which produce 'secondary' pollutants." [Francis Zwiers, Canada]	Accepted: text was changed by adding "co-emitted"
8-1328	8	64	51	64	54	This sentence suggests that an increase in regional mean temperatures could worsen ozone pollution. In fact, what drive ozone pollution over at least the Northeast US (and likely elsewhere in midlatitudes) is the frequency and duration of stagnation events (Leibensperger et al., 2008). These stagnation events are associated with high daily maximum temperatures. Leibensperger, E. M., L. J. Mickley, D. J. Jacob, Sensitivity of U.S. air quality to mid-latitude cyclone frequency and implications of 1980-2006 climate change, Atmos. Chem. Phys., 8, 7075-7086, 2008. [Loretta Mickley, United States of America]	Accepted: sentence removed
8-1329	8	64	53			The text should make clear that the effect described applies to tropospheric ozone only. [Nathan Gillett, Canada]	Accepted: done
8-1330	8	64	56	64	56	Should this not say "climate impact on pollution" rather than "pollutant emissions on climate"? This whole	Accepted: text rewritten

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						paragraph is about the impacts of climate change on pollution. [Government of Canada]	
8-1331	8	64		64		This FAQ is not well written currently and requires some significant rewriting. The topic is a good one for an FAQ and will be of considerable interest to governments, which makes it all the more important to improve the content. The Figure associated with this FAQ (FAQ8.2 Figure 1) is not currently referenced anywhere in the text for this FAQ. It looks to be a very useful Figure but will need supporting text that doesn't gloss over the uncertainties. [Government of Canada]	Noted: Text was changed to improved readability and Figure is referenced in the text. Uncertain processes are shown as dashed arrows.
8-1332	8	65	1	80	1	Several citations are missing eg WMO 2011 and UNEP 2011. It should be corrected by the editors. [Pieter Aucamp, South Africa]	Accepted, corrected
8-1333	8	65	5	65	5	Suggestion for one frequently asked question is simply: "How are human activities influencing the climate system? or more simply "How are humans changing the climate and how can we be sure? [Government of NORWAY]	Rejected, it is not the chapter team that decide the FAQs and decision made for long time ago
8-1334	8	65	6	65	6	Please add a FAQ: "What emission reductions would most effectively curb Arctic melting?" [Government of NORWAY]	Rejected, see comment 8-1333
8-1335	8	66	54			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process.
8-1336	8	67	28			The following article may be cited since it attempted to investigate the ability of RCMs to reproduce observed relationships between precipitation and elevation: Yasuhiro Ishizaki, Toshiyuki Nakaegawa, Izuru Takayabu 2012. Validation of precipitation over Japan during 1985–2004 simulated by three regional climate models and two multi-model ensemble means. Climate Dynamics. DOI: 10.1007/s00382-012-1304-5 [Tosiyuki Nakaegawa, Japan]	Rejected, not relevant for the forcing chapter
8-1337	8	67	48			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1338	8	68	13			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1339	8	68	17			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1340	8	68	36			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1341	8	69	1			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1342	8	70	39			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1343	8	70	48			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1344	8	71	29	71	30	the ending page number for this reference is missing. [Lin Huang, Canada]	taken into accout, corrected
8-1345	8	71	29			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1346	8	71	30			Last character missing [Indrani Roy, U.K.]	taken into accout, corrected
8-1347	8	72	56			Page numbers missing (check all the reference -there are many references in this chapter without page numbers) [Indrani Roy, U.K.]	Taken into account, references are corrected with page numbers or article number
8-1348	8	72	58			Page numbers missing [Indrani Roy, U.K.]	Taken into account, references are corrected with page numbers or article number

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8-1349	8	73	1			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1350	8	73	50			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference.
8-1351	8	74	17			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1352	8	76	43			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1353	8	77	17			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1354	8	77	20			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1355	8	77	31			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1356	8	79	51			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1357	8	80	18			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1358	8	80	30			Reference shown in Capital [Indrani Roy, U.K.]	Taken in to account: We will decapitalize the reference during the editoral process..
8-1359	8	82	1	82	11	It looks as if the Table is not in final form. The label HFC-1243zf should be HFO-1243zf and there are several fluoroalkenes in the Table that need names. CF3CF=CHF is HFO-1225ye. The naming of the RfCH=CH2 isomers is complicated when there are 4 or more carbons in the molecule. For example C2F5CH=CH2 is listed as HFO-1345zfc: this could also be labelled as HFO-1345zf or HFO-1345mc. When you get to 1-pentenes and larger you have to consider the possibility of branched isomers. For example, the naming of C4F9CH=CH2 depends on whether the isomer is linear i.e CF3CF2CF2CH=CH2 or branched i.e. CF3CF(CF3)CH=CH2. [Robert Waterland, United States of America]	Taken into account. The table has been improved since SOD with several names and acronyms added.
8-1360	8	82	1	84	23	I do not see the need for the AGWP and AGTP columns. Just give the AGWP's and AGTP's of CO2 in a footnote and the rest are trivial to compute (for the few that want them). This would greatly simplify the table and not reduce the information in any way. [Robert Portmann, United States of America]	Rejected. We give absolute metrics to obtain more transparency and reduce the dependecy on changes related to the reference gas.
8-1361	8	82	3			Table 8.A.1 provides GWPs which are different from those in AR4. The reasoning behind these changes is not clearly explained. [Government of Australia]	Taken into account. Discussed to the extent we space allowed.
8-1362	8	82	3			Table 8.A.1 provides two GWPs for methane - one for fossil methane and another for non-fossil methane. Additionally, the fossil methane GWP is not a fixed value, but a range. This is not explained and will potentially be confusing for policy-makers using GWPs in accounting for emissions. If these new GWPs are to be adopted, guidance should be provided somewhere in the report on how to distinguish between fossil and non-fossil methane sources, as well as how to select an appropriate value from the range provided for fossil methane. [Government of Australia]	Taken into account. Range not given now, but uncertainty range is discussed in footnote.
8-1363	8	82	9	84	1	Table 8.A.1. Many of the values for atmospheric lifetime and radiative efficiency for halocarbons in Table 8.A.1 have changed rather significantly from the Fourth Assessment Report. However, the values are cited as being taken from a yet to be published reference (Hodnebrog et al., 2012). As a result, reviewers are not able to verify the source the atmospheric lifetimes and radiative efficiency values for each compound. As in the Fourth Assessment Report, it is likely that some of these values are estimated or assumed. Such estimated or calculated values should be delineated from those which are based upon experimental measurements. [John Owens, United States of America]	Taken into account. See Supplementary Material.

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8-1364	8	82		83		The numbers in this table are far too small. A rework for a better visualisation is proposed. [Government of Germany]	taken into account. Table improved.
8-1365	8	82				Table 8.A.1: The reference for the data contained in this table is a review (Hodnebrog et al, 2012) that has been submitted but is apparently not yet available on the publisher's website, not even in "early view" form. Despite lack of access to this review, some general comments on Table 8.A.1 will be provided below. [James Franklin, Belgium]	Noted
8-1366	8	82				Table 8.A.1: GWPs and other metrics have been rounded off to the nearest unit, leading to "zero" values in some cases (in which the estimated GWP is presumably < 0.5). Although the contribution of these gases to radiative forcing may indeed be negligible compared to the overall forcing due to all souces, the notion that their GWP is "zero" is misleading and should be avoided. Indeed, for a compound such as the commercially significant HFO-1234yf (CF3CF=CH2), which breaks down in the atmosphere predominantly to 1 mole each of CF3COF and HCHO (Luecken et al, Environ. Sci. Technol 44, 343-348, 2010), the CO2 formed by further oxidation of the HCHO would, by itself alone, contribute about 0.4 to the GWP value. Furthermore, the updated 100-year GWP (of "zero") for the parent compound is surprising, given the previously published values of about 4 (Nielsen et al, Chem. Phys. Lett. 439, 18-22, 2007; Orkin et al, J. Phys. Chem. 114, 5967-5979, 2010), but the explanation for this change (mainly due to a revised radiative efficiency) will presumably be found in the currently unavailable review by Hodnebrog et al, 2012. [James Franklin, Belgium]	Taken into account. Although the GWP values in some cases round to zero, the AGWPs are given with 3 significant digits in all cases. Hence, an accurate GWP value can be derived based on the AGWP of the halocarbon and the AGWP of CO2. Neither Nielsen et al. (2007) nor Orkin et al. (2010) took into account the non-homogeneous atmospheric mixing of the HFO-1234yf compound, while this is taken into account here and leads to a considerably lower RE and GWP.
8-1367	8	82				Table 8.A.1: Analogous comments to the above can be made for trans-HFO-1234ze (trans-, or E-, CF3CH=CHF), also of commercial importance. The degradation of 1 mole of this compound could potentially lead to up to 3 moles of CO2 (Javadi et al, Atmos. Chem. Phys. 8, 3141-3147, 2008), which would make a contribution of about 1.2 to the 100-year GWP value. Furthermore, the parent-compound 100-year GWP was previously estimated to be about 6 - 7.5 (Sondergaard et al, Chem. Phys. Lett. 443, 199-204, 2007; Orkin et al, J. Phys. Chem. 114, 5967-5979, 2010), while it is listed as "1" in Table 8.A.1. [James Franklin, Belgium]	Taken into account. Same answer as above; the non-homogeneous atmospheric mixing of HFO-1234ze was not taken into account in Søndergaard et al. (2007) and Orkin et al. (2010).
8-1368	8	82				Table 8.A.1: The missing "industrial designations" ("ID") for the Z and E isomers of CF3CF=CHF are HFO-1225ye(Z) and HFO-1225ye(E) [James Franklin, Belgium]	Taken into account
8-1369	8	82				Table 8.A.1: The missing ID for the Z (or cis) isomer of CF3CH=CHF is HFO-1234ze(Z). Note that this compound was previously assigned a 100-year GWP of about 3 (Nilsson et al, Chem. Phys. Lett. 473, 233-237, 2009), so the revision to "zero" appears surprising. [James Franklin, Belgium]	Taken into account. Missing ID taken into account. Nilsson et al. (2009) did not account for non-homogeneous mixing in the atmosphere, and this leads to a considerably lower RE and GWP.
8-1370	8	82				Table 8.A.1: The missing IDs for CHCl3, CH2ClCH2Cl and CH2Br2 are respectively chloroform, 1,2-dichloroethane, and methylene bromide. [James Franklin, Belgium]	Taken into account.
8-1371	8	82				It is not clear why no lifetime is given here for CO2? The authors should consider including one. [Government of United States of America]	Rejected. There is not a single lifetime that can be given for CO2. The footnote explains what is used instead. See discussion in 8.7 and chapter 6.
8-1372	8	82				Table 8.A.1 It is unclear from the discussion whether the cited lifetime for CH4 is the pulse decay lifetime or the lifetime calculated using the instantaneous removal rate in the present day atmosphere. The current text in line 14 - 21 on p. 16 is misleading and should be rewritten. To be consistent, the pulse decay lifetime should be used in the GWP and GTP definitions. [Government of United States of America]	Taken into account.
8-1373	8	82				Table 8.A.1 The text on p. 16, line 46 suggested that this report recommends using 131 years for the lifetime of N2O. However, Table 8A.1 still has 121 years. This numbers need to be reconciled. [Government of United States of America]	Taken into account. Text will be clarified and numbers reconciled.
8-1374	8	82				Table 8.A.1 Putting the very short-lived species with the WMGHGs in the same table could be very misleading. Are their lifetimes calculated using a model assuming a certain emission pattern? What spatial distribution is used to calculate the radiative efficiency? It might be more prudent for the authors to put them in a separate table and explain. [Government of United States of America]	Taken into account. Caption improved and discsussion of impact of short lifetimes inlcuded in Supplementary Material.
8-1375	8	82				Table 8.A.1. It is important to update the lifetimes to reflect recent publications and more thorough analysis/ propagations of uncertainties. We all need to coordinate in the values we are using for the budget lifetimes in	Taken into account for N2O and CH4.

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						Chapters 5, 6, 8, and 11. The March 2012 paper (Prather etal, GRL 39: L09803) gives updated values: CH4 lifetime is 9.1+-0.9 y, perturbation lifetime (residence/feedback) is 12.4+-1.4 y, N2O lifetime is 131+-10y, with perturb. lifetime = 121 +-10 y, HFC-134a lifetime (only 1 value) is 14.2 +- 1.5 y. If there are better/more recent values then include and merge. The uncertainty in lifetime needs to be propagated to the other quantities in the table. Clearly the small ranges in GWP, AGWP, GTP for CH4 are not realistic. [Michael Prather, United States of America]	
8-1376	8	82				I am really pleased to see this table in the appendix. I like the inclusion of AGWP's and radiative efficiencies. This will go a long way to introducing the AGWP concept and get people thinking about this. Also radiative efficiencies. Get rid of anything having to do with temperature. This is a forcing chapter, not a response chapter. [Stephen E Schwartz, United States of America]	Noted. Suggestion about GTP is rejected.
8-1377	8	83	1			Table 8.A.1. The designation "HFE-7000" is a commercial tradename. The material should be referred to by its halocarbon designation HFE-347mcc. [John Owens, United States of America]	Taken into account (HFE-7000 is only given in parentheses).
8-1378	8	83	1			Table 8.A.1. The designation "HFE-7100" is a commercial tradename. The material should be referred to by its halocarbon designation HFE-449s1 (note that this ends with the number one not the letter L). [John Owens, United States of America]	Taken into account (HFE-7100 is only given in parentheses).
8-1379	8	83	1			Table 8.A.1. The designation "HFE-7200" is a commercial tradename. The material should be referred to by its halocarbon designation HFE-569sf2. [John Owens, United States of America]	Taken into account (HFE-7200 is only given in parentheses).
8-1380	8	83	1			Table 8.A.1. The Radiative Efficiency value for HFE-449s1 is incorrect. The value from 4AR is 0.31 Wm(-2)ppbv(-1) which is confirmed in a more recent publication: I. Bravo, Y. Díaz-de-Mera, A. Aranda, K. Smith, K. P. Shine and G. Marston, Atmospheric chemistry of C4F9OC2H5 (HFE-7200), C4F9OCH3 (HFE-7100), C3F7OCH3 (HFE-7000) and C3F7CH2OH: temperature dependence of the kinetics of their reactions with OH radicals, atmospheric lifetimes and global warming potentials, Phys. Chem. Chem. Phys., 2010, 12, 5115–5125. [John Owens, United States of America]	Rejected. The main reason for the higher RE here is the lifetime correction method applied.
8-1381	8	83	1			Table 8.A.1. The atmospheric lifetime value for HFE-347mcc is incorrect. The correct value should be 4.4 years which is the average of the values as measured in (1) K. Tokuhashi, A. Takahashi, M. Kaise, S. Kondo, A. Sekiya, S. Yamashita and H. Ito, Int. J. Chem. Kinet., 1999, 31, 846-853., (2) Y. Ninomiya, M. Kawasaki, A. Guschin, L. T. Molina, M. J. Molina and T. J. Wallington, Environ. Sci. Technol., 2000, 34, 2973-2978., and (3) I. Bravo, Y. Díaz-de-Mera, A. Aranda, K. Smith, K. P. Shine and G. Marston, Phys. Chem. Chem. Phys., 2010, 12, 5115–5125. [John Owens, United States of America]	Rejected. The lifetime of 5.0 years is taken from the most recent WMO assessment report from 2010.
8-1382	8	83	1			Table 8.A.1. The atmospheric lifetime value for HFE-449s1 is incorrect. The correct value should be 3.8 years which is the average of the values as measured in (1) N. Oyaro and C. Nielsen, Asian Chem. Lett., 2003, 7, 119-122. and (2) I. Bravo, Y. Díaz-de-Mera, A. Aranda, K. Smith, K. P. Shine and G. Marston, Phys. Chem. Chem. Phys., 2010, 12, 5115–5125. These are the published studies that measured the OH rate constant on the commercial mixture of isomers. [John Owens, United States of America]	rejected.The lifetime of 4.7 years is taken from the most recent WMO assessment report from 2010.
8-1383	8	83	1			Table 8.A.1. The atmospheric lifetime value for HFE-569sf2 is incorrect. The correct value should be 0.71 years which is the average of the values as measured in (1) L. K. Christensen, J. Sehested, O. J. Nielsen, M. Bilde, T. J. Wallington, A. Guschin, L. T. Molina and M. J. Molina, J. Phys. Chem. A, 1998, 102, 4839-4845., (2) N. Oyaro and C. Nielsen, Asian Chem. Lett., 2003, 7, 119-122. and (3) I. Bravo, Y. Díaz-de-Mera, A. Aranda, K. Smith, K. P. Shine and G. Marston, Phys. Chem. Chem. Phys., 2010, 12, 5115–5125., These are the published studies that measured the OH rate constant on the commercial mixture of isomers. [John Owens, United States of America]	Rejected. The lifetime of 0.8 years is taken from the most recent WMO assessment report from 2010.
8-1384	8	83				Table 8.A.1: On page 83, some of the chemical formulas are truncated to the right, since the column is not broad enough. This is also the case for one of the common names. [James Franklin, Belgium]	Taken into account. Table has been improved.
8-1385	8	83				Table 8.A.1: For the hydrofluoroethers (HFEs), some corrected designations are provided below, according to this reviewer's interpretation of ASHRAE standard 34-2010, which should be considered authoritative in this respect. [James Franklin, Belgium]	Taken into account. To the extent possible, the names and acronyms listed here are chosen to be the same as in previous IPCC and WMO assessments, and/or to follow the CAS (Chemical Abstracts Service)

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							standard.
8-1386	8	83				Table 8.A.1: The use of "HG" or "H-Galden" designations should be avoided, since they are derived from a registered trade mark. [James Franklin, Belgium]	Rejected. We choose to keep the HG designation for compounds where the acronym is missing.
8-1387	8	83				Table 8.A.1: CF3CHFOCF3 is HFE-227ea1 [James Franklin, Belgium]	Rejected. But compound is now listed as HFE-227ea.
8-1388	8	83				Table 8.A.1: CHF2OCHFCF3 (desflurane) is HFE-236ea2 [James Franklin, Belgium]	Taken into account.
8-1389	8	83				Table 8.A.1: CF3CH2OCF3 is HFE-236fa1 [James Franklin, Belgium]	Rejected. But compound is now listed as HFE-236fa (from CAS).
8-1390	8	83				Table 8.A.1: CF3OCH2CH3 is HFE-263fb1 [James Franklin, Belgium]	Rejected. The correct acronym is HFE-263fb2
8-1391	8	83				Table 8.A.1: CHF2CF2OCF2CF3 is HFE-329p2 [James Franklin, Belgium]	Rejected. Compound is listed as HFE-329mcc2 (as in AR4).
8-1392	8	83				Table 8.A.1: (CF3)2CHOCHF2 is HFE-338mmz2 [James Franklin, Belgium]	Rejected. Compound is now listed as HFE-338mmz1 (from US EPA).
8-1393	8	83				Table 8.A.1: (CF3)2CHOCH2F (sevoflurane) is HFE-347mmz2 [James Franklin, Belgium]	Rejected. Compound is listed as HFE-347mmz1.
8-1394	8	83				Table 8.A.1: CH3OCF2CF2CF3 ("HFE-7000") is HFE-347s3 [James Franklin, Belgium]	Rejected. Compound is listed as HFE-347mcc3 (as in AR4).
8-1395	8	83				Table 8.A.1: CHF2CH2OCF2CF3 is HFE-347pf2 [James Franklin, Belgium]	Rejected. Compound is listed as HFE-347mcf2 (as in AR4).
8-1396	8	83				Table 8.A.1: CHF2CF2OCH2CF3 is HFE-347mfc2 [James Franklin, Belgium]	Rejected. Compound is listed as HFE-347pcf2 (from US EPA).
8-1397	8	83				Table 8.A.1: (CF3)2CFOCH3 is HFE-347mmy2 [James Franklin, Belgium]	Rejected. Compound is listed as HFE-347mmy1.
8-1398	8	83				Table 8.A.1: (CF3)2CHOCH3 is HFE-356mm2 [James Franklin, Belgium]	Rejected. Compound is listed as HFE-356mmz1.
8-1399	8	83				Table 8.A.1: CF3CF2CH2OCH3 is HFE-365mc3 [James Franklin, Belgium]	Rejected. Compound is listed as HFE-365mcf3.
8-1400	8	83				Table 8.A.1: CF3CF2OCH2CH3 is HFE-365mc2 [James Franklin, Belgium]	Rejected. Compound is listed as HFE-365mcf2.
8-1401	8	83				Table 8.A.1: The complete formula of the compound (correctly) listed as HFE-43-10-pccc124 should be CHF2OCF2OCF2CF2OCHF2 [James Franklin, Belgium]	Rejected. Formula is listed as CHF2OCF2OC2F4OCHF2 (as in AR4).
8-1402	8	83				Table 8.A.1: The complete formula of the compound listed as HG-02 should presumably be CHF2(OCF2CF2)2OCHF2. This is HFE-53-12pcccc135 [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1403	8	83				Table 8.A.1: The complete formula of the compound listed as HG-03 should presumably be CHF2(OCF2CF2)3OCHF2. This is HFE-73-16pcccccc1357 [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1404	8	83				Table 8.A.1: The compound listed as HG-20, CHF2(OCF2)2OCHF2, is HFE-338pcc123 [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1405	8	83				Table 8.A.1: The complete formula of the compound listed as HG-21 should presumably be CHF2OCF2CF2O(CF2O)2OCHF2. This is HFE-53-12pcccc1235 [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1406	8	83				Table 8.A.1: The compound listed as HG-30, CHF2(OCF2)3OCHF2, is HFE-43-10pccc1234 [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1407	8	83				Table 8.A.1: CF3CF2CF2OCH2CH3 is HFE-467sf3 [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1408	8	83				Table 8.A.1: "Fluoroxene" should be "Fluroxene" [James Franklin, Belgium]	Rejected. Listed in CAS as both "Fluoroxene" and "Fluroxene".

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8-1409	8	83				Table 8.A.1: CHF2OCF2CHF2 is HFE-236ca1 [James Franklin, Belgium]	Rejected. But compound is now listed as HFE-236ca.
8-1410	8	83				Table 8.A.1: CF3CHFCF2OCHF2 is HFE-338mec3 [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1411	8	83				Table 8.A.1: The acronym PFPMIE has indeed been frequently used in the literature to designate the compound CF3OCF(CF3)CF2OCF2OCF3, or a complex mixture of which this substance is one component, but the justification of the name "perfluoropolymethylisopropyl ether" for this component is obscure and no doubt erroneous. [James Franklin, Belgium]	Rejected. This is consistent with AR4.
8-1412	8	83				Table 8.A.1: CHF2CHFOCF3 is HFE-236ea1 [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1413	8	83				Table 8.A.1: CF3CHFCF2OCH2CH3 is HFE-476mec3 [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1414	8	83				Table 8.A.1: CF3CF2CF2OCHFCF3 is HFE-42-11me3 (?) [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1415	8	83				Table 8.A.1: CHF2CF2CH2OCH3 is HFE-374pc3 [James Franklin, Belgium]	Rejected. Not listed in CAS
8-1416	8	84	26		29	The excessive number of digits shown here is inconsistent with the uncertainty. AND this uncertainty seems very small since the RF for maximal ozone depletion (today, sort of) has a value of -0.10 +-0.15. How can you get uncertainties in the GWPs of only 20%? [Michael Prather, United States of America]	Taken into account. Updated and made consistent.
8-1417	8	84	27	84	27	Why use 0.22 W m-2 here and elsewhere in the chapter use 0.20 (e.g., page 8-3, line 51)? Yes, it is a small difference but it best to be consistent. [Robert Portmann, United States of America]	Taken into account, Numbers updated.
8-1418	8	84				Table 8.A.1: C8F18 is PFC-71-18 [James Franklin, Belgium]	Taken into account.
8-1419	8	84				Table 8.A.1: While C10F18 can be designated as PFC-91-18, it would be useful to note that the 3 compounds refer to perfluorodecalin and its isomers. Otherwise, the fluorine-to-carbon ratio of C10F18 might suggest the presence of multiple bonds, which would not however be compatible with the long lifetime indicated. [James Franklin, Belgium]	Taken into account.
8-1420	8	84				Table 8.A.1: The last 5 compounds in this Table are olefins. It would seem more appropriate to designate them as perfluorolefins, "PFOs", in line with the term "HFOs", rather than PFCs. Indeed, the term PFC has a special meaning under the Kyoto Protocol, referring to the long-lived perfluorinated and saturated hydrocarbons, with consequently high GWPs, included in the greenhouse gas basket "HFCs + PFCs + SF6". According to this designation, the 5 compounds listed would be PFO-1114, PFO-1216, PFO-2316, PFO-1318, and PFO-1318, the last two being isomers. It might however be more useful to give them their chemical names, namely: tetrafluoroethene, hexafluoropropene, hexafluorobuta-1,3-diene, octafluorobut-1-ene, and octafluorobut-2-ene, respectively. [James Franklin, Belgium]	Taken into account. The last three compounds are listed with their chemical names.
8-1421	8	86				table 8.A.6: Bond 2012 revision should be checked. [Michael Schulz, Norway]	Taken into account. Updated.
8-1422	8	89				Figure 8.1: Inconsistency in nomenclature: figure uses Delta-T, but caption uses dT. [Philip Cameron-Smith, U.S.A.]	Editorial
8-1423	8	89				Figure 8-1: While the definition has been written this way extensively, so it may not be possible to change, the term "net flux imbalance" is confusing because "net" and "imbalance" both refer to the difference between incoming and outgoing radiation. If possible the authors should consider revising - and revising throguhout the report. [Government of United States of America]	Accepted. We now say 'net flux change' rather than 'imbalance'.
8-1424	8	90	4	90	10	Caption to Figure 8.2. Would be easier for the reader if the RCPs colours were presented as a legend and units were included on each axis. [Government of United Kingdom of Great Britain & Northern Ireland]	Editorial. Agreed
8-1425	8	90				Fig 8.2. There is something seriously wrong with these plots. Also, they contradict the numbers and units in the Annex II tables, which are decadal average emissions from the archived/reported RCP emissions. We need to reconcile this: Urge we use Tg(N) consistently for NH3, NOx, and N2O - using NO2 wt for NOx is misleading since most is emitted as NO. The NMVOC numbers in the tables is Tg(NMVOC) not Tg(C) and they are the same? which is correct. Likewise for OC and BC, there should be a larger difference for these numbers if the O & H are taken off. The plot is clearly showing only numbers every 10 years and so the average emissions should be used as was calculated from the RCP emissions given annually. A bigger issue	Noted: numbers were checked and validated against Annex II. Also units were changed.

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						is the CH4 emissions which are corrected to be consistent with current knowledge in Chapter 11. If the plotted numbers are indeed the RCP published values then Annex II and the Chapter 11 analysis is in error, let us resolve this asap. [Michael Prather, United States of America]	
8-1426	8	91				Figure 8.3 -- The authors should consider having the "r" and "mnbe" terms and values explained in the caption [Government of United States of America]	Accepted: this is now defined in the caption
8-1427	8	91				This figure is too busy; the median line is not needed and can be removed. It would be useful to add the number of ozone sonde sites used in each domain as labels, and to indicate the number of models involved in the caption. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Accepted: these terms are now defined
8-1428	8	92	7	92	7	Suggest adding date range for observations, to clarify that the observations only apply to the last couple of decades [Philip Cameron-Smith, U.S.A.]	Noted: we have not modified the figure because this information is available in the supporting table
8-1429	8	92				Figure 8.4 - What time period does the green shaded area represent? Also, it's not clear what the added value of the lower figure (8.4b) is. It should be fairly easy to pull out the Delta Burdens for 1980 and 2000 from figure 8.4a. [Government of United States of America]	Noted: green shaded area is removed and replaced by dashed line.
8-1430	8	92				Fig 8.4. This is an important figure, but the top and bottom panels are confusing. I understand why they have very different looking statistics since they are derived from the individual model changes, but then there is no way to use or interpret the upper plot. So why not drop it. Or just give the model means (not box and whisker) for the upper chart - simpler, as the stats on the top panel cannot really be used except to compare present day total burden. [Michael Prather, United States of America]	Noted: Only one panel is now shown.
8-1431	8	93				Figure 8.5 -- The graphs in this figure are oriented 90 degrees sideways compared to the graphs in Figure 8.2 -- the authors should consider having these types of graphs all oriented the same way for consistency (preferably the way they are oriented in Figure 8.2). Four reviewers made this same comment. [Government of United States of America]	Accepted: figure has been reversed
8-1432	8	93				Fig 8.5. This figure should be updated to use the projected abundances of CH4, N2O, and CO2 (for RCP 8.5) that are part of this assessment (Chapter 11 or CH4, N2O, Chapter 12 for CO2). These are different and they have uncertainties. We do not in this chapter need to document the CMIP5 boundary conditions (since you can refer to RCP pubs), but give our best estimate based on ACCMIP and related new results using those emissions. Chapters 11&12 put the numbers in the Annex II and show figures of these. I hope that Chapter 8 can use these values and their uncertainties to estimate projected RF. This does not mean that the RCP concentrations used in CMIP5 should not be evaluated for the RF that forced the models, but then just show the RF. The historical values of GHG and other climate forcings (abundance and RF) are very important, we must not lose those figures. Chapters 11 and 12 will show/discuss abundances/concentrations, including some aerosol AOD or loading, but not the RF, that really needs to come from Chapter 8. [Michael Prather, United States of America]	Accepted: Figure has been reversed. Additional SRES information was included. Only the original RCP information is shown as this is what is used in the remainder of Ch. 8
8-1433	8	93				figure 8.5: turn figures. [Michael Schulz, Norway]	Accepted: figure has been reversed
8-1434	8	93				Fig should be rotated so that time is on abscissa. [Stephen E Schwartz, United States of America]	Accepted: figure has been reversed
8-1435	8	94	4	94	7	What is the source of atmospheric GHG measurements used to produce Fig 8.6? It is not following IPCC rules to refer to a website for fundamental information that should be referencing peer reviewed literature. Does this use the average of AGAGE and NOAA measurements as produced inn Chapter 2? Need to be very specific here, that it is not based on just one network's measurements.. [Ronald Prinn, United States of America]	Taken into account: The data is now taken from a compilation of sources listed in Annex II.
8-1436	8	94		94		Figure 8.6 (a) and (b) is poorly drafted. Tick marks in (a) should extend above 1.000W/m2 and (b) above 0.1 W/m2 [M Daniel Schwarzkopf, United States of America]	Rejected. The author of the comment does not seem to realize that the tick mark placement is fully correct for a log scale.
8-1437	8	94				Fig. 8.6c – Why the annual change of radiative forcing of CO2 in Fig. 8.16c of FOD has been replaced by Fig. 8.6c which averages data over 5 years and therefore masks the annual fluctuations the amplitude of which reaches a factor as large as 4 in Fig. 8.16c of FOD ? Because the authors realized that 3/4 of the fluctuations are NATURAL since they FOLLOW temperature with a lag of 9-11 months as shown by Humlum et al (2012) ?	Rejected. The 5 year average is fully appropriate for a climate analysis. The objective of this figure is NOT to analyse the Carbon Cycle.

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						Please reintroduce the figure with the yearly resolution which shows the annual fluctuations, discuss them, and consider that the anthropogenic CO2 radiative forcing has to be reduced by at least 4, consistent with 13C/12C ratio. [François Gervais, France]	
8-1438	8	94				Fig. 8.6 - note the negative growth in CO2 in the early 1940s ? Is this real? It seems to come from the Etheridge et al Law Dome record but only with the 20-year smoothing curve. Fitting firn air measurements to an atmospheric surface forcing is not unique - there are a range of different curves that can give you the firn -air record. But, Law Dome does close off the firn quickly. I would recommend that you not highlight the inverse CO2 growth in the first 5 years of the 1940s unless you want to discuss it and say how certain you are of it. There is a large danger in differentiating a curve that was fitted to the firn data. A simple solution would be to do 10-year decadal bar charts in the figure and then you would have a near-zero for CO2 derivative in the 40s. You can do 5-yr bars in the 1980s when we have reliable atmospheric measurements for enough of the gases. IF you do want to show the negative CO2 growth in the early 1940s, then a better explanation is needed, especially as this may be a large target. [Michael Prather, United States of America]	Taken into account: The revised figure based on Annex II data shows less negative growth in this period.
8-1439	8	94				figure 8.6: the logarithmic scale prevents to see the rate of increase in a linear fashion. Although that is done in "the rate of change" figure, I am tempted to ask for a linear scale. Forcing differences translate almost linearly into temperature. Very small RF are not very interesting. [Michael Schulz, Norway]	Taken into account, by showing the figure both on linear and logarithmic scale
8-1440	8	94				Useful fig [Stephen E Schwartz, United States of America]	Noted. Thanks
8-1441	8	95				Figure 8.7 -- The Osterman et al. 2008 study is indicated by a black circle in the legend, but the black circle on the graph itself seems to be missing/is not visible. Also, it's not clear from the caption what the red and black error bars on the right hand side of the graph are supposed to indicate. The authors should consider adding text to the caption to explain them. [Government of United States of America]	Noted, but this figure has been removed
8-1442	8	95				Figure 8.7 - The text in the chapter about Figure 8.7 talks about Tg of tropospheric ozone increase, but Figure 8.7 itself is in DU units. The authors should consider having either the figure should be changed to Tg units, or the text should have the discussion in DU units, for easier/quicker comprehension. [Government of United States of America]	Noted, but this figure has been removed
8-1443	8	95				Figure 8.7: Need fuller explanation of symbols. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted, but this figure has been removed
8-1444	8	97				Figure 8.9. When first cited, explain why the sulphate radiative forcing does not follow SO2 emissions. [Nathan Gillett, Canada]	Rejected, not sufficiently of importance for this chapter to discuss such details
8-1445	8	97				The caption states that the total line is sum of the other 6 lines, but it is clear that it is different than the sum. [Robert Portmann, United States of America]	Taken into account, by modification of the total to also include RF of BC snow.
8-1446	8	97				Useful fig [Stephen E Schwartz, United States of America]	Noted
8-1447	8	98	1	98	7	It is recommended to delete Figure 8.10. Reasons: According to the figure, the radiative forcing history of black carbon in snow and ice reached a sharp peak in 1980. We think that exaggerated BC forcing. In addition, there is only one reference cited, which should not be considered sufficient evidence . [Government of China]	Taken into account, Figure 8.10 ise merged to Fig. 8.9.
8-1448	8	98		99		In the caption of these figures acronyms are used and not explained. Maybe they will be explained in the text but they should also be written out in the caption. [Government of Germany]	Accepted, see 8-1447
8-1449	8	98				Figure 8.10: This figure could be merged into 8.9; The content is limited and very uncertain. For instance, how does it correspond to the ice core data of high BC in Greenland in the beginning of the 19th century. The values are also very small. [Michael Schulz, Norway]	Accepted, see 8-1447
8-1450	8	99	4	99	4	add "upward" before TOA SW [Government of Poland]	Accepted as suggested
8-1451	8	99				Figure 8.11. The lower panel seems to show substantial LUC SW flux change over much of Australia. I'm not an expert on this, but has all this area really been affected by athropogenic land use change? Isn't much of the	Rejected. There is indeed a lot of anthropogenic land use in Australia

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						shaded area desert? [Nathan Gillett, Canada]	
8-1452	8	99				Figure 8.11: The color scale does not allow to see regions of equal forcing. Is it possible to use a non-continuous color scale? [Michael Schulz, Norway]	Accepted: Changed as suggested
8-1453	8	100				As stated in comment 1, this figure shoul be recalibrated based of a mean value around the middle of 2009, last minimum where the intensity gradient are low. [Bo Andersen, Norway]	Taken into account, We performed the standardization of the composite series to TIM annual average (2003-2012).
8-1454	8	100				Figure 8.12: Smoothing seems to cause artefacts at the edges of the time series, seen as abrupt changes in ACRIM around 1979 and in RMIB in 2009. [Natalie Krivova, Germany]	Taken into account, We have revised the data. Now the problem is gone. See Fig. 8.11 (previously 8.12).
8-1455	8	100				Fig. 8.12. Something wrong with the RMIB data after 2008, remove. [Michael Prather, United States of America]	Taken into account, We have corrected this problem, see Fig. 8.11 (previously 8.12).
8-1456	8	100				Figure 8.12: Is the RMIB curve after 2008 correct? [Michael Schulz, Norway]	Taken into account, We have corrected this problem, see Fig. 8. 11. (previously 8.12).
8-1457	8	101				Figure 8.13: Can the abrupt jumps in the Velasco curve at 1830 and 1940 be commented? [Michael Schulz, Norway]	Taken into account, They are related to the Sun leaving the Dalton and Modern minima and come from the time series of the group sunspot number, that is the input data for the Velasco et al. model. However, this curve has been deleted from Fig. 8.11 (previously 8.12 or 8.13).
8-1458	8	102				Figure 8.14- There is no mention of the 'Gao' or 'Crowly' studies in the caption. The authors should make sure these references are used. [Government of United States of America]	Accepted, Caption has been updated.
8-1459	8	102				Figure 8.14 -- In the caption "Paleoclimate Modal Intercomparison Project (top)," what is the "top" refering to? Also, are the Gao and Crowley lines on the graph other model estimates? Only Sato is mentioned in the caption. [Government of United States of America]	Taken into account, Caption has been updated.
8-1460	8	102				Figure 8.14 . Is the AOD from Pinatubo greater than from Krakatoa. The Sato data used at GFDL would not have it. It it difficult to distinguish the 3 data sources. [M Daniel Schwarzkopf, United States of America]	Taken into account, Caption has been updated. The data are based on the latest data sets from each of the three sources.
8-1461	8	102				This figure might be clearer if presented on a log scale similar to Fig 8.15 [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Taken into account, We tried it that way, but feel it is cleared to do it as a linear scale.
8-1462	8	103				Figure 8.15 -- The caption refers to panels a,b, but they're not labeled in the figure itself. Also, it's not clear what part A is referring to where the caption says "derived from (left) SAGE II" and "and (right) CALIOP scattering ration" -- do they mean the left and right sides of the color top section in the figure? Caption for A also refers to "black contours" -- do they mean the dotted lines in the color top figure? If so, the authors should consider indicating this as "dotted black contours." Finally, in part B the authors say "CALIOP (blue line)" but in the figure legend written on the figure itself, it says "CALIPSO" -- could the figure text be changed to "CALIOP" or the caption text all changed to CALIPSO? [Government of United States of America]	Taken into account, Figure has been updated, and the caption now corresponds to the figure.
8-1463	8	103				Fig. 8.15. Great figure, but there is obviously some observing artifact from 2007-2010 where it looks like a cut-off below 21 km. Is this real, OK. If not, then put in a note about the GOMOS? data. [Michael Prather, United States of America]	Accepted, This has been addressed in the new figure. The GOMOS profiles have been scaled to the SAGE II profiles (between 2002-2005) which in turn have been used to scale the CALIPSO profiles (through the calculation of a lidar ratio).
8-1464	8	104				Figure 8.16 -Is it possible for the authors to include some way to indicate the sign of forcing for each component? [Government of United States of America]	Rejected. This is not the purpose of this figure. There are other figres to show sign and amplitude of RF components
8-1465	8	104				Fig. 8.16. I do not like this because the minimum thickness (needed to be seen) exaggerates the importance	Rejected, the main purpose with the figure is to show

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						of several small components. If you want to keep the figure, then try putting those less relevant ones as dotted lines. [Michael Prather, United States of America]	development in the confidence level for the forcing agents, The most important forcings are highlighted.
8-1466	8	105	1	107	7	Fig. 8.17 and 8.19 do not include volcanic forcing because at this time volcanic RF is almost zero. This may mislead the reader that volcanic forcing has negligible effect on global mean temperature in 2011. Readers would like to estimate, based on RF from Fig. 8.17 and 8.19, how various drivers contribute to climate change but this marginalizes of volcanic impact. Because of historic volcanic eruption current temperature is smaller than if there were no volcano eruption. Both figures do not show cumulative RF which is related to temperature change between 1750 or 1980 and 2011. Anyway RF maybe zero some time but temperature may change due to climate system inertia and because of some large RF sometime between 1750 and 2011. Therefore, I advise to add for example mean RF for each component since 1750 to Fig 8.17, 8.19 or 8.18? [Government of Poland]	Taken into account, by showing a new figure (Fig 8.19) which highlight the importance of natural forcing over last 15 years. We include also a period 1970-2011 which is the same as in Box13.1. We refer to this box where importance of volcanic forcing is further explored.
8-1467	8	105	4			Please consider having this graph professionally rendered. We can do far better in aesthetically and effectively displaying this information. Please consider showing the short lived climate forcing agents, ie CH4, trop O3, HFCs, and black carbon in a graphical inset. The visibility of these quantitative forcings woud be valuable to scientists and policymakers alike. Please consider adding an explanation in the caption concerning the arrows on the black carbon term. [David Fahey, United States of America]	Taken into account, graphics highly improved.
8-1468	8	105	4			Please consider harmonzing the values in F8.17c with those in Table 7.1. The terms are not common to both and values are not identical. [David Fahey, United States of America]	Taken into account, by using the new terminology
8-1469	8	105		105		The figures are too small. [Government of Germany]	Editorial
8-1470	8	105		105		Fig. 8.17a,b: x-axes labels should read “radiative or adjusted forcing”. [Lazaros Oreopoulos, United States of America]	Editorial. Agreed and ERF is used in Fig 8.16 and RF in Fig 8.15 and Fig 8.17
8-1471	8	105				Figure 8.17c - What do the vertical error bars represent in this figure? Please clarify. [Government of United States of America]	Taken into account, by including a description in the caption
8-1472	8	105				Figure 8.17 - Should volcanoes be included in these figures? [Government of United States of America]	Rejected, volcanoes is difficult to include in Fig 8.15-Fig 8.16, but shown in Fig 8.18 and an important part of Figure 8.19 (new)
8-1473	8	105				Figure 8.17c. Please specify that the forcing is for changes in concentrations in 2011 resulting from emissions prior. Figure 8.17c: Is the O3 effect from N2O small, or ignored? Figure 8.17c. Is the aircraft bar just contrails? If so, please state explicitly that effects from emitted CO2, NOx, and water are not included.. Figure 8.17c. CO, NMVOC and NOx emissions led to CH4 changes, should one also include the O3 and H2O effects from CH4? [Government of United States of America]	Taken into account, the period 1750-2011 is given, values are provided in the supplementary material where it is stated that O3 from N2O is neglected due to insufficient quantification.Rejected, to the comment on non-contrail effect from aircraft since CO2 and other components had been added as various bars. O3 and H2O is already included to CH4 emissions (see bar 2 from top).
8-1474	8	105				Figure 8.17: it is unclear what the error bars in the y-axis direction should mean, e.g. those connecting CH4 and halocarbons. [Rolf Müller, Germany]	Taken into account, see comment 8-1471
8-1475	8	105				Figure 8.17c: The BC and OC forcing bars are not easily understandable. Why are there vertical error bars on CO and Nox and Halocarbons and CH4? A little special. "Components of radiative forcing"? Is that the best title for this graph? Maybe omit. [Michael Schulz, Norway]	Taken into account, Improvements have been made to OC and BC bars as well as further description in the caption, see comment 8-1471 fro vertical lines. Rejected on title.
8-1476	8	105				Specify meanings of uncertainties or error bars in caption. [Stephen E Schwartz, United States of America]	Taken into account, see comment 8-1471
8-1477	8	105				An argument can be made that the uncertainties indicated for LW forcings by CO2 and by WMGHGs are substantially underestimated. CO2 forcings and climate response of 15 atmosphere-ocean general circulation models (GCMs) that participated in round 5 of the Coupled Model Intercomparison Project (CMIP-5) were	Rejected, an uncertainty of 20% for ERF is assessed based on Vial et al. (2013). This is (-20% to +20%) and see further description in Section 8.3.2 in TOD.

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						compared by Andrews et al (2012). Forcing and temperature response coefficient were inferred from the output of the model runs respectively as intercept and slope of a graph of net top-of-atmosphere energy flux versus global mean temperature anomaly subsequent to a step-function quadrupling of atmospheric CO2. (Because the model experiments examined response to a quadrupling of CO2, rather than a doubling, the intercept had to be divided by 2 to obtain the forcing pertinent to doubled CO2). The forcing is interpreted as an "adjusted forcing" that includes rapid adjustments, mainly of atmospheric structure, that modify the TOA radiative flux on time scales shorter than a year or so. A key finding of Andrews et al. was the spread of values of forcing exhibited by the different GCMs, 16%, 1-sigma. The spread in forcing is a consequence of differing treatments of the radiation transfer in the several models as well as different treatments of clouds that interact with radiation. As the forcing inferred from the analysis of Andrews et al. is an adjusted forcing, it appropriately reflects differences among the models in rapid (<~ 1 yr) response of atmospheric structure to the imposed forcing. This spread in forcings inferred from the climate model runs is substantially greater than the uncertainty specified in the Figure. That there is such a range of forcing as inferred from GCM runs should not come as much of a surprise. For example, although the Radiative Transfer Model Intercomparison Project (Collins et al., 2006) reported a 1-sigma spread in longwave forcing at 200 hPa among the GCMs compared of only 8.5%, that study was restricted to cloud-free atmospheres, with the reason given that "the introduction of clouds would greatly complicate the intercomparison exercise," from which one infers that the spread of forcing in a model with clouds would greatly exceed that in an idealized cloud-free model. Hence the finding of a 1-sigma spread of ± 16% in the forcings (i.e., 5-95% range ± 26%, well greater than the ± 10% shown in the figure) is likely as accurate an assessment of the maximum level of confidence as can be placed at the present time in forcing by LLGHGs. Andrews, T., Gregory, J. M., Webb, M. J. and Taylor, K. E. 2012. Forcing, feedbacks and climate sensitivity in CMIP5 coupled atmosphere-ocean climate models. Geophys. Res. Lett. 39, L09712. Collins, W. D., Ramaswamy, V., Schwarzkopf, M. D., Sun, Y., Portmann, R. W., Fu, Q. et al. 2006. Radiative forcing by well-mixed greenhouse gases: Estimates from climate models in the IPCC AR4. J. Geophys. Res. 111, D14317. [Stephen E Schwartz, United States of America]	We underscore further that the uincertainty in RF of +-10% is with radiative transfer codes more validated with LBL codes than usually used in many GCMs and meteorological data (including clouds) adopted is closer to observation than in many GCMs.
8-1478	8	105				For all the discussion about adjusted forcings, I am surprised to see that they are so close to unadjusted forcings. This deserves discussion; is adjusted forcing not all that useful after all? [Stephen E Schwartz, United States of America]	Taken into account, this is described in section 8.1 and 8.3.2 in the TOD
8-1479	8	105				In panel C I am unable to figure out the colors or the vertical error bars, e.g. on CO. [Stephen E Schwartz, United States of America]	Taken into account, see comment 8-1471
8-1480	8	105				Meaning of horizontal error bars needs to specified in caption. 5-95%? [Stephen E Schwartz, United States of America]	Taken into account, see comment 8-1471
8-1481	8	105				Figure 8.17. What is the significance of the vertical bars. [M Daniel Schwarzkopf, United States of America]	Taken into account, see comment 8-1471
8-1482	8	105				Figure 8.17. The range of AF for CO2 and the other WMGGs is similar to that for Aerosols.This means that the confidence level for WMGHGs is not as high as stated. [M Daniel Schwarzkopf, United States of America]	Rejected, the contribution to the overall anthropogenic uncertainty is smaller for GHGs than aerosols, and the relative uncertainty is much smaller.
8-1483	8	106	1	106	1	The Figure 8.18 graph appears to have been cut off a year early, or else the description at the RHS is incorrect. Please can it be extended to include 2011 values, as it incorrectly implies that it already does. The bars on the RHS state "Year 2011", matching statements in the text about radiative forcing in 2011, and are exactly equal in size to the final height of the corresponding sections of the shaded line graph. But that graph appears to end in 2010, not 2011. Comparison of the post 2000 values of volcanic forcing (which should show peaks in 2003 and 2005 per GISS forcing data) confirm that the final data points shown must indeed be values for 2010, not 2011. Moreover, the final value for Total Anthropogenic adjusted forcing, measured off Figure 8.18, is 2.20 W/m^2, some way below the best estimate of 2.24 W/m^2 for 2011 stated on page 8-39, line 4. Extending the graph from 2010 to 2011 at its 2000-2010 ex volcanic trend would take the final value to the correct level of 2.24 W/m^2. NB It would be very helpful if the values comprising this and other graphs	Accepted, it is corrected to include 2011. It is given in the caption that values are available in Annex II.

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						were made available in digital format. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	
8-1484	8	106	4	106	4	For the spikes due to volcanic activities it would be good to mention the event itself (e.g. Pinatubo) [European Union]	Rejected, sufficient that this is mention in section 8.4.2
8-1485	8	106				Figure 8.18 - Regarding the time evolution of RF of aerosol-cloud-interactions, where is the source for the time evlution of RF of aerosol-cloud interactions shown in Figure 8.18? [Government of United States of America]	Accepted, a reference is made to Supplementary material where this information is included.
8-1486	8	106				Meaning of error bars needs to specified in caption. 5-95%? [Stephen E Schwartz, United States of America]	Accepted, included as suggested.
8-1487	8	107		107		Fig. 8.19: x-axis label should read “radiative or adjusted forcing”. [Lazaros Oreopoulos, United States of America]	Rejected, it is given RF and ERF in the caption
8-1488	8	107				Figure 8.19 - Please clarify whether this figure represents the change in forcing from 1980 to 2011 (in other words RF2011 relative to RF1980) or it is the average RF from 1980-2011 relative to 1750. Also, is it appropriate to include RF from volcanoes? [Government of United States of America]	Taken into account, with adding 'the forcing between 1980 and 2011' in the reference to the figure. We have not found it appropriate to include volcanoes in this figure, but we have included it in the new figure 8.19 in TOD.
8-1489	8	107				It seems from reading the caption that this figure is meant to include the AF bars (hatched), but only does so for a couple of the items. The authors should conside rrevising the figure accordingly. [Government of United States of America]	Taken into account by describing the ERF and RF concepts much more clearly through the whole chapter.
8-1490	8	107				Meaning of error bars needs to specified in caption. 5-95%? [Stephen E Schwartz, United States of America]	Accepted, by adding 'Uncertainties (5-95% confidence range) are given for RF (dotted lines) and ERF (solid lines). ' to the caption.
8-1491	8	107				Figure 8.19. The confodence level for total anthropogenic is low even for 1980-2011. [M Daniel Schwarzkopf, United States of America]	Rejected, we have not stated any confidence level for this period and the anthropogenic forcing
8-1492	8	108	5	108	5	Improve quality of graph [European Union]	Taken into account, quality of figure highly improved, with a) and b) merged to one figure
8-1493	8	109	4	109	4	Improve quality of graph [European Union]	Taken into account by better quality for the lines.
8-1494	8	109	4	109	4	add "anthropogenic" after Global mean [Government of Poland]	Accepted, revised.
8-1495	8	109				Figure 8.21 - This figure seems incomplete. Why are there 2 RCP scenarios used for the WMGHG plots, 4 RCPs for the Net, 1 for aerosols, and 3 for ozone? Also, there are many inconsistencies with the legend described in the caption vs what is actually used in the figure. The authors are encouraged to revisit this figure for completion. [Government of United States of America]	Taken into acount. Added that individual components for some RCPs were omitted for visual clarity.
8-1496	8	109				Figure 8.21: Figure seems to me incomplete. Where are for instance the WMGHG forcing lines for RCP45+60? [Michael Schulz, Norway]	Taken into acount. Added that individual components for some RCPs were omitted for visual clarity.
8-1497	8	109				Figure 8.21 Are some of the lines missing here? I don't see any dot-dashes, circles or squares? [Kate Willett, United Kingdom]	Taken into acount. Added that individual components for some RCPs were omitted for visual clarity.
8-1498	8	110				Figure 8.22 - Please clarify what effects are included in producing the figures in row 5, labeled "All aerosol adjusted atmospheric forcing" in the figure (but described as "atmospheric absorption" in the caption. Are WMGHGs also included in this figure? [Government of United States of America]	Accepted. The caption now matches the figure.
8-1499	8	111				Fig 8.23. The first row of figures has something wrong with the color scale? The STD is huge, it should be noted in the caption so that people realize this (STD > 3 x |mean value|?). It is not clear what is the difference between the two different STDEVs on the top of the right panel of figures? both are standard deviations? [Michael Prather, United States of America]	Noted. The standard deviations are quite large in some regions. Note that the color scale on the mean typically saturates in those regions. The 2nd value of STD DEV is explained in the caption.
8-1500	8	114				Fig. 8.26: The arrows from the left Metrics-box to the central boxes as well as those from the right Mitigation-box do not indicate cause-effect relationships, what is their meaning? There is no influence of these lateral boxes on the central ones. The same for the dotted arrows. It is suggested to delete the lateral boxes and the	taken into account. More explanation added.

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						dotted arrows. [Government of Germany]	
8-1501	8	114				Figure 8-26: The ideas of the cause-effect chain and the cascading uncertainty have been published by some earlier studies (see Figure 1 of Schneider (2002, Climatic Change, 52, 441–451) and further references therein). The original papers need to be properly acknowledged. [Katsumasa Tanaka, Switzerland]	rejected. We agree that several earlier studies have presented a cause-effect chain. Here we refer to papers that put this into a metric context.
8-1502	8	115		116		Figs 8.27 & 28 are both nice, but given space limitations, pick one to make the point. In 27, why the 1.5 and 13 yr, why not pick the CH4 perturbation lifetime (11 y)? [Michael Prather, United States of America]	Rejected. Fig 8.27 and 8.28 illustrate different issues. The latter shows the impact of the reference gas in the denominator on the GWP values. We have however removed 8.28b.
8-1503	8	115		116		Fig 8.27a is a useful add. Delete Figure 8.27 b and expunge any discussion of temperature change in this chapter; Figure 8.28 is a useful add. [Stephen E Schwartz, United States of America]	Noted. We will however keep 8.27 b since it illustrates one of the metrics discussed in the chapter.
8-1504	8	115				Figure 8.27: The relation of the three emission pulses to each other and the two metrics should be specified more clearly. Now the point that the figure tries to make is difficult to grasp, as there is no equivalence neither in the sense of GWP nor GTP between the emission pulses. The gases' lifetimes are specified, but how the emitted amounts (i.e. tonnes) and/or radiative efficiencies differ between the three pulses? Clarify also whether the RF and temperature pulses with the same colour in panels a) and b) are equivalent. [Tommi Ekholm, Finland]	Taken into account. We think the figure is clear but some more explanation and clarifications may be given to the extent that space allows.
8-1505	8	115				Fig. 8.27: Do the gases in b) have the same life times associated with the colors given in a)? [Government of Germany]	Taken into account. More explanation and clarifications is given.
8-1506	8	115				Figure 8.27 -- The caption contains the term GTP but GTP is defined in the text on pg 52, ln 31, after the placement of the graph. The authors should consider shifting the placement of Figure 8.27 so it is later in the text, or spell out GTP in the caption. [Government of United States of America]	Taken into account. We will spell out GTP in the caption.
8-1507	8	115				Fogire 7-27: Units for the y-axes are missing. [Katsumasa Tanaka, Switzerland]	Noted. This is meant to be a generic illustration of the concepts, not an illustration with numerical information.
8-1508	8	116				Figure 8.28 - This could use a better caption, especially since AGWP is not explained in the text. What are the inputs to these calculations? Two reviewers made this same comment. [Government of United States of America]	Taken into account. Caption has been improved.
8-1509	8	116				Figure 8-28a: Values of CH4 AGWP and SF6 AGWP do not have to be scaled (if they are still scaled) because they are plotted against the second y axes. [Katsumasa Tanaka, Switzerland]	Noted. SF6 figure removed.
8-1510	8	117				Figure 8.29 is very interesting, but needs better caption and internal labeling to be more readily understood. [Michael Prather, United States of America]	Taken into account. Caption and labelling will be improved.
8-1511	8	119				Figure 8.11 No identification is given for the colours used, [Pieter Aucamp, South Africa]	Taken into account, colour scale improved
8-1512	8	119				Figure 8.31 - Please provide the (unweighted) mass emission for each species in the figure. The authors could quite readily add it to the legend. Also, it would be helpful if the authors could clarify what GWP and GTP values were used in the calculation? In the case for NOx, are the values from aviation NOx, shipping NOx? Depending on whether the authors want to make a case for GTP values derived using step increase in emission, the authors may consider adding to this figures values for GTP(step)-weighted emissions in addition to GTP(pulse)-weighted emission. [Government of United States of America]	Taken into account. We will provide a reference to the emission data. And have added which metric values that are used (in the Supplementary Material).
8-1513	8	120		122		Too may Aamaas figures, not easy to read and repeats. Use of these with such quantifiable numbers seems a bit dangerous unless these are really 'assessed" here. Fig 8.34 - the label on the Y axis is confusing and not helpful. The figure needs to be redone if possible. There is confusion as to why CO2 from biomass counts here (e.g. for savanna) as it regrows each year. Presumably this is open fires and includes forest fires. There is recent work by Jim Randerson showing that some wildfires have a cooling effect. Perhaps this figure and analysis is overly simplistic. [Michael Prather, United States of America]	Taken into account. Number of figures reduced. And sensitivity tests done to check robustness.

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8-1514	8	120				Fig. 8.32: "man-made": Please be gender-neutral, this word is "human-made" or anthropogenic. [Government of Germany]	Accepted as suggested.
8-1515	8	120				Figure 8.32 - The definition for GTP mentions using a step change in emissions. The only places where results from sustained step change emissions are discussed are in Figures 8.32 and 8.33. There are very few sentences discussing this in the text. If the authors want to include those results, please consider including GTP(step) weighted emissions in addition to the GTP(pulse) weighted emissions in Figure 8.31. Also, what GTP values were used in the calculations? In the case for NOx, are the values from aviation NOx, shipping NOx? [Government of United States of America]	Taken into account. We have removed 8.32b and 8.33b. Thus, less weight is put on step changes. And we will add which metric values that are used (in the Supplementary Material).
8-1516	8	123	1	123	1	FAQ8.1, Figure 1: Units for the graph are missing. I do not understand, what the picture tries to show. [Urs Neu, Switzerland]	Taken into account. The FAQ figure is only for illustration and designed for a broad audience; The caption has been expanded for a better description of the various processes that are illustrated. There is no need for units on the upper left plot whose purpose is to show the increase of saturation water vapor with temperature
8-1517	8	123	1	123	7	FAQ 8.1 - Figure 1 could be made more useful with an explanation of the various processes and interactions. [Government of Australia]	Taken into account. The caption has been expanded for a better description of the various processes that are illustrated
8-1518	8	124	1	124	1	FAQ8.2, Figure 1: Replace "ozone pollution controls" by "ozone precursor controls". Reason: ozone pollution cannot be directly controlled, this is only possible for its precursors (which are actually listed in the boxes below). [Urs Neu, Switzerland]	Noted: we feel that there is no contradiction between using ozone pollution control and the comment. The arrows are pointing towards the specific targets, which clearly indicate that ozone is not emitted but produced.
8-1519	8	124	1	124	1	I'm not an expert by a longshot, but I think the labelling and the directions of some of the arrows in this figure need to be changed. For the green and yellow titles in the upper boxes, I would replace "controls" with "reduction" (the objective of emissions regulations is to reduced tropospheric ozone and PM). In the middle layer, I would replace "Target emissions" with "Emissions controls" (these are presumably the things that can be controlled by regulation). The arrows connecting the upper layer with the middle layer should therefore point upwards (regulation of emissions in the middle layer aims to reduce pollutants in the upper layer). There presumably could also be black lines connecting the upper layer to warming or cooling impacts. [Francis Zwiers, Canada]	Noted: we have checked the arrow directions and they are all representative of our state of knowledge
8-1520	8	124		124		The caption for this Figure should make clear whether or not this schematic is intended to illustrate only direct radiative effects or whether aerosol-cloud interactions are intended to be encompassed as well. [Government of Canada]	Noted: the main text now mentions all aspects (direct and indirect), so we haven't changed the caption.
8-1521	8	124				This figure would be cleaner with fewer line-crossings: in the middle row move NH3 to the far right hand side and shunt BC, OC and SO2 to the left one space. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Noted: for consistency, all boxes are listed in alphabetical order and kept that way. The final version of the figure has been improved to limit the clutter by changing arrow sizes.
8-1522	8	129				Supplementary Material 8.SM.2.2. The value from Shapiro et al. (2011) was recently reduced by a factor of 2 by Judge et al. (2012, A&A 544) [Natalie Krivova, Germany]	Taken into account: Shapiro et al. used the model A (supergranule cell interior) of Fontenla et al. (1999). But Judge et al. indicate that by using such model Shapiro et al. overestimated the quiet-Sun irradiance variations by a factor of around 2. Also Shapiro et al. in their paper indicate that taking other Fontenla et al. model, their forcing would have an uncertainty of 50%. This remark in the text of Supplementary

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							material Section S8.6.
8-1523	8	130				Supplementary Material 8.SM.2.3. Values listed for the UV contribution to the TSI changes are not quite up to date. The estimate of 30% is now considered to be the lower limit. Leaving out the controversial SORCE results, all models and other data provide the range of 30-90% for the contribution of the UV below 400 nm (Ermolli et al. 2012 ACPD 12), with a more probable value of around 60% (Krivova et al. 2006, A&A 452; Morrill et al. 2011, Sol. Phys. 269; Ermolli et al. 2012). The difference of the results by Harder et al. (2009) in the UV to other estimates is a factor of 2-6. [Natalie Krivova, Germany]	Taken into account: SM revised and update them with the help of the papers suggested here.