IPCC Working Group I Fourth Assessment Report

Expert and Government Review Comments on the Second-Order Draft

Chapter 2

The following compilation of review comments and author responses is supplied by the Working Group I Technical Support Unit as a record of the process used to prepare the Working Group I report. These comments and responses are not to be edited and/ or re-distributed in part or in full to others.

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

Batch AB (15 June 2006)

No.	Batch	Page:line From	Page:line To	Comment	Notes
2-1	A	0:0	0:0	The chief defect of this Chapter is the total absnce of the main greenhouse gas, water vapour, By comparison, the others are insignificant.The usual excuse for this blatant omission is that computer models are so defective that the only way they can deal with the undoubted importance of water vapour is to relegate it to the status of a "feedback", and so remove its importance from public scrutiny. This Chapter is about greenhouse gases, not about the limitations of computer models. Water vapour is the most imporatnat greenhouse gas, and it should appear at the beginning, before all the others.You will, of course, have to admit that we know very little about its average or local concentration either recently or historically, and you may well conclude that this ignorance is an overwhelming liability to our current efforts to try and examine the possible influences on the climate of changes in greenhouse gases. [VINCENT GRAY (Reviewer's comment ID #: 88-110)]	Water vapour is condsiderd in many other chapters (eg. 8). We also discuss it here. We currently explain why water vapour is not a forcing in section 2.3 and will bring these statements forward to the intro. Note that climate response can be thought of as a consequence of "forcing" and "feedback" – both are equally important. The value of diagnostic analyses in terms of these has been recognized for a long time.
2-2	A	0:0	0:0	You similarly ignore the influence of clouds in your section on "Aerosols". Clouds are also a major influence on radiative forcing,.They also represent a defect in model treatment of the climate, where they are treated as a"feedback". Again, you cannot use this defect of the models as an excuse for ignoring their inflkuence in a Chapter devoted to radiative forcing. [VINCENT GRAY (Reviewer's comment ID #: 88-111)]	Section 2.4 does consider cloud-aerosol effects, and references t o Chapter 7 where other effects are discussed. Cloud changes themselves are discussed in chapter 3 and 8. It is highly useful to partition in terms of climate forcing and feedback. Both are important in the climate response as discussed in the various chapters of this report.
2-3	A	0:0		Scientific convention is to define uncertainty range as +/- two standard deviations (95% confidence interval). This is acknowledged in Box TS 1.1., Pg TS 4, lines 41-42, where the +/- two standard deviations is given as the default range, and in Chapter 3 (Pg. 3-7, lines 18-19), where the authors use +/- two standard deviations because "This allows us to assess what is really unusual." The use of +/- one standard deviation for uncertainty range in some cases (e.g. for RF) is misleading, because it cuts the uncertainty range in half. It is also confusing, because in most other places in this report (e.g. Chapter 3) the conventional approach of using +/- two standard deviations is used. The conventional approach should be used through out the report and all uncertainty ranges stated as +/- two standard deviations. [Lenny Bernstein (Reviewer's comment ID #: 20-46)]	We now will use a 90% confidence interval throughout this chapter, and make this the convention for the report. For many forcings and other things not enough infomation is available to define higher confidence intervals
2-4	A	0:0		During the past 5 years, a government level of Sino-Japan Joint Project on 'Aeolian Dust Experiment on Climate Impact' (ADEC) has been implemented (e.g., see Mikami et al., 2006) and a lot of achievements and new findings have been reached (e.g., see Special	Accepted. This reference is now included in the section on the radiative forcing due to mineral dust.

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				Issue on ADEC, Journal of the Meteorological Society of Japan, Vol. 83A, March 2005, and total of 20 papers are there). It has somehow similar importance with several international aerosol projects such as the ACE-Asia, especially for the Asian Dust, it should be reflected in the related parts of the AR4 of IPCC WG1.For reference, see" Mikami et al. 2005: Aeolian Dust Experiment on Climate Impact, 2006: An Overview of Japan-China Joint Project ADEC, Global Planetary Change, (accepted)." [Govt. of China (Reviewer's comment ID #: 2006-27)]	
2-5	A	0:0		Chapter 2: overall, this chapter is exhausted with details. Perhaps it is necessary ... but it is not a joy to read. [Govt. of Finland (Reviewer's comment ID #: 2009-10)]	Accepted. Will be shortened by ~10%, and detail considerd on a case-by-case basis
2-6	A	0:0		1/ The review concerns the section related to Aerosols. Well written and clear in general. However, sub-sections need to be renamed. Suggestions: 1/Introduction and Summary of the TAR, 2/ Development of Observation Tools (2.1 Satellite based observation, 2.2 Ground based observation), 3/ Advances in Modelling, 4/ Direct Radiative Forcing, 5/ Indirect Radiative Forcing - Cloud-Aerosol Interaction. [Savitri GARIVAIT (Reviewer's comment ID #: 82-4)]	Noted. There has been some slight restructuring, but mainly in the titles that are now more explicit about the contecnts of each section.
2-7	A	0:0		2/ Aerosols from Biomass Burning - Emissions from agricultural area burnings, which constitute one of the major sources of atmospheric aerosols especially in SE Asia, are missing. Although related data are scarce, their impacts on air quality and global warming are expected to be quite different from those of forest fires and should be stated. [Savitri GARIVAIT (Reviewer's comment ID #: 82-5)]	Rejected. Aerosols from biomass burning explicitly include agricultural emissions. Indeed the majority of emissions from much of the African continent are from these sources.
2-8	A	0:0		3/ At the end of the Chapter, a section dedicated to Perspectives, i.e. what are the next steps in R&D or Trend in atmospheric changes in GHG and aerosols, is recommended in order to involve more resercher from developing countries. [Savitri GARIVAIT (Reviewer's comment ID #: 82-6)]	Rejected. Not within scope of report
2-9	A	0:0		Congratulations on an overall very well structured and clearly written chapter. [Govt. of Germany (Reviewer's comment ID #: 2011-99)]	Thank-you
2-10	A	0:0		A broad-ranging and well-written chapter - the authors should be commended on it. [Nathan Gillett (Reviewer's comment ID #: 84-1)]	Thank-you
2-11	A	0:0		I think the authors should try to work to ensure that the chapter is more intelligible to non-specialists. While some parts of the chapter are clearly explained, others are more difficult to understand for a non-specialist. I think some sections should be simplified, and in other cases concepts and terms should be more clearly explained when introduced. [Nathan Gillett (Reviewer's comment ID #: 84-2)]	Accepted. Will will take further steps to homogonise details in this draft
2-12	A	0:0		Uncertainties quoted in this chapter are exclusively 1 sigma, whereas most of the rest of the report uses 2 sigma uncertainty ranges, and this is what is advised in the TS, Box 1.1. I would suggest using 2 sigma uncertainty ranges, for consistency with the rest of the	We now will use a 90% confidence interval throughout this chapter, and make this the convention for the

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				report. This issue could become particularly accute in the TS/ES, when plots using different uncertainty ranges may be shown side-by-side. There also seems to be some misunderstanding over the meaning of a 1 sigma uncertainty range - where the issue is intruduced in the footnote on pg 3 of the ES, the text states that 'RF agents with a high level of scientific understanding will have very likely have a RF that falls within the uncertainty range', whereas for a 1 sigma uncertainty range the probability is at best only P>66%, ie. likely. [Nathan Gillett (Reviewer's comment ID #: 84-3)]	report. For many forcings and other things not enough infomation is available to define higher confidence intervals
2-13	A	0:0		Uncertainty ranges on radiative forcings in this chapter are generally calculated as the sample standard deviation of a range of published estimates. This approach is not justified anywhere in the chapter, and it is not completely clear to me that this gives the best estimate of the true uncertainty. To take an extreme case, if the errors in each study were random and independent, then the appropriate estimate of the error in the true radiative forcing would be the standard error on the mean, a factor of sqrt(N-1) smaller: If I want to know the uncertainty in the mean temperature in Bergen based on a hundred individual measurements, the appropriate metric is the standard error on the mean. On the other hand, if all the studies contain the same systematic error, then the true error may be larger than the sample standard deviation. I think more thought should be given to this issue, and a justification of the sample standard deviation approach should be given somewhere. [Nathan Gillett (Reviewer's comment ID #: 84-4)]	We now will use a 90% confidence interval throughout this chapter, and make this the convention for the report. For many forcings and other things not enough infomation is available to define higher confidence intervals. Our approach will be clearly explained
2-14	A	0:0		This chapter discusses rates of change of CO2 in the atmosphere, and changes in radiative forcing, but nowhere does it state that the forcing is approximately logarithmic in the atmospheric concentration of CO2 (although this information is contained in the TAR, which is referenced). This seems to me to be a basic bit of science which must be communicated to the reader before he or she can relate changes in CO2 concentration (as shown e.g. in fig 2.3) and changes in forcing (as shown in fig 2.4). [Nathan Gillett (Reviewer's comment ID #: 84-9)]	Accepted. Added to section 2.3
2-15	A	0:0		A stylistic point: This chapter refers to 'TAR' throughout e.g. 'Since TAR several studies have attempted to..' without the definite article. Chapters 3 and 9 refer to 'The TAR', and indeed chapter 2 occasionally also uses the definite article e.g. pg 8, ln 54. I prefer 'the TAR'. [Nathan Gillett (Reviewer's comment ID #: 84-24)]	Accepted
2-16	A	0:0		no comments [Xueliang Guo (Reviewer's comment ID #: 93-2)]	Accepted
2-17	A	0:0		Overall much clearer than First-Order Draft; I have only minor comments. [Joanna Haigh (Reviewer's comment ID #: 95-1)]	Thank-you
2-18	A	0:0		This version is better than the prior one and is, generally speaking, quite good. I only have	Thank-you

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				a few minor comments. [Patrick Hamill (Reviewer's comment ID #: 97-1)]	
2-19	A	0:0		A very well written and accessible chapter [Gareth S. Jones (Reviewer's comment ID #: 121-6)]	Thank-you
2-20	A	0:0		The chapter tried to do two things and did it well. It provides RF change relative to 1750, and it also discusses the mechanisms that affect changes in concentrations of the greenhouse gases and their precursors. The latter is necessary for two reasons, to understand the trends of the changes in concentrations of the past and predict the trend in the future, and to explain how some of the forcing are calculated from model simulated changes in concentration from 1750. [Malcolm Ko (Reviewer's comment ID #: 135-1)]	Thank-you
2-21	A	0:0		The chapter's treatment of uncertainty is inconsistent and confusing. As a default option it uses the scientific convention of +/- two standard deviations. In the words of the authors of Chapter 3 (Pg 3-7, lines 18-19): "This allows us to assess what is really unusual." However, in some cases, e.g. for radiative forcing (RF), uncertainty range is given as +/- one standard deviation. While these cases are identified, no reason is given for departing from what WG I recognizes is accepted scientific practice. While careful readers of this report will be able to identify those cases where WG I has departed from the scientific norm, more casual readers will assume that WG I is following normal practice and be misled into thinking that the uncertainty range is half its actual size. Standard practice should be followed throughout this report and all uncertainty ranges be given as +/- two standard deviations. [Jeff Kueter (Reviewer's comment ID #: 137-43)]	We now will use a 90% confidence interval throughout this chapter, and make this the convention for the report. For many forcings and other things not enough infomation is available to define higher confidence intervals. Our approach will be clearly explained
2-22	A	0:0		General: I congratulate the Authors to a much-improved Chapter. It's well structured, reads soundly and is not overloaded with too much information. The syntheses section 9 is a very appreciated initiative. [Caroline Leck (Reviewer's comment ID #: 144-1)]	Thank-you
2-23	A	0:0		General: Please indicate if the reported mixing ratios are by volume/molar ppb(v) or by mass (ppb(m). A compromize might be to state which one is generally used at the beginning of the Chapter unless otherwise is notified. [Caroline Leck (Reviewer's comment ID #: 144-2)]	Agreed. Mole fractions used for all LLGHGs and mentioned at top of section 2.3..
2-24	A	0:0		Please make sure that all abbreviations used are properly explained. [Caroline Leck (Reviewer's comment ID #: 144-12)]	Accepted
2-25	A	0:0		This chapter shows how far the science has progressed in the last fifteen years. The clarity of the chapter will help demonstrate that the basic science of climate change is in fact mature. [Michael Manton (Reviewer's comment ID #: 157-1)]	Thank-you

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2-26	A	0:0		I would encourage IPCC to consider having only one solar physicists on the lead author team of such an important chapter. In particular since the conclusion of this section about solar forcing hangs on one single paper in which J. Lean is a co-author. I find that this paper, which certainly can be correct, is given too much weight. However, I will use the opportunity to stress that I have a lot of respect for the professional work of J. Lean. The inclusion of an expert on the cosmic ray cloud physics would improve this section. In fact a lot of the text and information in this section can be found in Lean et at. "Source contributions to new understaning of global change". Below, please find a list of relevant papers, most of them not listed (a few were obviously added the last 6 months after comments submitted autumn 2005): Solar Irradiance and Arctic Temperatures Reference Soon, W. W.-H. 2005. Variable solar irradiance as a plausible agent for multidecadal variations in the Arctic-wide surface air temperature record of the past 130 years. Geophysical Research Letters 32 L16712, doi:10.1029/2005GL023429. Marsh, N.D. and Svensmark, H. 2000. Low cloud properties influenced by cosmic rays. Physical Review Letters 85: 5004-5007. Nigel Marsh and Henrik Svensmark, Space Science Review, 94, 215-230, 2000. G., N. Marsh, G.A. Kovaltsov, K. Mursula, and O.G. Gladysheva, Latitudinal dependence of low cloud amount on cosmic ray induced ionization, Geophys. Res. Lett., Marsh, N., and H. Svensmark, Galactic Cosmic ray and El Niño-Southern Oscillation trends in ISCCP-D2 low-cloud properties, J. Geophys. Res., 108(D6), AAC 6-1, doi:10.1029/2001JD001264, 2003. Marsh, N., and H. Svensmark, Solar influence on earth's climate, Space Sci. Rev., 107, 317-325, 2003. Palle Bago, E. and Butler, C.J. 2000. The influence of cosmic rays on terrestrial clouds and global warming. Astronomy & Geophysics 41: 4.18-4.22. Svensmark, H. and Friis-Christensen, E. 1997. Variation of cosmic ray flux and global cloud coverage - A missing link in solar-climate relationships. Journal of Atmospheric and Solar-Terrestrial Physics 59: 1225-1232. Kniveton, D.R. and Todd, M.C. 2001. On the relationship of cosmic ray flux and precipitation. Geophysical Research Letters 28: 1527-1530. Marsden, D. and Lingenfelter, R.E. 2003. Solar activity and cloud opacity variations: A modulated cosmic ray ionization model. Journal of the Atmospheric Sciences 60: 626-636. Palle Bago, E. and Butler, C.J. 2000. The influence of cosmic rays on terrestrial clouds	Noted. Appreciate the sense of the comment and the acknowledgement of the expertise already there in the current author team. The LAs were chosen by IPCC bureau a long time ago, it would seem impractical now to formally include additional experts or additional authors at the current stage of the AR4 process. As a consequence of the zero-order, first-order and now the second-order drafts, the section has been seen, reviewed, revised by the authors and vetted by a number of solar and climate experts. Thus, the draft is in a mature state. Nevertheless, taking up the sense of the comment, and notwithstanding the late stage in the AR4 process (with 2 drafts already made publicly available and reviewed), attempts were specifically made by the chapter author team after receiving this comment to solicit, in a short period of time, suggestions from six solar experts (four responded), so as to improve upon the text..

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				and global warming. Astronomy & Geophysics 41: 4.18-4.22. Feynman, J. and Ruzmaikin, A. 1999. Modulation of cosmic ray precipitation related to climate. Geophysical Research Letters 26: 2057-2060. Shaviv, N.J. and Veizer, J. 2003. Celestial driver of Phanerozoic climate? GSA Today 13 (7): 4-10. Solanki, S.K., Schussler, M. and Fligge, M. 2000. Evolution of the sun's large-scale magnetic field since the Maunder minimum. Nature 408: 445-447. Svensmark, H. 1998. Influence of cosmic rays on Earth's climate. Physical Review Letters 22: 5027-5030. Lockwood, Nature 1999 399 437 Awalulia i JGR, 1997, 102, p 24229 Lockwood, 2003, Journal of Geophysical Research, Vol 108, No 13, s1128 Marsden, D. and Lingenfelter, R.E. 2003. Solar activity and cloud opacity variations: A modulated cosmic ray ionization model. Journal of the Atmospheric Sciences 60: 626-636. • Cliver, E. W., m. fl. 1998, GRL, 25, 1035 • Eichkorn, S., m. fl. 2002, GRL, 29 (14), 10.1029 • Fligge, M., 1999, Astronomy and Astrophysics, 346, 313-321 • Friis-Christensen, E., og Lassen, K., 1991, Science, 254, 698-700 • Hansen J. E., 2000, Space Sci. Rev., 94, 349-356. • Lassen, K., og Friis-Christensen, E., 2000, J. Geophys. Res., 105, 27493-27495 • Marsh, N., og Svensmark, H., 2003, J. Geophys. Res., • Reichel, R. P., m. fl. 2001, JGR, Vol. 106, No. A8, 15653 • Sonnemann, G., 1998, J. Atmos. Solar-Terr. Phys., 60, 1625-1630 • Thejll, P., og Lassen, K., 2000, J. Atmos. Solar-Terr. Phys., 62, 1207-1213 Bard, E., Raisbeck, G., Yiou, F. and Jouzel, J. 2000. Solar irradiance during the last 1200 years based on cosmogenic nuclides. Tellus 52B: 985-992. Pang, K.D. and Yau, K.K. 2002. Ancient observations link changes in sun's brightness and earth's climate. EOS, Transactions, American Geophysical Union 83: 481,489-490. Zhang, Q., Soon, W.H., Baliunas, S.L., Lockwood, G.W., Skiff, B.A. and Radick, R.R. 1994. A method of determining possible brightness variations of the sun in past centuries from observations of solar- type stars. Astrophysics Journal 427:	

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				L111-L114. Damon, P.E. and Laut, P. 2004. Pattern of strange errors plagues solar activity and terrestrial climatic data. EOS, Transactions, American Geophysical Union 85: 370, 374. Tourpali, K., Schuurmans, C.J.E., van Dorland, R., Steil, B. and Bruhl, C. 2003. Stratospheric and tropospheric response to enhanced solar UV radiation: A model study. Geophysical Research Letters 30: 10.1029/2002GL016650. [Govt. of Norway (Reviewer's comment ID #: 2018-42)]	
2-27	A	0:0		Very nice chapter. Well done! [Joyce Penner (Reviewer's comment ID #: 197-1)]	Thank-you
2-28	A	0:0		In general, the conclusions of this chapter inasmuch as aerosols are concerned is biased too heavily towards 2005 and 2006 results. The authors may put too much weight on the latest results. [Michel J. ROSSI (Reviewer's comment ID #: 220-12)]	Rejected. We think that the balance of the aerosols section is about right. There is no point in revisiting some of the earlier model results when they have been superceded, particularly when, as happens frequently, the model has a particular development line. It would do model development credit no favours if early results were included when more up to date versions of results from the same model are available.
2-29	A	0:0		I have reviewed all (I think!) previous drafts of this chapter, and it has been a pleasure to watch it mature into the present excellent state. I would still prefer it to be somewhat shorter, as it is rather overwhelming to read in a short period of time - but I am sure there will be many occasions in the future when I will welcome the detail. [Keith Shine (Reviewer's comment ID #: 236-13)]	Thank-you. We will trim a bit
2-30	A	0:0		I wonder whether the estimates of radiative forcing come too early in the document, and thus should be relegated to a later chapter. I think it would be more logical first to present direct observations - of atmospheric composition, surface and atmospheric climate, cryosphere, ocean, palaeo - and then discuss radiative forcing as a lead in to modelling, attribution and prediction. As things stand, Chapter 2 brings modelling in at an early stage to estimate radiaitive forcing, and then we go back to observations [Adrian Simmons (Reviewer's comment ID #: 242-24)]	Noted, but rejected. RFs are discussed with concentrations for context
2-31	A	0:0		It is stated in Box TS 1.1 (page TS-4, lines 41-42) that ± two standard deviations is the	We now will use a 90% confidence

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				default range "where values are specified . . . as a central estimate with a plus/minus range", and in Chapter 3 (page 3-7, lines 18-19), where the authors use ± two standard deviations because "This allows us to assess what is really unusual." However, radiative forcing (and other variables?) in general seems to make use of 1-sigma. A consistent approach should be used through out the report and it should be made clear in the chapter when the approach differs or whenever one sigma is used. Also, to add to the confusion, in some cases in this chapter [e.g. for radiative forcing (RF – Page 21, line 43)], the uncertainty range is given as ± one standard deviation with an additional, unexplained range that is asymmetric about the centroid. [Govt. of United States of America (Reviewer's comment ID #: 2023-47)]	interval throughout this chapter, and make this the convention for the report. For many forcings and other things not enough infomation is available to define higher confidence intervals. Our approach will be clearly explained
2-32	A	0:0		Organizations and government agencies periodically are reorganized or change names. The latest names should be reflected in this report. An example is periodic references to "CMDL", a former laboratory in NOAA. This now should be referenced as the NOAA Earth System Research Laboratory (NOAA/ESRL) or simply NOAA if the ultimate source is unambiguous. There are likely others. [Govt. of United States of America (Reviewer's comment ID #: 2023-48)]	Agreed. NOAA names changed to NOAA/ESRL or NOAA/GMD where relevant including Figure captions. In section 2.3 we use NOAA/GMD exclusively.
2-33	A	0:0		Due to unfortunate overlaps with teaching, field work, proposal due dates, and O3 Assessment reviews, I have been unable to devote the same attention to the second draft review as I gave the first draft. I have focussed mainly on Chapter 2 in both reviews, with an emphasis on the sections on trace gases, and I find these much improved. This is especially so with respect to the overview of experimental trace gas observations. The concerns expressed below are relatively minor compared to my earlier concerns. [Ray Weiss (Reviewer's comment ID #: 284-1)]	Noted
2-34	A	0:0		In a number of cases in the text of this chapter the chemical formulae for chemical compounds does not follow the IUPAC convention, but in other places it is correct. The convention requires that within each formula, or for each functional group, the order be carbon first, hydrogen second, and then the halogens in alphabetical order. In other words, bromine comes before chlorine, comes before fluorine, comes before iodine. [Ray Weiss (Reviewer's comment ID #: 284-2)]	Agreed. IUPAC nomenclature adopted where relevant.
2-35	A	0:0		Some of the figures and text still use the NOAA/CMDL acronym, rather than the new acronym of NOAA/GMD or NOAA/ESRL. I believe that GMD is the preferred replacement for CMDL, and that ESRL should not be used because it is a much larger organizational unit. [Ray Weiss (Reviewer's comment ID #: 284-4)]	Agreed but see reply to 2-32.
2-1314	B	0:0		Physiological forcing: this section focuses on perturbation of the climate system through changes in CO2 concentrations and their impacts on plant physiology. That is, the section is mostly focused on direct impacts of terrestrial vegetation. There should also be a	NOTED. This paper will be read, although since this is a single paper, and it may be difficult to separate

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				mention of feedbacks on marine aerosol production, both from a meteorological perspective (i.e. changing wind fields) for the inorganic sea-salt production and from a biological perspective for primary and secondary organic aerosol production resulting from plankton activity changes. Reference: O'Dowd, C.D., M.C. Facchini, F. Cavalli, D. Ceburnis, M. Mircea, S. Decesari, S. Fuzzi, Y.J. Yoon, and J.P. Putaud, Biogenically-driven organic contribution to marine aerosol, Nature, doi:10.1038/nature02959,. 2004. [Govt. of Ireland (Reviewer's comment ID #: 2025-1)]	direct CO2 effects (arguably a forcing) from meteorological effects (a feedback), it is difficult to justify introducing discussion of this new process at this stage. Passed to Chapter 7
2-36	A	1:1		The title of the Chapter is "Changes …in radiative forcing". But radiative forcing is pesented as the change since 1750, so the chapter heading should perhaps refer to radiative forcing, rather than changes in it. See also comment #1 [Adrian Simmons (Reviewer's comment ID #: 242-23)]	cHapter names cannot change. But we agree!
2-37	A	1:8	1:8	My affiliation should be France / U.K. [Olivier Boucher (Reviewer's comment ID #: 27-19)]	accepted
2-38	A	1:10		delete "," [Junying Sun (Reviewer's comment ID #: 261-1)]	accepted
2-39	A	1:11	1:11	There are two commas after Dave Keeling (USA). Remove one [Patrick Hamill (Reviewer's comment ID #: 97-2)]	accepted
2-40	A	1:14		change" ." To "," [Junying Sun (Reviewer's comment ID #: 261-2)]	accepted
2-41	A	1:30	1:30	Insert a Heading "2.3.1. "Atmospheric water vapour (H2O)" and renumber the rest 248 2-248 112 [VINCENT GRAY (Reviewer's comment ID #: 88-2)]	Rejected, no reason given
2-42	A	1:39	2:39	There should be separate Headings fpr the different kinds of aerosols; ordinary clouds, sulphate-based enhancement, black carbon, dust, sea salt. [VINCENT GRAY (Reviewer's comment ID #: 88-113)]	Rejeceted, no reason given
2-43	A	2:0		I think it better to add the CO2 distribution map from WMO GHG Bulletin (http://www.wmo.ch/web/arep/gaw/ghg/ghg-bulletin-en-03-06.pdf) to obtain a quick look of latitudional and seasonal CO2 distribution. [Takashi Maki (Reviewer's comment ID #: 153-2)]	Rejected from FOD based on reviewer comments.
2-44	A	2:3		In each chapter or major section a clear reference should be made at (1) the first occurrence of a LOSU term ("medium," etc) and (2) a qualitative term of uncertainty ("very likely," etc) directing the reader to the detailed description of these sets of terms and/or to the glossary. [Govt. of United States of America (Reviewer's comment ID #: 2023-49)]	Accepted. Terms will be cross referenced on first use
2-45	A	2:6	2:14	Make clear that RF takes account of human & natural drivers	Taken into account. We already do in

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				[Govt. of Australia (Reviewer's comment ID #: 2001-139)]	1st paragraph so their suggestion is not used. However, this text is added to section 2.2 for clarity
2-46	A	2:30	2:31	For policy reader phrase 'high level of scientific understanding' is hard to comprehend. Why is the matter not described in terms of 'confidence' taxonomy set out in TS-3 and -4 (and in Chapter 1, 1-26)? [Govt. of Australia (Reviewer's comment ID #: 2001-140)]	Rejected. LOSU is based on this taxonomy. More explaination added. Foot note also added explainnig wording choice
2-47	A	3:0		Exec Sum: 5 pages seems much too long. The 2 page summary for Ch. 9 seems to me a good example of the appropriate length and style [Isaac Held (Reviewer's comment ID #: 105-10)]	Accepted. Will be shortened to 3 pages. Some aspects of style will be adjusted
2-48	A	3:0		Executive summary. Precise values of the RF are given, but there is some imprecision on the timing. Some readers may also confuse statements on changes in RF as to whether the changes are real or due to more accurate estimates. [Michael Manton (Reviewer's comment ID #: 157-2)]	Wording will be tightened for clasrity of timing and re-evaluation
2-49	A	3:0		I do not understand why some of the executive bullet points are indented, others not, and again others have the 2nd and following paragraphs indented. For example it does not make sense that the CO2 bullet is indented, but the CH4 bullet not. It would help to having real bullets. [Thomas Peter (Reviewer's comment ID #: 198-1)]	Accepted. Indents will be dropped. Bold bullets retained
2-50	A	3:3	3:27	I appreciate that the RF definition is given with such a rigor - a really necessary improvement compared to TAR. However, as this definition comes across very technically you might want to consider making it a footnote (keeping ist full length) instead of the first bullet point of the exec summary. [Thomas Peter (Reviewer's comment ID #: 198-2)]	Accepted
2-51	A	3:4	2:5	Suggest deleting "resultant equilibrium" as this can be related to much more [William Hare (Reviewer's comment ID #: 99-3)]	Rejected. True but we choose the more formal definition for clarity
2-52	A	3:4	3:5	I don't see why radiative forcing relates any more to equilibrium temperature response than it does to transient temperature response. In fact the estimated uncertainty in transient climate response is smaller than the spread in climate sensitivity, which means the forcing is more directly related to the transient temperature response than to the equilibrium response. I suggest deleting 'equilibrium'. [Nathan Gillett (Reviewer's comment ID #: 84-5)]	Rejected. We choose the more formal definition for clarity
2-53	A	3:5	3:5	The word "relates" should be changed to "is related to" [Patrick Hamill (Reviewer's comment ID #: 97-3)]	accepted
2-54	A	3:5	9:10	The authors recognize that the use of RF to measure climate change is limited as it does not represent the overall climate response. This is especially true for absorbing aerosols.	Partly accepted. Text reworderd for clarity. Although, it is debatable

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				Black carbon, like greenhouse gases has positive forcing at TOA but opposite from greenhouse gases has a negative surface forcing. Therefore absorbing aerosols may significantly perturb surface temperature and other climate variables even in the case of zero RF. For absorbing aerosols, the surface forcing is needed to evaluate climate response. Therefore, the panel suggests [Govt. of United States of America (Reviewer's comment ID #: 2023-50)]	whether surface forcing can be used as a comparative measure to the extent that RF is.
2-55	A	3:5	9:10	1) adding at the end of the second sentence of the executive summary: "an exception is absorbing aerosols" [Govt. of United States of America (Reviewer's comment ID #: 2023-51)]	Meaning accepted. However, text modified differently to the suggested change. "the range of" changed to "most"
2-56	A	3:5	9:10	2) adding Page 5, Line 43: "...and potentially surface temperature." [Govt. of United States of America (Reviewer's comment ID #: 2023-52)]	Rejected. The current text is sufficient to explain this. Surface forcing is not necessarily an indicator of the energy blance change, which is needed for surface T changes
2-57	A	3:5	9:10	3) replace Page 7, line 14-15 by the sentence on Page 61, Line 27-29 which mentions than both RF and surface forcing are important to evaluating climate response, and should not be directly compared [Govt. of United States of America (Reviewer's comment ID #: 2023-53)]	Rejected. The text lines quoted here pertain to a slightly different point than the purpose in ES, where we want to make sure that the reader understands that the manner in which the RF is used as a measure of the equlibrium surface temperature response cannot be carried over to the surface forcing, despite the fact that surface flux changes are an important aspect of surfacee heat and moisture budgets.
2-58	A	3:5	9:10	4) adding to Page 9, Line 10, a sentence to note the limitation of RF in the case of absorbing aerosol, for which the impact on climate should rather be measured in terms of surface forcing. [Govt. of United States of America (Reviewer's comment ID #: 2023-54)]	Rejected Absorbing aerosol is discussed extensively eslsewhere, this parragraph is merely an introduction. We already state RF is not perfect, Also, use of surface forcing as a measure is highly debatable.
2-59	A	3:7	3:12	I suggest moving the definition of radiative forcing to the start of the paragraph. Otherwise you are telling us how radiative forcing is calculated before telling us what it is. [Nathan Gillett (Reviewer's comment ID #: 84-6)]	Accepted

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2-60	A	3:8	3:8	Change the semicolon to "and" [Patrick Hamill (Reviewer's comment ID #: 97-4)]	Accepted
2-61	A	3:11		The word "measured" is perhaps better replaced by "calculated", "evaluated" or "estimated". The RF as defined is not a physical quantity that one can go out and measure in the real atmosphere. [Adrian Simmons (Reviewer's comment ID #: 242-25)]	Accepted. "evaluated" now used
2-62	A	3:16	3:21	Replace this paragraph with 'The global mean temperature response to a given radiative forcing is approximately the same (to within 25%) for all the main anthropogenic and natural drivers of climate change', or something similar. As it stands, the paragraph may be a bit hard for non-specialists to understand. [Nathan Gillett (Reviewer's comment ID #: 84-7)]	Accepted. Good idea
2-63	A	3:16	3:21	I have no confidence whatsoever in this statement. Many "natural" contributors to radiative forcing are almost unknown. These include water vapour, clouds, indirect effects of many aerosols, chnages in atmospheric circulation, changes in the sun, plus possible "feedbacks", changes in ocean circulation. Your figure of ~25% is a gross underestimate. This paragraph is super optimistic. In any case, this is supposed to be about atmospheric constituents, not about models [VINCENT GRAY (Reviewer's comment ID #: 88-114)]	Rejected. Text misunderstood. We argue throught the text for an opposing view. Feedback disccusion is elsewhere. Feedback and forcing elements are mixed up in this comment – one needs to consider these separately, otherwise fairly hopeless to analyze climate change.
2-64	A	3:16	3:16	Replace "high" by "very low" [VINCENT GRAY (Reviewer's comment ID #: 88-115)]	Rejected, no reason given
2-65	A	3:16	3:19	State those forcing factors for which this general rule does not apply: "Exceptions to this rule include indirect aerosol forcing and forcing by black carbon." [Joyce Penner (Reviewer's comment ID #: 197-2)]	Rejected. No clear evidence for a non standard efficacy except in fairly hypothetical experiements (i.e. absorbing aerosol all in BL)
2-66	A	3:19	3:19	Does phrase 'since 1750 humans have …exerted….' really capture the intended meaning? Or is it more about the cumulative effect of human activity since 1750 on the composition of the atmosphere has exerted a warming influence? [Govt. of Australia (Reviewer's comment ID #: 2001-141)]	Accepted. Bullet reworded
2-67	A	3:20	3:20	Replace "designed as" by "capable of acting" [VINCENT GRAY (Reviewer's comment ID #: 88-116)]	Rejected. In fact RF can but has limited use here
2-68	A	3:20	3:21	This sentence ("RF is useful for comparing the global mean climate response of greenhouse gases, however, it cannot be as a general indicator of the regional and or seasonal aspects of climate response") and should be reworded to give more information: "Rt"RF is useful for comparing the global mean climate response of greenhouse gases, however, it cannot be as a general indicator of the regional and or seasonal aspects of climate response"	Comment seems to refer to an earlier draft?

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				[William Hare (Reviewer's comment ID #: 99-1)]	
2-69	A	3:23	3:27	The first sentence of this section would be more accurate (and consistent with the text on page 2-71, lines 20-22. If it read as follows: "The global warming potential (GWP) remains an appropriate metric for comparing the potnetial climatic impact of the emissions of different long lived climate gases." The two following caveats would make more sense in this context. [Steven Baughcum (Reviewer's comment ID #: 16-1)]	Paragraph now deleted
2-70	A	3:23	3:27	I suggest deleting this paragraph. At present it does not define 'Global Warming Potential', therefore the non-specialist reader will gain nothing from this. If this paragraph is retained, then a simplified definition should be included. [Nathan Gillett (Reviewer's comment ID #: 84-8)]	accepted
2-71	A	3:23	3:23	Replace "an approppropriate" by "a possible" [VINCENT GRAY (Reviewer's comment ID #: 88-117)]	Paragraph deleted
2-72	A	3:23	3:24	This sentence ("The Global Warming Potential (GWP) remains an appropriate metric for comparing the potential climate impact of the emissions of different forcing agents.") is not really correct as it appears to apply to all forcing agents. It is really only applicable in the way implied here (comparing the potential climate impact) for well mixed forcing agents eg the long lived greenhouse gases (see comments on Section below). Hence this needs to be reworded to say something like: "The Global Warming Potential (GWP) remains an appropriate metric for comparing the potential climate impact of the emissions of the well mixed greenhouse gases". An additional sentence is also needed to capture the qualifications needed on this statement eg "The choice of timeframe is an important variable with different consequences for long and shorter term climate change where a basket of emissions are weighted with GWPs calculated with different time horizons." [William Hare (Reviewer's comment ID #: 99-2)]	Paragraph deleted fo space saving
2-73	A	3:23	3:23	"appropriate" … I am not sure about this … how about "useful" or "accepted"? [Keith Shine (Reviewer's comment ID #: 236-14)]	Paragraph now deleted
2-74	A	3:26	3:27	This sentence is not very clear as it tries to say two things at the same time. 1/ Different metrics may be needed to describe different impacts (e.g. temperature, precipitation, runoff) and 2/ some metrics can be useful for one emission pathway of short-lived species but not for another one. [Olivier Boucher (Reviewer's comment ID #: 27-20)]	Para deleted
2-75	A	3:28	3:28	There needs to be a paragraph here with your conclusions on the possible changes in radiative forcing of water vapour [VINCENT GRAY (Reviewer's comment ID #: 88-120)]	Rejected, no reason given
2-76	A	3:29	3:44	Information on the further increase of LLGHGs seem to be highly policy relevant and therefore should be also reflected in the SPM and the TS.	Accepted, added to SPM,TS

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				[Govt. of Austria (Reviewer's comment ID #: 2002-43)]	
2-77	A	3:29	3:30	Add the year the RF of 2.59 refers to? [Guus Velders (Reviewer's comment ID #: 276-7)]	accepted
2-78	A	3:30	3:30	It is unacceptable to use one standard deviation as a measure of accuracy. You MUST dopuble the figure given to 2.59±0.52 [VINCENT GRAY (Reviewer's comment ID #: 88-118)]	Partially accepted 5-95% used
2-79	A	3:30	3:30	Insert after "led" some date. Is it 1750? [VINCENT GRAY (Reviewer's comment ID #: 88-121)]	Rejected. Timeframe noted at beginning of ES in RF definition
2-80	A	3:30	3:30	Add sfter "2.59±,52" , "These figures are from measurements solely over the ocean. There is very little information on concentrations or radiative forcing over land surfaces" [VINCENT GRAY (Reviewer's comment ID #: 88-129)]	Rejected. Please see comment 2-282
2-81	A	3:31	2:31	Add at end "over the oceans" [VINCENT GRAY (Reviewer's comment ID #: 88-130)]	Rejected. Please see comment 2-282
2-82	A	3:31	3:31	Footnote 1: "very likely have a RF that falls within the uncertainty range". Should just be "likely have a RF that falls within the uncertainty range." if the range is +/- 1 sigma, then there is only a 66% chance of falling within that range. [William Collins (Reviewer's comment ID #: 45-2)]	Accepted – though uncertainty range changed
2-83	A	3:31	3:32	Delete "Their RF has a high level of scientific understanding" This staement is contradicted by the quoted confidence limits which you have tried to minimise by using only one standard deviation. [VINCENT GRAY (Reviewer's comment ID #: 88-119)]	Rejected. Uncertainty carefully explained
2-84	A	3:31	3:38	The radiative forcing (RF) of CO2 is one of the most important issues of this paper. It is not appropriate to deal with its structural uncertainties only in a footnote It is questionable that the level of its scientific understanding is high. There is a lot of unsolved problem. The Goody-Yung analytic solution of the Schwarzchild radiation transfer equation is only approximative (Eddington- or semi-infinite approximation). In the theory there is still an inexplicable temperature discontinuity at the ground, which does not appear in the praxis. It can be debated also that the radiative forcing of CO2 'very likely' falls within the given uncertainty range. Theoretically it is unclear, why the Earth's greenhouse effect MUST BE 33 C, why the amount of water vapour in the air must be the same as it is. There are no sound computational basis behind Trenberth's statement on H2O / CO2 60% / 26% RF ratio (Chapter 1, Page 41, Line 21). Miskolczi (according to his HARTCODE high-resolution radiative transfer code gives 9% for CO2 RF relative to total. There are also unsolved problems with the Venus greenhouse effect (it is deeply debated if it were 'runaway'). See the forthcoming TellusB article for details. [MIKLOS ZAGONI (Reviewer's comment ID #: 300-2)]	Rejected. High level of understanding supported by literature.

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2-85	A	3:32	3:32	Add at end "but is within the uncertainty limits" [VINCENT GRAY (Reviewer's comment ID #: 88-122)]	Rejected. Comment unjustified
2-86	A	3:34	3:38	This section states that CO2 concentrations have increased more rapidly in the past decade than at any other time previously, and then goes on to discuss the raditaive forcing change. The unwary reader might assume that because the concentration has increased more rapidly, the forcing must have increased more rapidly, but this is not necessarily true, given the logarithmic dependence of the forcing on the concentration. This should be clarified. Could the functional dependence of the forcing on the main GHG concentrations be reproduced in the AR4 from the TAR? This is key background information. [Nathan Gillett (Reviewer's comment ID #: 84-10)]	Partially accepted. Clarified in text, not in ES
2-87	A	3:34	3:35	Replace "is" on line 34 to "1950s" on line 35 with "may have slightly increased, but so far it is not statistically significant" [VINCENT GRAY (Reviewer's comment ID #: 88-123)]	Rejected, no reason given
2-88	A	3:34	3:40	The quotation of various dates could confuse a reader as to whether the RF of 1.63 W/m2 is a change since since pre-industrrlal times or since 1996 or even 1950. [Michael Manton (Reviewer's comment ID #: 157-3)]	Accepted. Bullet reworded
2-89	A	3:36	3:36	After "1.63 +/- 0.16 Wm-2" add "since preindustrial times". I realise this is implicit in the definition, but since a lot of dates are mentioned in this paragraph it is worth repeating to avoid confusion. [William Collins (Reviewer's comment ID #: 45-1)]	Accepted. Bullet reworded
2-90	A	3:36	3:36	...been the warmest since 1500. Please add the relevant citation for that statement. Luterbacher et al. 2004 Luterbacher, J., Dietrich, D., Xoplaki, E., Grosjean, M., and H. Wanner, 2004: European seasonal and annual temperature variability, trends and extremes since 1500, Science, 303, 1499-1503. [Jürg Luterbacher (Reviewer's comment ID #: 151-2)]	Not relevent here
2-91	A	3:36	3:36	Although there is only one occurence of "an RF" (p.50 L.1) but 44 occurences of "a RF", I would still think that the former is correct. [Thomas Peter (Reviewer's comment ID #: 198-5)]	Accepted – An RF addopted – are we certain?
2-92	A	3:36	3:36	Changes in CO2...". Be more specific, e.g. "Increases in CO2 concentration... [Guus Velders (Reviewer's comment ID #: 276-8)]	accepted
2-93	A	3:37	3:37	Double the confidence figures to 1.63±0.32. You cannot get away with one standard deviation like this [VINCENT GRAY (Reviewer's comment ID #: 88-124)]	Accetped .Uncertainties changed
2-94	A	3:37	3:39	It looks curious that the RF due to land clearing could be half the total CO2 RF, while the uncertainty is twice the uncertainty in the total. I may not suggesting an error, but it may be worth stating that the uncertainty in land clearing is the major uncertainty in the total.	Agreed. Text to be rewritten to avoid confusing emissions with changes in mole fractions relevant to RF.

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				[Michael Manton (Reviewer's comment ID #: 157-4)]	Reworded using AR4 "likely" clause.
2-95	A	3:37	3:37	"which (VERY) LIKELY dominates that of all forcings" - the difference between the highest magnitude of the possible indirect aerosol effect and the lowest magnitude of the CO2 forcing is pretty small. [Keith Shine (Reviewer's comment ID #: 236-15)]	Agreed. Adopt "very likely".
2-96	A	3:38	3:38	Insert after "report" "except, possibly, water vapour" [VINCENT GRAY (Reviewer's comment ID #: 88-125)]	Rejected – no reason given
2-97	A	3:38	3:38	Add at end "but is within the stated confidence limits" [VINCENT GRAY (Reviewer's comment ID #: 88-126)]	Accepted. See reply to 2-87.
2-98	A	3:38	3:38	The sentence "This is … quoted the TAR." can be confusing in suggesting a correction rather than a rate of change. The point is that the TAR is not a significant date for rates. I would either delete the sentence, or quote the change over the last decade. [Michael Manton (Reviewer's comment ID #: 157-6)]	Accepted
2-99	A	3:39	3:41	Delete from "CO2" on line 39 to "century" on line 41.This statement makes no sense. "Emissions" do not hsve a an RF value. It can only arrise from CONCENTRATION changes. You do not indicate how the two may be related, so delete these sentences [VINCENT GRAY (Reviewer's comment ID #: 88-127)]	Agreed about confusion. See reply to 2-94.
2-100	A	3:40	3:41	Should specify whether the change in land-use contribution is 'absolute' (seems unlikely) or 'proportional' [ian Enting (Reviewer's comment ID #: 63-1)]	Accepted. Percentage used.
2-101	A	3:40	3:40	say, "percentage" or "fractional" contribution of land-use has decreased. The absolute amount has increased. [Chris Jones (Reviewer's comment ID #: 120-10)]	Accepted. See 2-100.
2-102	A	3:42	3:42	Replace "1999" by "1980" [VINCENT GRAY (Reviewer's comment ID #: 88-128)]	Reject. Not supported by the measurements.
2-103	A	3:42	3:42	Replace "more than 1.8" by "1.5". It is irresponsible to select only five years as representing a "trend", particularly as you do not mention the huge uncertainties. The alleged increase is not statistically significant [VINCENT GRAY (Reviewer's comment ID #: 88-131)]	Reject. Editorial decision to refer to post-TAR..
2-104	A	3:42	3:44	Delete the sentence from "Over the same period" to the end. It does not belong here as it refers to emissions which, I hope you know, is not the same as atmospheric concentrations.You need a separate section on "Emissions" with an explanation of the rtelationshoip between "emissions" and "atmospheric concentrations" [VINCENT GRAY (Reviewer's comment ID #: 88-132)]	Reject but see reply to 2-94.
2-105	A	3:43	3:43	An explanation needs to be included as to why 'Figures '6.5 to 7.2 GtC' are not the same as in Table TS-1?	Accepted. Numbers are now consistent

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				[Govt. of Australia (Reviewer's comment ID #: 2001-142)]	
2-106	A	3:44	3:44	"anticipated" may be a better word than "considered" here [Olivier Boucher (Reviewer's comment ID #: 27-21)]	Accepted. ES bullet reworded
2-107	A	3:44	3:44	Add a section dealing with carbon dioxide emissions [VINCENT GRAY (Reviewer's comment ID #: 88-133)]	Rejected – emissions already covered
2-108	A	3:44	3:44	It would be useful (if politically inappropriate) to ascribe a cause to the higher emissions in recent times [Michael Manton (Reviewer's comment ID #: 157-5)]	Attribution to China in section, but not ES – as not being sent for Govt review again
2-109	A	3:46	3:49	General: An example of comment #2 is shown. [Caroline Leck (Reviewer's comment ID #: 144-3)]	Noted
2-110	A	3:46	3:46	Is it worth flagging that the apparent sudden increase in methane, wrt TAR, is due to a recalibration … this really threw me when I saw the value. [Keith Shine (Reviewer's comment ID #: 236-16)]	Accepted. Reworded
2-111	A	3:47	3:47	Authors need to say if this includes indirect forcing of CH4 [Govt. of Australia (Reviewer's comment ID #: 2001-143)]	Accept. Will state that indirect effects not included because these are captured in the RFs for ozone and all gases destroyed by OH
2-112	A	3:47	3:47	Double the confidence figures to "0.48±0,10" [VINCENT GRAY (Reviewer's comment ID #: 88-134)]	5-95% confidence limits used
2-113	A	3:47	3:47	Insert after "LLGHG RF" "Methane concentrations are only measured over the oceans and little is known of their concentrations over land. It was only discovered recently that significant quantities of methane are emitted from forests." [VINCENT GRAY (Reviewer's comment ID #: 88-137)]	Rejected. Statements are adequate
2-114	A	3:49	3:50	Remove the space between the word "decade" and the period. [Patrick Hamill (Reviewer's comment ID #: 97-5)]	Accepted
2-115	A	3:50	3:50	Insert after "negative" "and if current trends continue concentrations will fall at an increasing rate" [VINCENT GRAY (Reviewer's comment ID #: 88-135)]	Rejected. Future predictions not covered here
2-116	A	3:50	3:50	Footnote: This is most unsatisfactory. Either you are able to provide 95% conficence figures (NOT only one standard deviation) from some acceptable statistical procedure, or you are guessing, and you should not pretend that these guesses have some sort of statistical significance when they do not.ALL the "Levels of scientific understanding" are sspect as they are made by the people who produce the figures. You should use honest guesswork terms like "thought to be". "possible", may be" and so forth 274 2-274 138 [VINCENT GRAY (Reviewer's comment ID #: 88-135)]	Rejected. Terminology carefully explained

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2-117	A	4:1	4:2	Replace with "The most likely reason is the continued draining of wetlands, which are the lsrgest source of methan emissions". This is a better reason than the one you give [VINCENT GRAY (Reviewer's comment ID #: 88-136)]	Rejected. Not supported by literature
2-118	A	4:2	4:2	"small inferred trends" … but line 22 on the same page says "no detectable change" in OH [Keith Shine (Reviewer's comment ID #: 236-17)]	Accept. Text "small—trends" now reads "negligible net long term change"
2-119	A	4:6	4:7	What is the uncertainty on this radiative forcing? [Nathan Gillett (Reviewer's comment ID #: 84-11)]	Gunnar Myhre et al to provide this. It is dominated by radiation not concentration uncertainties.
2-120	A	4:6	4:6	Insert after "contributed" "about" [VINCENT GRAY (Reviewer's comment ID #: 88-139)]	Reject. We are adding error bars (see 2-119 reply)
2-121	A	4:7	4:7	CFC-12 remaining the third… [Olivier Boucher (Reviewer's comment ID #: 27-22)]	Reject. We prefer to keep the "as".
2-122	A	4:13	4:14	Give brief details of the sources of nitrous oxide. At present the text states that the emissions come mainly from tropical regions, without saying from what processes (agriculture?). [Nathan Gillett (Reviewer's comment ID #: 84-12)]	Reject. Exact processes not definitely identified but nitrogen bacteria in soils and ocean are the likely cause. Discussed later in section 2.3.3.
2-123	A	4:13	4:13	Double the confidence limits to 0.16± 0.04 [VINCENT GRAY (Reviewer's comment ID #: 88-140)]	5-9% confiidence used
2-124	A	4:13	4:14	Transfer the last sentence to a section on "emissions" You don't seem to know that "concentrations" and "emissions" are different [VINCENT GRAY (Reviewer's comment ID #: 88-141)]	Accepted in part. After "regions" we have added "in controlling the observed concentrations."
2-125	A	4:14	4:14	Ascribe a cause to the role of the tropics in nitrous oxide [Michael Manton (Reviewer's comment ID #: 157-7)]	Noted. See reply to 2-122.
2-126	A	4:16	4:19	I assume these numbers are for concentrations not emissions. [Olivier Boucher (Reviewer's comment ID #: 27-23)]	Accept. Added "Concentrations of " before "many".
2-127	A	4:16	4:16	I assume this is not a complete list of the Kyoto protocol gases. Either the Kyoto Protocol gases need to be defined somewhere, or just say 'Many greenhouse gases'. [Nathan Gillett (Reviewer's comment ID #: 84-13)]	Reject. This lists all the relevant gases showing very rapid increases.
2-128	A	4:16	4:17	"Many of the Kyoto Protocol gases". Change to "Many of the fluorine containing Kyoto protocol gases", CO2, CH4 and N2O are also Kyoto gases. [Guus Velders (Reviewer's comment ID #: 276-9)]	Accept. Added "fluorine-containing".
2-129	A	4:17	4:17	Insert after "are:", "about" [VINCENT GRAY (Reviewer's comment ID #: 88-142)]	Reject. Factors are well measured to the number of significant figures quoted.
2-130	A	4:19	4:19	Insert after "was". "about"	Reject. But Gunnar Myhre to provide

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				[VINCENT GRAY (Reviewer's comment ID #: 88-143)]	error bar on RF.
2-131	A	4:21	4:21	The concentration of OH, a key …, has … [Olivier Boucher (Reviewer's comment ID #: 27-24)]	Accept. "Concentrations of" added before "OH".
2-132	A	4:21	4:28	It would be worthwhile to add an adjective or two (like short-lived) to help explain the variability of OH. The uncertainty in the global level of OH could also be explicit, to help explain the 'no detectable net change'. It also explains at least partly why methane is hard. [Michael Manton (Reviewer's comment ID #: 157-8)]	Reject. Causes are not easily condensed into a couple of words but details are addressed in section 2.3.5.
2-133	A	4:23	4:23	You may want to change "the major producer" to "a major producer". [Olivier Boucher (Reviewer's comment ID #: 27-25)]	Accept. Text changed.
2-134	A	4:23	4:24	Reaction with OH is the… major producer for some aerosols'. Which ones? Give more details. [Nathan Gillett (Reviewer's comment ID #: 84-14)]	Accept. Add "(sulfates, nitrates, some organics)" after "aerosols".
2-135	A	4:27	4:27	"This could have …" - both vague and confusing, as it appears to be referring to the future. If it had an affect, then it will have had it. [Keith Shine (Reviewer's comment ID #: 236-18)]	Accept. Change "could have" to "has".
2-136	A	4:30	4:55	It should be made clear that all the RF estimates are valid for the same date. [Michael Manton (Reviewer's comment ID #: 157-9)]	This is already made clear in the footnote on page 1
2-137	A	4:30	4:31	It could be misleading to talk about a decrease of ozone RF when this quantity is negative. Does this mean ist absolute value will increase? [Thomas Peter (Reviewer's comment ID #: 198-3)]	Accepted. "weaker" now used. There are now 2 'weaker" in the bullet; will re-craft bullet appropriately.
2-138	A	4:30		Define "satellite observations era" [Richard Soulen (Reviewer's comment ID #: 248-32)]	Accepted
2-139	A	4:31	4:31	Double the confidence limits, to two standard deviations "-0.03 ± 0.14" [VINCENT GRAY (Reviewer's comment ID #: 88-144)]	Confidence level increased to 5-95%
2-140	A	4:32	4:35	This seems to suggest that ozone recovery is already occuring, whereas I thought the consensus is that decreases in ozone may have slowed, but a clear recoverey is not yet detectable. The latest draft of the next WMO ozone assessment has - 'Over the past 10 years, between 60N and 60S, the decline of… total ozone has weakened, and in some latitude and altitude regions ozone has increased', and 'Since the late 1990s, ozone depletion over Antarctica has neither worsened no improved'. At a minimum, I would suggest inserting 'is no longer decreasing and' after 'global stratospheric ozone' and deleting 'still' after 'Antarctic ozone hole'. [Nathan Gillett (Reviewer's comment ID #: 84-15)]	Bullet substantially shortened. Text on recovery now dropped
2-141	A	4:32	4:32	Surely "with a medium level of scientific understanding".means that the confodence limits are too narrow. So what is the point of them? Delete them [VINCENT GRAY (Reviewer's comment ID #: 88-146)]	Rejected. Our apporach is consistent with the error analysisin Section 2.9

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2-142	A	4:34	4:34	the statement about "may be showing signs of recovery" is not in line with the statements in SROC, 2005 (Pyle et al., 2005). If this statement is to be made here I believe that it should be clearly stated in the chapter that a) recovery has been detected and b) on which new information since SROC this statement can be made. An alternative would be to use wording in accordance with SROC 2005. [Rolf Müller (Reviewer's comment ID #: 181-35)]	Accepted. Statement on recovery now dropped
2-143	A	4:35	4:35	"antarctic spring" would be better than "spring" only. [Tiziano Colombo (Reviewer's comment ID #: 46-2)]	Text now dropped
2-144	A	4:38	4:39	I would certainly support the use of observations of stratospheric ozone. I believe that the transport across the tropopoause in models can be easily subject to model artefacts, especially in the tropics. This is not saying the models are bad, it is just a very difficult process to simulate in a numerical model. [Rolf Müller (Reviewer's comment ID #: 181-38)]	Noted. Unfortunately, new results based on observations were not available in time
2-145	A	4:39	4:39	Line needs to be eliminated in final draft. [Thomas Peter (Reviewer's comment ID #: 198-4)]	Accepted
2-146	A	4:41	4:41	An explanation for the asymmetrical uncertainty band needs to be provided. [Lenny Bernstein (Reviewer's comment ID #: 20-47)]	Footnote added to add explaination
2-147	A	4:41	4:41	The notation for the range is not clear. I assume you mean a range of [0.25,0.50]. [Olivier Boucher (Reviewer's comment ID #: 27-26)]	Footnote added to add explaination
2-148	A	4:41	4:41	Double confidence limits: to two standarddeviations "0.35 (+0.3/-0.2)" [VINCENT GRAY (Reviewer's comment ID #: 88-145)]	5-95% now used
2-149	A	4:41	4:42	Surely "with a medium level of scientific understanding".means that the confodence limits are too narrow. So what is the point of them? Delete them [VINCENT GRAY (Reviewer's comment ID #: 88-147)]	Rejected – confidence limits alrady expalined
2-150	A	4:41	4:46	Most readers will not be familiar with asymetrical uncertainty bands. The Executive Summary needs to provide either an explanation or a reference to the explanation provided in the body fo the chapter. [Jeff Kueter (Reviewer's comment ID #: 137-44)]	Accpeted – footnote added
2-151	A	4:41		An explanation for the asymmetrical uncertainty band needs to be provided. Here and elsewhere, any uncertainty that is not the default ± 2-sigma requires an explanation or cross-reference. [Govt. of United States of America (Reviewer's comment ID #: 2023-55)]	Accpeted – footnote added
2-152	A	4:42	4:43	Vary in space or time? [Nathan Gillett (Reviewer's comment ID #: 84-16)]	Bullet will be shortened
2-153	A	4:45	4:45	It is implied, I guess, that increased complexity means "better". Why not just say "better" if that is what is meant	accepted

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				[Isaac Held (Reviewer's comment ID #: 105-11)]	
2-154	A	4:48	4:48	Replace "likely" with "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-148)]	Rejected –no reason given
2-155	A	4:48	4:48	Double the confidence limits to two standard deviations. "0.07 ± 0.1" [VINCENT GRAY (Reviewer's comment ID #: 88-149)]	5%-95 confidence limints now used
2-156	A	4:48	4:48	Surely "with a low level of scientific understanding".means that the confodence limits are too narrow. So what is the point of them? Delete them [VINCENT GRAY (Reviewer's comment ID #: 88-150)]	Our error approach is clearly outlined in section 2.9, we will refer to this on first use
2-157	A	4:53	4:53	"understanding" instead of "undersanding". [Tiziano Colombo (Reviewer's comment ID #: 46-3)]	accepted
2-158	A	4:53	4:55	This sentence is presumed to refer to stratospheric water but does not reassert so. If so, it is incorrect in that some of the change arises from methane increase and so is an RF. In any case, the rest of the change arises from unknown causes that may or may not be a response [Howard K. Roscoe (Reviewer's comment ID #: 219-1)]	Accepted. Sentence dropped
2-159	A	4:55		Here it is explicity stated, of water vapour, that "it is therefore considered as part of the climate response, rather than a[n] RF". But this statement confusingly occurs in a section that begins with another statement identifying the RF due to the component of water vapour in the stratosphere, which may result from both direct (methane emission changes) and indirect (changes in strat/trop exchange) effects. Moreover, at the end of the very next page, 2-5, there is talk of a small RF of tropospheric water vapour due to irrigation. Part of the RF of other elements of atmospheric composition (e.g. ozone) is in fact a climate response rather than a direct forcing of anthropogenic origin, but this part is not estimated. This is all a bit unsatisfactory. Readers may wish at this point to be told the radiative forcing due to water vapour changes, even if it is largely an indirect rather than a direct effect of anthropogenic changes. [Adrian Simmons (Reviewer's comment ID #: 242-26)]	Accepted. Sentence dropped. RF of water vapour not known
2-160	A	5:0		There are three conclusions about radiative forcing from aerosols and associated uncertainty that appear on pages 2-5 and are elaborated later in the chapter: [Govt. of United States of America (Reviewer's comment ID #: 2023-56)]	Noted
2-161	A	5:0		1) A combined total direct aerosol RF is given as –0.5 ± 0.4 Wm–2, with a low level of scientific understanding… [Govt. of United States of America (Reviewer's comment ID #: 2023-57)]	Noted
2-162	A	5:0		2) The cloud-albedo RF due to aerosols (also referred to as first indirect or Twomey effect) is estimated to be –0.9 ± 0.5 Wm-2, with a very low level of scientific understanding. Other processes related to aerosol-cloud interactions remain highly	Noted

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				uncertain and there is a very low level of scientific understanding of these processes… [Govt. of United States of America (Reviewer's comment ID #: 2023-58)]	
2-163	A	5:0		3) Observations and models indicate that both the direct effect of aerosols and aerosol-cloud interactions lead to a substantial reduction of shortwave radiative flux at the surface which alters the surface heat and moisture budgets… [Govt. of United States of America (Reviewer's comment ID #: 2023-59)]	Noted
2-164	A	5:0		At the suggestion of one of the authors of Chapter 2, several scientists undertook a detailed elicitation of 24 experts about uncertainty in radiative forcing by aerosols. The paper was peer reviewed and accepted for publication in Climatic Change before the literature cut-off date. (Granger et al, 2006, Elicitation of expert judgment of aerosol forcing, (Climate Change DOI: 10.1007/s10584-005-9025-y.) We recommend that IPCC compare their uncertainties in RF to the uncertainties reported in this paper, which used an independent approach. [Govt. of United States of America (Reviewer's comment ID #: 2023-60)]	Accepted – paper referenced but not highlighted and results do not influcence our error analyses
2-165	A	5:1	5:1	Double the confidence figures to two standard deviations:" -0.5 ± 0.8" [VINCENT GRAY (Reviewer's comment ID #: 88-151)]	Noted. We now will use a 90% confidence interval throughout this chapter, and make this the convention for the report. For many forcings, not enough infomation is available to define higher confidence intervals. Our approach will be clearly explained
2-166	A	5:1	5:1	Surely "with a low level of scientific understanding".means that the confodence limits are too narrow. So what is the point of them? Delete them [VINCENT GRAY (Reviewer's comment ID #: 88-152)]	Rejected. LOSU and confidence limits can not be translated into each other.
2-167	A	5:1	5:2	I think these mentions of "low scientific understanding" and, on line 24 "very low level" is really an overstatement--and use of these terms causes more problems than benefits. The available science bounds these terms reasonably well, at least relative to the size of the GHG forcing and we should not be expressing all that we don't know in the way done here. We basically have the general magnitude, but do not understand anywhere near all the processes involved, but having a sense of things is quite helpful. I particularly think it unhelpful to have these terms included in the bar graph of the relative RF of the various species as the terms do not seem to correlate well with the overall importance of the uncertainty to the problem at hand (e.g., having low understanding of contrails does not seem to really matter, where low understanding of aerosols certainly does). [Michael MacCracken (Reviewer's comment ID #: 152-254)]	Noted. The LOSUs have been redefined. However they will be found on the bar graph to allow the community to monitor progress relative to TAR; and to express the general scientific understanding of the forcing, both in terms of the magnitude and the understanding of the processes leading to the quantification.We use "Evidence" and "Consensus" to express the understanding. These are important, alongside the numerical best estimate

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					and uncertainty.
2-168	A	5:1	5:40	If one simply adds the direct and indirect aerosol effects together you get a significantly larger negative forcing than deduced by inverse methods from global temperature data (see e.g. Forest et al, GRL, 2006, sections 9.2.1 (chapter 9)). Need some statement on total net aerosol forcing in this ES that is in harmony with Chapter 9 inverse studies. [Ronald Prinn (Reviewer's comment ID #: 202-1)]	Rejected. The author of the chapter cannot comment under the rules
2-169	A	5:12	5:12	Double the confidence limits to two standard deviations, to "-0.4 ± 0.4" [VINCENT GRAY (Reviewer's comment ID #: 88-153)]	Noted. We now will use a 90% confidence interval throughout this chapter, and make this the convention for the report. For many forcings, not enough infomation is available to define higher confidence intervals. Our approach will be clearly explained
2-170	A	5:12		Be consistent throughout the Chapter, please replace "species" with constituents or compounds. Species belong to the world of animals. [Caroline Leck (Reviewer's comment ID #: 144-4)]	Rejected. Species and Components are used both widely in the literature and are readily understood.
2-171	A	5:13	5:13	Double the confidence levels: to two standard deviations "-0.1 ± 0.2", and "+0.2 ± 0.2" [VINCENT GRAY (Reviewer's comment ID #: 88-154)]	Noted. We now will use a 90% confidence interval throughout this chapter, and make this the convention for the report. For many forcings, not enough infomation is available to define higher confidence intervals. Our approach will be clearly explained
2-172	A	5:14	5:14	Double confidence limits to two standard deviations "0.0 ± 0.2", "-0.1 ± 0.2", "-0.1 ± 0.4" [VINCENT GRAY (Reviewer's comment ID #: 88-155)]	Noted. We now will use a 90% confidence interval throughout this chapter, and make this the convention for the report. For many forcings, not enough infomation is available to define higher confidence intervals. Our approach will be clearly explained
2-173	A	5:23	5:23	Double the confidence limits to two standard deviations:Double the confidence limits to two standard deviations "-0.9 ± 1.0" Surely "with a very low level of scientific understanding".means that the confodence limits are too narrow. So what is the point of them? Delete them [VINCENT GRAY (Reviewer's comment ID #: 88-156)]	Noted. We now will use a 90% confidence interval throughout this chapter, and make this the convention for the report. For many forcings, not enough infomation is available to define higher confidence intervals. Our approach will be clearly explained

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2-174	A	5:27	5:27	add after GCM (General Circulation Model) [Claudia Marcolli (Reviewer's comment ID #: 158-1)]	accepted
2-175	A	5:28	5:30	Re-phrase to make clearer. [Nathan Gillett (Reviewer's comment ID #: 84-17)]	accepted
2-176	A	5:32	5:34	The points raised in 10) above will affect the wording of the this summary statement. My feeling is that it is too strong. [Graham Feingold (Reviewer's comment ID #: 69-17)]	Wording changed in accordance with comment top play down constraint
2-177	A	5:33	5:33	I find it strange to specify for the first time a best estimate for RF and then to say that the actual RF is LIKELY to be lower. If so, why not specify the most likely value right away? [Thomas Peter (Reviewer's comment ID #: 198-6)]	Accepted -text changed
2-178	A	5:35	5:35	The text states that there are only a few studies of the cloud albedo affect based on satellite data, and goes on to say that this introduces significant uncertainties in the RF estimate. If the studies are good ones, with appropriate treatment of errors, then I do not see why this should introduce uncertainties. Indeed a single study with good data and a suitable treatment of errors should be all one needs. I think this may relate to this chapter's calculation of uncertainties based on the sample standard deviation of published estimates, which I think may not always be the best approach. [Nathan Gillett (Reviewer's comment ID #: 84-18)]	Accepted – text changed. We try and keep uncertanity calcs transparent
2-179	A	5:45	5:57	I am not sure how the uncertainties are propagated here: -0.2+/-0.3 and +0.1with a factor of 3. This should give a large uncertainty range than -0.1+/-0.3. [Olivier Boucher (Reviewer's comment ID #: 27-29)]	Bullet now splits estimates
2-180	A	5:47	5:47	Double confidence limits to two standard deviations "-0.1 ± 0.6" Surely "with a very low level of scientific understanding".means that the confodence limits are too narrow. So what is the point of them? Delete them [VINCENT GRAY (Reviewer's comment ID #: 88-157)]	5-95% confidence used
2-181	A	5:52	5:52	Hasn't most reforestation (e.g. in the easter US) occurred through natural regrowth rather than planting trees? I suggest replacing 'tree planting' with 'reforestation'. [Nathan Gillett (Reviewer's comment ID #: 84-19)]	accepted
2-182	A	5:52	5:52	LIKELY increase [Keith Shine (Reviewer's comment ID #: 236-19)]	accepted
2-183	A	5:53	5:53	Double the confidence limits to two standard deviations. "-0.2 ± 0.6" Surely "with a low level of scientific understanding".means that the confodence limits are too narrow. So what is the point of them? Delete them [VINCENT GRAY (Reviewer's comment ID #: 88-158)]	5-95% now used
2-184	A	5:55	5:55	Double the confidence limits to two standard deviations. Replace "factor of three" with	5-95% now used

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				":factor of six" Surely "with a low level of scientific understanding".means that the confodence limits are too narrow. So what is the point of them? Delete it [VINCENT GRAY (Reviewer's comment ID #: 88-159)]	
2-185	A	5:56	5:57	In light of the statement in Section 2.5.6 that the reduction in water vapour flux due to deforestation is estimated to be 3 x larger than the flux due to irrigation, it is not justified to mention irrigation radiative forcing without mentioning the other. However, the radiative forcing from reduced transpiration from forests is swamped by uncertainties in the radiative forcing due to surface albedo changes, so it - and hence the irrigation radiative forcing - is not worht mentioning. Furthermore, the non-radiative forcing from irrigation is much larger but is not mentioned here either. It is not consistent to mention a small forcing but not mention much larger related forcings. [Danny Harvey (Reviewer's comment ID #: 101-1)]	Reference to irrigation dropped
2-186	A	5:56	5:56	insert "be" [Michel J. ROSSI (Reviewer's comment ID #: 220-13)]	Text reworded
2-187	A	6:5	6:5	Replace 'human energy production' with 'human activitites'. 'Engergy production' sounds like power generation only. [Nathan Gillett (Reviewer's comment ID #: 84-20)]	Reject – standard notation used
2-188	A	6:8	6:17	If something is just 0.01 W/m2, what difference does it make whether it is 3 times smaller than the previous report or not. The executive summary statement could just be that this effect is now considered to be negligible. [Isaac Held (Reviewer's comment ID #: 105-12)]	Reject. RF is forecast to grow so important to quote nmber
2-189	A	6:9	6:9	Double the confidence limits to two standard deviations. Replace "two" with ":four" Surely "with a low level of scientific understanding".means that the confidence limits are too narrow. So what is the point of them? Delete them [VINCENT GRAY (Reviewer's comment ID #: 88-160)]	5-95% used
2-190	A	6:9	6:9	Does "also" mean "in addition"? I think so, needs to be clarified. [Thomas Peter (Reviewer's comment ID #: 198-7)]	Text calrified
2-191	A	6:10	6:10	TAR reported their best estimate of the contrail RF of 0.02 W/m2 (TAR, p.379), which by a factor of 2 (not by a factor of 3-4) larger than the best value of 0.01 W/m2 quoted in AR4. [Michael Danilin (Reviewer's comment ID #: 55-15)]	aCcepted – we havecorrected ES
2-192	A	6:12	6:12	It is too strong to say that ''observational studies provide evidence…", since to my best knowledge only one study (Mannstein and Schumann, 2005) addressed this issue. Better to re-write the beginning of this sentence as follows: ''Observational studies suggest ..." [Michael Danilin (Reviewer's comment ID #: 55-16)]	Bullet shortened and reworded although WE belive wording currently accuate
2-193	A	6:12	6:26	This bullet on surface forcing comes across in a similr way as the the first bullet on RF.	Rejected. Box idea is a good one, but

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				They are very helpful, should not be shortened, but are at the same time very technical and are not suited as the leading and trailing bullets of the exec summ. I would put them into a big prominent box right after the exec summ. This will not take away importance but will help refering to them. [Thomas Peter (Reviewer's comment ID #: 198-9)]	will be a distraction and would be inconsistent with the attention this subject receives in this chapter. Will revise the bullets slightly to simplify seemingly technical words and phrases.
2-194	A	6:19	6:20	This chapter is not about warming (climate response), it is about RF. You should quantify the likelihood of the RF being greater than 0 or some other positive threshold. I doubt for instance that a RF of 0.1 Wm-2 would necessarily imply a net warming since 1750. [Olivier Boucher (Reviewer's comment ID #: 27-30)]	Accepted –text reworded
2-195	A	6:19	6:25	This paragraph could come at the very beginning. [European Commission (Reviewer's comment ID #: 2008-10)]	accepted
2-196	A	6:19	6:25	Consider moving this paragraph to the beginning of the Executive Summary. [Govt. of Germany (Reviewer's comment ID #: 2011-100)]	accepted
2-197	A	6:19	6:19	Replace "very likely" with "probably" [VINCENT GRAY (Reviewer's comment ID #: 88-161)]	Reejcted –not reason given
2-198	A	6:19	6:19	Insert after "climate". This includes urban influences and ebergy emissions, and may not include effects of human greenhouse gas emissions" 298 2-298 162 [VINCENT GRAY (Reviewer's comment ID #: 88-161)]	Rejected – not supported in literature
2-199	A	6:19	6:25	An explanation needs to be provided as to how RF is weighted by LOSU. This will not be "intuitively obvious to the casual observer." [Jeff Kueter (Reviewer's comment ID #: 137-45)]	Weighting no longer done
2-200	A	6:19		The phrase, "Since 1750, humans have very likely exerted a net warming influence on climate." should be immediately followed by the statement: "Moreover, it is likely that this warming influence exceeds +0.8 W/m2, which has been suggested as a minimum threshold for any combination of natural and anthropogenic forcings to be capable of explaining the observed, industrial-era increase in global-mean surface temperature." [Govt. of United States of America (Reviewer's comment ID #: 2023-61)]	This attribution is not made in Chapter 2 but statement added in Chapter 9
2-201	A	6:20	6:20	Double confidence figures to give two standard deviations "2.9 ± 0.6" [VINCENT GRAY (Reviewer's comment ID #: 88-163)]	5-95% used
2-202	A	6:20	6:20	Revise the magnitude of the total error (Is it 0.4 instead of 0.3?) [Govt. of Spain (Reviewer's comment ID #: 2019-18)]	Error corrected
2-203	A	6:21	6:25	I would have expected a second number for the anthropogenic RF that includes aerosol direct, cloud-albedo, and land-use albedo RF [Claudia Marcolli (Reviewer's comment ID #: 158-2)]	Number added
2-204	A	6:22	6:24	You are following a very dangerous path here by mixing objective and subjective	Accepted. Now ojective and

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				statements. I have no idea what a quantity weighted by its level of scientific understanding (high, medium, low, very low) is. The uncertainty arising from the low and very low level of scientific understanding RF is already accounted for by setting a rather broad uncertainty range, so I do not see the need for double counting the uncertainty as uncertainty and a weighting by level of scientific understanding, whatever this means. I am afraid that there might be no other way than saying "1/ the net forcing from RF with medium to high LOSU is very likely to be larger than xx Wm-2, and 2/ the net forcing from all anthropogenic RF is likely to be positive (if this is what is shown on page 67 and figure 2.24)". It is then up to chapter 9 to make a more definite statement about the likelihood of a human impact on global warming. [Olivier Boucher (Reviewer's comment ID #: 27-31)]	subjective satements seperated
2-205	A	6:23	6:23	An explanation of how RF values have been weighted by LOSU needs to be provided. [Lenny Bernstein (Reviewer's comment ID #: 20-48)]	No longer weighted
2-206	A	6:23	6:23	The statement "… and weighted by level of scientific understanding …" is unclear in respect to how the different RF PDFs were actually combined. If the scientific understanding is lower, this is probably reflected in a wider uncertainty range, but was actually a weighting of these different uncertainty ranges applied in terms of "counting CO2 forcing twice and aerosol forcing only once" ? Please clarify. [Govt. of Germany (Reviewer's comment ID #: 2011-101)]	No longer weighted
2-207	A	6:23	6:23	Replace "very likely to be " with "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-164)]	Rejected – science is correct
2-208	A	6:23		Although it is not necessary to provide detailed explanations in the chapter summary bullets, it is not clear here or in the text how RF values have been weighted by "level of scientific understanding". It should be made clear exactly how this was done. Was there an additional weighting other than the size of the uncertainty? – i.e., in Figure 2.24 (p. 158), the top panel suggests that this might be true, whereas the bottom panel indicates that errors were applied equally to expand the total error after adding aerosols and other components with low level of scientific understanding. [Govt. of United States of America (Reviewer's comment ID #: 2023-62)]	No longer weighted
2-209	A	6:27	6:28	Over what period did this change in solar output occur? [Nathan Gillett (Reviewer's comment ID #: 84-21)]	Acccepted. Will be revised. Change is w.r.t. 1750.
2-210	A	6:27	6:50	This result is very significant. Given the scrutiny it will be given, you will need to ensure that you are confident enough to make the statements despite the high uncertainty and low understanding. It is not clear to me that chapter 6 supports this result explicitly. [Michael Manton (Reviewer's comment ID #: 157-10)]	Noted Reworded slightly to account for low level of undersanding. Chapter 6?
2-211	A	6:27	:50	Mention is made of the 11-year solar cycle, but it is well known that every other cycle is different. This, in effect, results in a 22-year cycle. One has to look at changes from one	Rejected. The 22-year cycle is on the polarity of the magnetic fields wheras

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				22-year period to the next to ascertain changes in the solar irradiance. The double cyle is due to the reversal of the solar magnetic field. This has technical merit. Consider it if it makes any difference. [Govt. of United States of America (Reviewer's comment ID #: 2023-63)]	the brightness and darkness of the irradince sources are independent of magnetic polarity of the active regions. Furthermore, the total irradiance is the balance of two competing effects (sunspot darkening and faculae brightening), both of which nincrease with solar activity. During rthe past three solar cycles it appears that the net modulation of total solar irradiance was approximately thre same suggesting that it has not exhibited a 22-year cycle.
2-212	A	6:28	6:28	Replace "two" with "four" Surely "with a low level of scientific understanding".means that the confodence limits are too narrow. So what is the point of them? Delete it [VINCENT GRAY (Reviewer's comment ID #: 88-165)]	5-95% confidence used
2-213	A	6:36	6:36	"(peak to peak)", this maybe pedantic, but shouldn't this be (trough to peak) or (min to max) or something similar? [Gareth S. Jones (Reviewer's comment ID #: 121-7)]	Accpeted – change text to "min to max"
2-214	A	6:37	6:37	Change to 'UV radiation with wavelengths below 310nm contributes 15% of the variability in total solar irradiance over its 11-year cycle.' [Nathan Gillett (Reviewer's comment ID #: 84-22)]	Accepted, text reworded
2-215	A	6:37	6:38	This sentence is slang and can be easily misunderstood. Try "Changes in UV radiation below 310 nm contribute 15% of the total solar irradiance changes during the 11-year cycle". I guess this is what is meant. [Thomas Peter (Reviewer's comment ID #: 198-8)]	Accepted, text reworded
2-216	A	6:41	6:43	The text states that variations in global column ozone due to changes in solar UV are comparable to CFC-induced ozone depletion in size. I think this is too strong a statement, and is not well-supported by the material in the chapter - e.g. fig 2.10. [Nathan Gillett (Reviewer's comment ID #: 84-23)]	Accpeted. Text reworded.
2-217	A	6:41	6:41	add after QBO-driven (Quasi-Biennial Oscillation); maybe some words to the QBO should be added to the glossary [Claudia Marcolli (Reviewer's comment ID #: 158-3)]	Accpeted. Text modified.
2-218	A	6:48	6:48	add after ENSO (El Niño-Southern Oscillation) [Claudia Marcolli (Reviewer's comment ID #: 158-4)]	Accpeted. Text modified.
2-219	A	6:52		Define "satellite observations era"	Accepted in part. This is defined

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				[Richard Soulen (Reviewer's comment ID #: 248-33)]	inside text and possibly earlier in ES.
2-220	A	6:53	6:53	Not all volcanic eruptions are climate-relevant. [European Commission (Reviewer's comment ID #: 2008-11)]	Accepted. 'Explosive' is meant to imply climate-relevant.
2-221	A	6:53	6:53	Please insert "climate-relevant" before "explosive volcanic eruption" as there have been different volcanic eruptions since Pinatubo, although not climate relevant. [Govt. of Germany (Reviewer's comment ID #: 2011-102)]	Accepted. See 2-220.
2-222	A	6:54	6:54	Footnote. This is nonsense. Guesswork cannot represent any statistical level [VINCENT GRAY (Reviewer's comment ID #: 88-166)]	Rejected. Uncertanity guide carefully explained to sufficient detail, both here and in the TS
2-223	A	7:2		The influence of volcanic aerosols on the radiative energy budget may be transitory, but that effect is translated into the ocean heat budget and it lasts for quite a long time there (decades to centuries). Thus, this statement seems a bit misleading in how it describes the impact of vulcanism on the Earth's energy balance. [Govt. of United States of America (Reviewer's comment ID #: 2023-64)]	Accepted. Will revise, objective here is to focus on forcing, not response. Response discussed in chap. 9.
2-224	A	7:6	7:10	This paragraph is potentially confusing since it is not explained here that the spatial structure in RF is not simply related to the spatial structure of the response. It may in fact be the case that we have more confidence in the spatial structure of the response than of the RFs. [Isaac Held (Reviewer's comment ID #: 105-13)]	Accepted, in part. Intent here is to cover forcing essentially, not make references to the response from these forcings. Will revise to improve clarity.
2-225	A	7:17	7:17	Replace "very likely" with "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-167)]	Rejected.
2-226	A	7:17	7:20	This paragraph to the uninitiated why a negative surface forcing can still be associated with a surface warming. [William Hare (Reviewer's comment ID #: 99-4)]	Accepted in part. No attempt will be made here to discuss response.
2-227	A	7:17		Insert "aerosol" between "mean" and "surface" [Govt. of United States of America (Reviewer's comment ID #: 2023-65)]	Accepted. Will improve clarity.
2-228	A	7:25	7:25	Northern-to-Southern (capital lett.) [Michel J. ROSSI (Reviewer's comment ID #: 220-14)]	Accepted.
2-229	A	7:26		Capitalize "Northern . . . ' 969 2-969 34 [Richard Soulen (Reviewer's comment ID #: 248-14)]	See 2-228.
2-230	A	8:0	9:	It migh be good to mention the following things: The concept of radiative forcing depends not only on the linearity relation ?Ts = ? RF but also on the additivity assumption: ?Ts = ? ?i RFi where the RFi are the various individual forcings. It might also help to give the current value for ? at this point, even though the discussion of this quantity is in another chapter. Of course the fact that ? is independent of i is also important.	Rejected. This is an introductory section, Section 2.8 discusses additivity. You do not need to know the climate sensitivity to quote RF, so it's value does not have to be stated here

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				[Wilmer Anderson (Reviewer's comment ID #: 5-67)]	
2-231	A	8:12	8:12	"The chapter assesses" instead of "The chapter assess". [Tiziano Colombo (Reviewer's comment ID #: 46-4)]	Accepted
2-232	A	8:12	8:12	Replace "assess" with "assess" [VINCENT GRAY (Reviewer's comment ID #: 88-168)]	Replaced with assesses
2-233	A	8:12	8:12	assess' should be 'assesses' [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-22)]	accepted
2-234	A	8:12	8:12	assess --> assesses [Thomas Peter (Reviewer's comment ID #: 198-10)]	accepted
2-235	A	8:13	8:13	Insert after "natural" insert "greenhouse gase changes (water vapour, methane)" [VINCENT GRAY (Reviewer's comment ID #: 88-169)]	Rejected - no reason given
2-236	A	8:18	8:18	, involve' should be 'or involve' [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-23)]	accepted
2-237	A	8:19	8:20	"Water vapour is the strongest greenhouse gas in the atmosphere and as most of its changes can be considered part of the climate rsponse, rather than a forcing, its main effect is as a climate feedback", What is "considered" by others does not constitute a reason why the radiation forcing of the most important greenhouse gas in the atmosphere should be ignored in a Chapter dealing with this problem. There ought to be a special section which lists its properties and importance. Chapter 8 merely tries to fit it into a preconceived "feedback" category for which there is no observational evidence [VINCENT GRAY (Reviewer's comment ID #: 88-170)]	"considrered" refers to the consesnus in the science community and is retained. To aviod overlap, we do not discuss water vapour properties and its importance in the climate feedback here. See also reply 2-1,2-2.
2-238	A	8:22	8:23	see comment #1 [Danny Harvey (Reviewer's comment ID #: 101-2)]	Rejected. Irrigation included here for completeness
2-239	A	8:22	8:22	Section 2.3.7 rather than section 2.3.8. Also, injection of water by aircraft was mentioned in section 2.3.7 (p.24, line 46). [Malcolm Ko (Reviewer's comment ID #: 135-2)]	Accepted. Text on aircraft added
2-240	A	8:26	8:26	"the chapter reassesses" instead of "the chapter reassess". [Tiziano Colombo (Reviewer's comment ID #: 46-5)]	Accepted
2-241	A	8:26	8:26	reassess' should be 'reassesses [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-24)]	Accpeted
2-242	A	8:31	8:32	occurs' should be 'occur' [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-25)]	accepted
2-243	A	8:31	8:34	A better flow of thoughts would be: "Compared to other RF agents, their trends are considerably better understood and quantified; because of this, the chapter does not devote as much page space to them as previous assessments (although the processes	Accepted

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				involved and the related budgets are further discussed in Chapter 7, Sections 7.3 and 7.4). Nevertheless LLGHGs remain ..." [Thomas Peter (Reviewer's comment ID #: 198-11)]	
2-244	A	8:33	8:33	"better understood" - I would say that the trend in methane is not better understood - and indeed is a bit of a mystery. [Keith Shine (Reviewer's comment ID #: 236-20)]	Accepted, "understood" dropped
2-245	A	8:52		Later on in the chapter the concept of "surface forcing" is used. Please explain here what the relationship is between radiative forcing and surface forcing. [European Commission (Reviewer's comment ID #: 2008-12)]	Noted. These two forcings aee an outcome of the forward calculation. For aerosols, a figure illustrates the two forcings. While the relationship is simple for scattering aerosols, for other agents such as LLGHGs and absorbing aerosols, it is not possible to generalize.
2-246	A	8:52		Section 2.2 "Concept of Radiative Forcing (RF)". Please amend this section with an explanation of the concept and the implications of "Surface Forcing". Surface forcing is used extensively later in the report, e.g. figures 2.26 and 2.27, and the non-experts need an introduction in order to avoid misinterpretations, e.g. answers to the question "How can it be that the surface forcing is negative while at the same time the surface heats up (see. Figure 2.26 b))?" [Govt. of Germany (Reviewer's comment ID #: 2011-103)]	Accepted in spirit, surface forcing will be introduced here, but main usage is in sections 2.4 and 2.9. Note that discussion on surface forcing in the chapter is much less than RF; this is consistent with where the literature is on the subject. While surface forcing needs to be recognized (which is what the chapter has done),literature shows that surface forcing and its potential significance / implications have developed much less on a quantitative basis than RF, including consequences for and linkages to the response of the climate system. Note also that this chapter is focussed on forcing aspects, not response.
2-247	A	8:52		SECTION 2.2 Concept of Radiative Forcing (RF). Other terms used in the report such as surface forcing need to be defined and explained here [William Hare (Reviewer's comment ID #: 99-5)]	Accepted in spirit, surface forcing will be introduced here, but main definition in section 2.8. See 2-246.
2-248	A	8:54	10:8	Contrary to the stated 'high scientific understanding', these lines reveal huge scientific uncertainties in the RF concept. According to Page 8 Line 56, RF is defined (a): at the	Rejected. Not sure what "high scientific understanding" refers to.

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				tropopause, (b): after allowing stratosphere to find its radiative equilibrium, and (c): with fixed surface and tropospheric temperatures. Here (b) is clearly not true, because recently stratosphere is in a cooling process, accompanied by a decreasing Outgoing Longwave Radiation (OLR). From (a). (tropopause) it is a long (theoretical) way to deduce changes in the ground temperatures, particularly because of the theoretically problematic 'temperature discontinuity at the ground', which comes from the inappropriate semi-infinite solution of the radiative transfer equation (see Goody-Yung). Lines 9-10 confess that RF provides a limited measure of climate change, its only advantage that it is 'calculable' and 'comparable'. Lines 5 to 8 on Page 9 also talks about difficulties. These problems altogether show the questionability of the statement on High Scientific Understanding. From this point all the estimations about future RF changes can be debated. [MIKLOS ZAGONI (Reviewer's comment ID #: 300-3)]	WE never use this phrase referring to the RF concept. Also, we don't quite get the physics of the argument. They seem to support our statements on RF caveats, so our text is retained. RF definition and its linkage to surface temperature changes are traceable to the earliest IPCC reports, and even before that to the literature of the early 1970s.
2-249	A	8:57	8:57	"state" … I think this is too vague these days - can it be elaborated on? [Keith Shine (Reviewer's comment ID #: 236-21)]	No, section 2.8 elaborates on this. It is more useful here to use the exact wording of the TAR
2-250	A	9:3	9:5	"RF can be related through a linear relationship to the global mean equilibrium temperature change at the surface ...". It would be good to know whether this is an exact or an approximate (linearized) relationship. The equal sign suggests an exact relationship, but this could be misleading. [Thomas Peter (Reviewer's comment ID #: 198-12)]	Rejected. This text is introductory. RF elaborated on in Section 2.8
2-251	A	9:10	9:10	Delete 'overall'. RF does not represent the climate response at all. [Nathan Gillett (Reviewer's comment ID #: 84-25)]	Rejected. It is somewhat representative of the global-mean surface temperature response
2-252	A	9:18	9:26	Whether a given radiative effect is a "forcing" or a "feedback" really depends on the specific climate model in question. If ozone or stratospheric water vapor changes are prescribed, then ozone and stratospsheric water vapor are forcings as far as that particular climate model is concerned. If ozone chemistry and stratospheric water transport happen to be integral parts of the climate model, then ozone and stratospheric water vapor changes (and their radiative effects) would be feedback effects. The same considerations apply to any other radiative quantities, or their precursors. It all depends on how they are treated by the model physics and parameterizations. [Andrew Lacis (Reviewer's comment ID #: 138-5)]	Noted. No suggestion given for alteration, their comment seems to support current text.
2-253	A	9:21	9:21	Sentence starting "Emissions of forcing precursors" is unclear. Aerosols can be directly emitted, or formed from emitted precursors. Ozone can't be directly emitted, but can be formed from precursors, or destroyed by ozone depleting substances. I suggest replacing "Emissions of forcing precursors, such as aerosol, ozone and LLGHGs, are" with	Accepted. Good idea

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				"Emissions of forcing agents, such as LLGHGs, aerosols and aerosol precursors, ozone precursors and ozone depleting substances, are" [William Collins (Reviewer's comment ID #: 45-3)]	
2-254	A	9:21	9:21	replace precursors by agents [Claudia Marcolli (Reviewer's comment ID #: 158-5)]	accepted
2-255	A	9:23	9:26	This is not expressed clearly; in particular, what does "climate response" mean here? [Govt. of Finland (Reviewer's comment ID #: 2009-11)]	The word "climate" has been dropped for clarity
2-256	A	9:28	9:44	the paragraph needs careful editing for punctuation and syntax [Danny Harvey (Reviewer's comment ID #: 101-3)]	Accepted
2-257	A	9:33	9:33	I suggest replacing 'biogeochemical feedback' with 'biogeochemical response to climate change'. I found the present wording ambiguous, since there has of course been a biogeochemical response to the changed concentrations of LLGHGs, e.g. associated with marine and terrestrial sinks of CO2. [Nathan Gillett (Reviewer's comment ID #: 84-26)]	Accepted. Good idea
2-258	A	9:33	9:33	"little or no" … ??? I am not sure of the meaning of the word "feedback" here, but surely there is a HUGE biogeochemical influence on CO2 concentrations [Keith Shine (Reviewer's comment ID #: 236-22)]	Accepted. Wording changed
2-259	A	9:39	9:39	important' should be 'important ones' [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-26)]	accepted
2-260	A	9:48	9:49	I found this unclear. Clarify. [Nathan Gillett (Reviewer's comment ID #: 84-27)]	State clarified
2-261	A	9:48		do a global search of the word "which", which is often used when "that" is the correct word (as in this case) (there is a good explanation of correct usage inside MS Word) [Danny Harvey (Reviewer's comment ID #: 101-4)]	accepted
2-262	A	9:56	9:57	a climate models response --> an apostrophe is missing [Thomas Peter (Reviewer's comment ID #: 198-13)]	accepted
2-263	A	10:1	10:1	practise' should be 'practice' [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-27)]	Accepted
2-264	A	10:1		In practise" to "In practice [Junying Sun (Reviewer's comment ID #: 261-3)]	accepted
2-265	A	10:2	10:2	to adequately quantify' should be 'to be adequately quantified' [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-28)]	accepted
2-266	A	10:7	10:10	I like the warning/advise on not using RF to assess emissions. I think it should be given more ink, elsewhere, if not here. However, there is a bit of a mixed message. Although RF should not be used for attribution, times and again the authors came close to doing it. I am particularly concerned about figure 2.25 which gives the RF in 2004 due to	Noted. Caveats will be added to section 2.9. However, figure will not be altered.

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				emissions and changes since 1750. A better way to explain this may be to stick to a version of Figure 2.24, and add to the figure to show what percentage each emission contributes to the change in concentration of the GHG. This could be done by adding a small pie chart to each bar. Another advantage in doing it this way is that one can then include the effects from OH trend on CH4 (p. 20, line 18), and on dynamics feedback on ozone (p. 21, line 20). [Malcolm Ko (Reviewer's comment ID #: 135-3)]	
2-267	A	10:9		this is the first of many examples in the early parts of this chapter of comma splices (joining two independent clauses with a comma, which is incorrect) [Danny Harvey (Reviewer's comment ID #: 101-5)]	Accepted. Changed to full-stop
2-268	A	10:13	10:13	Once again there should here be a paragraph on water vapour. What proportion of the greenhouse effect does it represent, are there any reliable measurements of its mean value, distribution spatially and over time and are these really related in some way only to "climate response? 307 2-307 171 [VINCENT GRAY (Reviewer's comment ID #: 88-5)]	Rejected please see comment 2-1
2-269	A	10:13	10:13	Section 2.3 needs a short introduction and roadmap. Table 2.1 deserves more discussion on its own in addition to just summarizing the numbers. It would be useful in the short introduction for section 2.3 to point out that gases in 2.3.1 to 2.3.4 are long-lived GHG where there are observations to derive what their concentrations were in 1750. This is in contrast to the other forcing agent, ozone, aerosol, water vapor discussed in sections 2.3.5 through 2.3.7. I also question whether section 2.3.5 may fit better in Chapter 7, and section 2.3.8 in section 2.2. The short introduction will help explain the linkage with the other subsections. [Malcolm Ko (Reviewer's comment ID #: 135-4)]	Noted. A good idea but there are strict space limitations in the report. Most of this is tackled in section 2.2
2-270	A	10:16	10:19	It would be useful here to add a sentence on the glacial-interglacial range of atmospheric carbon dioxide to put the subsequent discussion in perspective. Then refer to Chapter 6 for more details. [Bette Otto-Bliesner (Reviewer's comment ID #: 193-1)]	Taken into account. This is a good idea but strict space limitations prevent this. We refer the reader to Chpater 6 covering paleo climate
2-271	A	10:23	10:23	May need to tighten drafting of first sentence - 'Growth in CO2 concentrations since 1750 continues to slow………..' [Govt. of Australia (Reviewer's comment ID #: 2001-144)]	Accepted. First sentence changed
2-272	A	10:23	10:30	Point about accelerating rate of increase in CO2 concentrations over time needs to appear in Ch.2 Executive Summary/TS/SPM. [Govt. of Australia (Reviewer's comment ID #: 2001-145)]	Accepted but this point is already made in the Chapter 2 ES. Not our decision as to whether it also goes into TS and SPM
2-273	A	10:23	10:23	… largest sustained RF since 1750… actually Pinatubo produced a larger RF, but it lasted for only a few years.	Accepted. Excellent point ...change made

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				[Andrew Lacis (Reviewer's comment ID #: 138-6)]	
2-274	A	10:23		Delete "since 1750". RF is already defined as the change since 1750. [Adrian Simmons (Reviewer's comment ID #: 242-27)]	Accepted. Change made
2-275	A	10:25	10:25	275-285 needs a reference (suggest MacFarling Meure et al., GRL, 2006) [Govt. of Australia (Reviewer's comment ID #: 2001-146)]	Accepted Reference inserted
2-276	A	10:25	10:25	Specify pre-industrial: …pre-industrial era (1000-1750 AD) [Govt. of Australia (Reviewer's comment ID #: 2001-147)]	Accepted
2-277	A	10:28	10:28	Replace "10" by "24" [VINCENT GRAY (Reviewer's comment ID #: 88-172)]	Rejected. The range is clearly defined as 10 years and is now updated with new data as 1996 to 2005
2-278	A	10:28	10:28	Replace "1995" by "1980" [VINCENT GRAY (Reviewer's comment ID #: 88-173)]	Rejected. The range referred to is 1996 to 2005 as above
2-279	A	10:29	10:29	Insert after "increased "linearly" 310 2-310 174 [VINCENT GRAY (Reviewer's comment ID #: 88-173)]	Rejected. Analysis shows that the growth rate is increasing. It is not linear.
2-280	A	10:29	10:29	Replace "19ppm" with 1.5ppmv per year." 311 2-311 175 [VINCENT GRAY (Reviewer's comment ID #: 88-173)]	Rejected the growth for the period 1995 to 2004 was 19 ppm.
2-281	A	10:29	10:30	Delete from "the highest" to the end on line 30. There is no statistical evidence that the rate of increase has changed over this period,if the uncertainties are considered. It is unfair to choose a small sequence without uncertainties as "evidence" of an increase [VINCENT GRAY (Reviewer's comment ID #: 88-176)]	Rejected. The public domain data for Mauna Loa show that the growth rate was less than 1 ppm/yr at the start of the record in the 1950s The average over the last 10 years is at least a factor of two higher
2-282	A	10:30	10:30	Add at end. "It should be pointed out that the figures refer to the average concentration over the oceans. We have little information on the concentrations over land, where radiative forcing is therefore uncertain" [VINCENT GRAY (Reviewer's comment ID #: 88-177)]	Rejected. A large number of the NOAA/GMD sites are land based with several at least 1000kms from the nearest ocean. (please see site map at http://www.cmdl.noaa.gov/gallery/ccgg_figures/ccggmap for details) As pointed out in the text the Scripps sites are predominantly in the Pacific but the global annual means of both networks for 2004 and 2005 show agreement to better than 0.1ppm
2-283	A	10:46	10:46	Add at end. "It should be pointed out that the figures refer to the average concentration over the oceans".	Rejected. Comment as for 2-282

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				[VINCENT GRAY (Reviewer's comment ID #: 88-178)]	
2-284	A	10:52	10:52	Insert after "regions" "Unfortunately little progress has been made so far, so we have no reliable information on the greenhouse gas concentrations over land surfaces, or their possible contributions to radiative forcing." [VINCENT GRAY (Reviewer's comment ID #: 88-179)]	Rejected. Comment as for 2-282
2-285	A	10:53	10:53	Saying that flasks 'supplement' the continuous measurements does not give credit to their complementary role in providing a vital archive. [Michael Manton (Reviewer's comment ID #: 157-11)]	Taken into account. However we believe the rest of the section does show the value of both measurement types, especially the wide spread coverage that flasks provide.
2-286	A	10:56	11:5	I recognise that terrific job that CMDL (as they were) does in collecting and analysing CO2 data, and I am looking to downplay that work. However, in an assessment where we are seeking gl. [Michael Manton (Reviewer's comment ID #: 157-12)]	Rejected: Sorry but this comment appears to be incomplete
2-287	A	10:57		;" to ", [Junying Sun (Reviewer's comment ID #: 261-4)]	Rejected. Sorry location for the suggested "to" not found
2-288	A	11:1	11:3	There are several other flask and in situ networks. These should be mentioned and described in terms of what they find and how they compare. [Govt. of Australia (Reviewer's comment ID #: 2001-148)]	Accepted However strict space limitations prevent this and the two main networks, NOAA/GMD and Scripps data cited only. There are many other excellent networks eg CSIRO Australia and their data is readily available from the databases cited in the text.
2-289	A	11:2	11:5	"by the WMO Global Atmosphere Watch programme" should be replaced by "by the World Data Centre for Greenhouse Gases in the WMO Global Atmosphere Watch programme" from the contrast in this sentence. [Yukitomo TSUTSUMI (Reviewer's comment ID #: 270-4)]	Accepted. Suggested change made
2-290	A	11:7	11:7	It is not enough to refer to "emissions" in such a casual way, and you do not seem to understand that they are not the same as atmospheric concentrations.The Governmenta of the world are obsessed with "Emissions", yet you refrain from discussing them properly.There needs to be a separate section on "Emissions" It needs to discuss where they come from, how how they are measured , with what level of accuracy, and how they are related to concentrations.There should be a Table with historic figures of the various emission sources and a graph which plots them. You should do it for methane and other greenhouse gases as well as carbon dioxide. [VINCENT GRAY (Reviewer's comment ID #: 88-180)]	Accepted A good point but this is beyond the brief for this chapter. The link with fossil fuel CO2 emissions and the CO2 airborne fraction is discussed in chapter 7 which also discusses the budgets for methane

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2-291	A	11:7	11:7	Delete "The driving forces for" [VINCENT GRAY (Reviewer's comment ID #: 88-181)]	Accepted The "driving forces for" deleted
2-292	A	11:7	11:7	Delete "global" [VINCENT GRAY (Reviewer's comment ID #: 88-182)]	Rejected. We consider the global atmosphere
2-293	A	11:7	11:7	Insert after "are", "considered to be" [VINCENT GRAY (Reviewer's comment ID #: 88-183)]	Rejected. Carbon isotopic data and several other indicators show the role of fossil fuel CO2 emissions.
2-294	A	11:7	11:7	Insert after "mainly", "from" [VINCENT GRAY (Reviewer's comment ID #: 88-184)]	Accepted Change made
2-295	A	11:7	11:12	If you are going to mention climate feedbacks on ocean carbon uptake (which I think is reasonable) then you should also mention that climate affects the terrestrial uptake (probably by more than the ocean). [Chris Jones (Reviewer's comment ID #: 120-12)]	Accepted Change made
2-296	A	11:7	11:8	the explanation for this statement could be more clear. It is now burried in the text in 20-52. [Corinne Le Quere (Reviewer's comment ID #: 143-2)]	Accepted Statement clarified
2-297	A	11:10	11:12	Delete sentence from "Also" in line 10 to "2001)" in line twelve. This sentence makes no sense. It sis surely obvious that absorption of CO2 is not an "emission". It is also highly dubious that it should be considered a "feedback" 321 2-321 185 [VINCENT GRAY (Reviewer's comment ID #: 88-2)]	Rejected. This statment required by reviewers of the FOD but will be modified following comments 2-697 and 2-698
2-298	A	11:10	11:12	don't imply that ocean uptake is decreasing - merely that it is probably less than it would have been in the absence of warming (given the same CO2 levels) [Chris Jones (Reviewer's comment ID #: 120-11)]	Accepted Text changed
2-299	A	11:12		insert "carbon cycle" after "climate", as the feedback being discussed is not normally regarded as a purely climate feedback [Danny Harvey (Reviewer's comment ID #: 101-6)]	Accepted Text changed
2-300	A	11:14	11:14	Delete "After entering the atmosphere". CO2 exchanges ALL THE TIME, not only "after entering the atmosphere" [VINCENT GRAY (Reviewer's comment ID #: 88-186)]	Rejected. Sorry not clear what the problem is here. The text clearly indicates CO2 exchange between the active carbon resevoirs.
2-301	A	11:14	11:16	What is the difference between the "short-lived biosphere" and the "long-lived biosphere":. Surely it is a contnuum, not readily divided into categories. [VINCENT GRAY (Reviewer's comment ID #: 88-187)]	Agreed. It is a continuum. The literature assessed for this report uses these two categories
2-302	A	11:14	11:14	Insert after "short-lived", "components of the" [VINCENT GRAY (Reviewer's comment ID #: 88-188)]	Accepted Text changed as suggested
2-303	A	11:16	11:16	Insert after "long-lived" , "components of"	Noted. See comment 2-302

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				[VINCENT GRAY (Reviewer's comment ID #: 88-189)]	
2-304	A	11:16	11:16	Insert "the" before "deep ocean" [VINCENT GRAY (Reviewer's comment ID #: 88-190)]	Accepted Text changed
2-305	A	11:20	11:20	The RF is also a function of the concentration of CO2 at the start of the period. This is a key point relating to the logarithmic dependence of the forcing on the CO2 concentration, which is not made anywhere in the chapter. [Nathan Gillett (Reviewer's comment ID #: 84-28)]	Accepted – depedence noted here
2-306	A	11:20	11:37	This paragraph might be better placed as part of a separate sevtion on "emissions327 2-327 191 [VINCENT GRAY (Reviewer's comment ID #: 88-28)]	Noted. Please see comment 2-290
2-307	A	11:28	11:30	This statement is circular. Perhaps say 'decreases at a predicatable rate'. [Nathan Gillett (Reviewer's comment ID #: 84-29)]	Accepted text changed
2-308	A	11:28	11:28	Put the year in brackets? [Thomas Peter (Reviewer's comment ID #: 198-15)]	Accepted Modification made
2-309	A	11:29	11:30	One breif statement on why the former plant C is lighter might be helpful at this point. [Thomas Peter (Reviewer's comment ID #: 198-16)]	Accepted. Change made as suggested in comment 2-308
2-310	A	11:39	11:39	"Atmospheric oxygen". Give some actual figures. [VINCENT GRAY (Reviewer's comment ID #: 88-192)]	Noted. Values are provided in figure 2.3 and on line 44.
2-311	A	11:39	11:52	This paragraph should form part of a sepoerate section on "Emissions" [VINCENT GRAY (Reviewer's comment ID #: 88-193)]	Noted. Please see comment 2-290.
2-312	A	11:47		the year is 2006 in the reference list [Danny Harvey (Reviewer's comment ID #: 101-7)]	Accepted. Year changed to correct citation.
2-313	A	11:54	12:8	This paragraph should form part of a sepoerate section on "Emissions" [VINCENT GRAY (Reviewer's comment ID #: 88-194)]	Noted. Please see comment 2-290
2-314	A	11:54	12:8	The actual graph (Figure 2-3) shoiws how. his paragraph has selected figures from short-term fluctuations to give a false impression of excessive growth of emissions. [VINCENT GRAY (Reviewer's comment ID #: 88-195)]	Rejected. The data are the complete public domain data available from CDIAC and complemented using BP annual statistical reports of world energy usage through to 2004. The 2005 BP has just been released and will be included in the next draft of this report.
2-315	A	11:55	11:55	Insert after "increased", "irregularly, with a dip in 1992-3 and 1998", [VINCENT GRAY (Reviewer's comment ID #: 88-196)]	Accepted. Text modified
2-316	A	11:56	11:56	It is the emission rate, not the emission growth rate, which rose from 6.5 to 7.2 GtC/yr. [Olivier Boucher (Reviewer's comment ID #: 27-27)]	Accepted. Text clarified

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2-317	A	11:56		Please add a number of the percentage of increase per yr, see example line 55 same page, [Caroline Leck (Reviewer's comment ID #: 144-10)]	Accepted %age added
2-318	A	11:57	11:57	Insert after "representing a' , "short"333 2-333 197 [VINCENT GRAY (Reviewer's comment ID #: 88-10)]	Noted. Length of period is specified
2-319	A	11:57	11:57	Delete "much" [VINCENT GRAY (Reviewer's comment ID #: 88-198)]	Accepted
2-320	A	12:2	12:2	Replace "emission rates" by "emissions 335 2-335 199 [VINCENT GRAY (Reviewer's comment ID #: 88-198)]	Accepted
2-321	A	12:2		Please reduce the number of significant digits. [Caroline Leck (Reviewer's comment ID #: 144-11)]	Accepted
2-322	A	12:3	12:6	A paper by Oeschger, Siegenthaler and Heimann, (In Interactions of Energy and Climate,, ed Bach et al, 1980: Reidel, Dordrecht) refers to growth:fossil ratio as the 'apparent airborne fraction', since carbon cycle dynamics determine the growth:total_emissions ratio. Using the term airborne fraction (without 'apparent') for growth:fossil is an unfortunate legacy of Dave Keeling that leads to a confused description, here and in chapter 7. Suggest inserting word 'apparent', citing Oeshger et al, and footnote to note Keeling usage [ian Enting (Reviewer's comment ID #: 63-2)]	Accepted. Excellent point...wording changed as suggested but unfortunately no space for an extra reference. This topic is discussed in chapter 7
2-323	A	12:3	12:3	Replace "50%" with "60%" It is not often that I catch you out with a figure that is too low! [VINCENT GRAY (Reviewer's comment ID #: 88-200)]	Accepted.
2-324	A	12:6	12:8	In th elong run, the airborne fraction will depend on climate change - you could mention here: "Jones and Cox (2005) show that the airborne fraction in 2003 was anomalously large compared with the rest of the observational record.". Ref: Jones C. D. and Cox P. M., 2005, "On the significance of Atmospheric CO2 growth-rate anomalies in 2002-03", GRL, 32. [Chris Jones (Reviewer's comment ID #: 120-13)]	Noted. Comment referred to chapter 7 where airborne fraction is discussed in more depth.
2-325	A	12:8	12:8	what is this based on? [Corinne Le Quere (Reviewer's comment ID #: 143-1)]	Noted. The growth rate is derived from the data provided in the sentence
2-326	A	12:10	12:20	This is poorly integrated with previous discussion. Proposed resolution of previous paragraph, by using term 'apparent airborne fraction' should help. [ian Enting (Reviewer's comment ID #: 63-3)]	ACCEPTED. The text will be revised with the aim of improving intregration with previous paragraph.
2-327	A	12:10	12:20	This paragraph should be in a separate section on "Emissions" [VINCENT GRAY (Reviewer's comment ID #: 88-201)]	Noted please see comment 2-290
2-328	A	12:22	12:30	Let me start by repeating that CMDL and SIO (and others) do a great job. However, an outsider reading this paragraph would see it suggesting an inability of us to give a global	Noted. The agreement between both networks in 2005 was also excellent.

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				estimate of CO2 level. What an indictment! [Michael Manton (Reviewer's comment ID #: 157-13)]	Middle sentence of the paragraph deleted to remove confusion
2-329	A	12:24	12:24	The statistical derivations of uncertainties are different for each network'. Does this mean that the uncertainties are different? Or that the uncertainties were calculated in different ways? Clarify. [Nathan Gillett (Reviewer's comment ID #: 84-30)]	Noted and clarified
2-330	A	12:24	12:24	I think it better to show the statistical analysis result of CO2 such as WMO GHG Bulletin (http://www.wmo.ch/web/arep/gaw/ghg/ghg-bulletin-en-03-06.pdf) published 2006 March. [Takashi Maki (Reviewer's comment ID #: 153-1)]	Noted. Citation checked but no statistical analysis found
2-331	A	12:25	12:26	the agreementwas excellent but the accord is not always as good… sounds strange, rephrase? [Thomas Peter (Reviewer's comment ID #: 198-18)]	Accepted. Please see comment 2-328
2-332	A	12:27		NOAA/CMDL", to "NOAA/ESRL [Junying Sun (Reviewer's comment ID #: 261-5)]	Accepted. Change to NOAA/GMD
2-333	A	12:30	12:30	Add at end"These figures are for averages over oceans./ We have no comparabe figures for carbon dioxide concentration, or of other greenhouse gases, over land surfaces" [VINCENT GRAY (Reviewer's comment ID #: 88-202)]	Rejected. Please see comment 2-282
2-334	A	12:32	12:34	CO2 from ice cores is only available for 2005 years???? [Joyce Penner (Reviewer's comment ID #: 197-3)]	Noted. Opening para of the section refers to records going back 740,000 years and chapter 6. The data referred to here are to set the basis for the RF calcualations which is the brief of this chapter
2-335	A	12:34	12:34	Preferred reference is: MacFarling Meure et al., 2006 (H25) [Govt. of Australia (Reviewer's comment ID #: 2001-149)]	Accepted New reference inserted
2-336	A	12:34		The author's name is mis-spelled: it should be "MacFarling Meure". [Keith Lassey (Reviewer's comment ID #: 140-1)]	Accepted
2-337	A	12:35	12:35	..before ~1800 compared to after 1800, although CO2 concentrations dropped about 5-10 ppm between 1600 and 1800. Concentrations of CH4 and N2O were also slightly lower during this period. Note: Chapter 6 doesn't show these changes, suggest either an additional figure in Ch 6 or in this chapter is needed since the radiative forcing calculation depends on what is the pre-industrial baseline (as discussed in the next few lines). Question 2.1 Figure 1 (Ch 2 P 163) seems to address this and could be referred to. [Govt. of Australia (Reviewer's comment ID #: 2001-150)]	Accepted. Figure in chapter 6 shows both concentrtions and forcing – also in our FAQ. Figures now explicitly refered to
2-338	A	12:40	12:40	Delete "glacier" replace with "ice"	Accepted suggested change made

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				[Govt. of Australia (Reviewer's comment ID #: 2001-151)]	
2-339	A	12:40	12:40	law Dome is not a 'glacier': use term 'ice cap' [ian Enting (Reviewer's comment ID #: 63-4)]	Accepted
2-340	A	12:41	12:41	The age resolution of Law Dome ice core air is ~10 years, not ~5 years [Govt. of Australia (Reviewer's comment ID #: 2001-152)]	Accepted
2-341	A	12:43	12:43	The RF calculation needs explaining, both here and in the fig caption for Fig 2.4: What is the start period used (the issue is discussed in the previous paragraph but not answered-it appears to be 1750)? Are the ice core, firn air and flask data adjusted to provide global mean concentrations? Is the indirect forcing of CH4 included? [Govt. of Australia (Reviewer's comment ID #: 2001-153)]	Accepted
2-342	A	12:43	12:43	Add at end "These figures are for a strictly limited number of sites. They may not be representative of the whole earth"s surface as the greenhouse gases are not as "well-mixed" as is so frerquently stated" [VINCENT GRAY (Reviewer's comment ID #: 88-203)]	Rejected. The sentence shows the connection between CO2 data extracted from the ice cores and the Cape Grim In situ record. Both data sets are provided by the same CSIRO laboratory. No other sites are implied.
2-343	A	12:47	12:56	Make sure this section is consistent with and informs a similar discussion of radiative forcing in Chapter 10. [European Commission (Reviewer's comment ID #: 2008-13)]	Accepted
2-344	A	12:47	12:56	Please include here the full details on the AOGCM versus Line-by-line code inter-comparisons as it is key information belonging into the main radiative forcing chapter. Please coordinate with Chapter 10 to "take over" (large parts of) their sections 10.2.1.3 "Comparison of modeled forcings to estimates in Chapter 2" and 10.2.1.4 "Results from RTMIP: Implications for fidelity of forcing projections". In Chapter 10, these sections do not seem to fit in appropriately due to the high detail given, unfamiliar terminology for Chapter 10 readers, and overproportional focus on radiative forcing compared to emission uncertainty, sea level rise etc... [Govt. of Germany (Reviewer's comment ID #: 2011-104)]	Rejeted – Chapter 10 will keep this as refers to RF used in models of climate projections
2-345	A	12:47	12:47	Delete first sentence [Michael Manton (Reviewer's comment ID #: 157-14)]	Accepted. Sentence deleted
2-346	A	13:1	13:1	Double the confidence limits to two standard deviations:"1.63 plus or minus 0.32" [VINCENT GRAY (Reviewer's comment ID #: 88-204)]	5-95% given
2-347	A	13:2	13:2	Insert after "chapter" "but omits water vapour and clouds" [VINCENT GRAY (Reviewer's comment ID #: 88-205)]	5-95% given
2-348	A	13:2		change";" to "," [Junying Sun (Reviewer's comment ID #: 261-6)]	Accepted

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2-349	A	13:3	13:3	Insert after "2001)", "but remains within the 95% confidence limits" [VINCENT GRAY (Reviewer's comment ID #: 88-206)]	Rejected. Concentation change is not uncertain
2-350	A	13:3	13:3	Insert after "this" , "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-207)]	Rejected
2-351	A	13:3		1.46" to "1.46 W m-2 [Junying Sun (Reviewer's comment ID #: 261-7)]	Accepted
2-352	A	13:4	13:6	In the decade 1995 to 2004 the RF due to CO2 increased by about 0.28 Wm-2, an increase far greater than observed for any decade since the beginning of the industrial era.' Far greater' seems a bit strong, based on my reading of Fig 2.4. [Nathan Gillett (Reviewer's comment ID #: 84-31)]	Accepted ...changed to "greater"
2-353	A	13:4	13:4	Delete "much" [VINCENT GRAY (Reviewer's comment ID #: 88-208)]	Accepted
2-354	A	13:5	13:5	Replace "observed" by "calculated" [VINCENT GRAY (Reviewer's comment ID #: 88-209)]	Accepted
2-355	A	13:14	13:14	Insert "which provide the data required for the understanding of its atmospheric budget and for the calculation of its RF". [Govt. of Australia (Reviewer's comment ID #: 2001-154)]	Accepted ...change made
2-356	A	13:20	13:20	To follow the conclusions of Spahni et al.'s 2005 Nature paper more closely revise to say that the atmospheric methane did not exceed 773+/-15 ppbv. Although this high value is a single measurement at MIS 9.3 made on the Vostok ice core, there is not evidence to ignore it. [Bette Otto-Bliesner (Reviewer's comment ID #: 193-2)]	Accepted .change made
2-357	A	13:22	13:22	Insert "...CH4 levels increased from about 620 ppb to about 700 ppb in 1700" [Govt. of Australia (Reviewer's comment ID #: 2001-155)]	Accepted ...change made
2-358	A	13:22	13:23	Atmospheric CH4 concentrations in 1800,1900 and 1992 are given here. However, new observation data have come out, such as global average CH4 concentration is 1787ppb in 2003 (WDCGG No.29, p18). So, CH4 concentrations in 1800, 1900 and 2000 should be given here. [Govt. of China (Reviewer's comment ID #: 2006-28)]	Accepted ...reworded
2-359	A	13:23	13:23	Insert MacFarling et al., 2006 (GRL in press) as a second reference [Govt. of Australia (Reviewer's comment ID #: 2001-156)]	Accepted Reference inserted
2-360	A	13:23	13:23	Add at end "These measurements were, of course, for very few sites and they may not be representative of the whole earth's surface" [VINCENT GRAY (Reviewer's comment ID #: 88-210)]	Noted However the long lived ghg's changes over years or more can be found from one atmospheric baseline location with only minor adjustments for N-S gradient to get global mean

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					concentrations. The ice core records cited, and Law Dome in particular, have been thoroughly assessed for their ability to accurately reproduce the atmospheric concentrations of CO2, CH4 and N2O (refs Etheridge et al., 1996, 1998; MacFarling Meure et al., 2006) over hundreds if not thousands of years.
2-361	A	13:25	13:40	It is still not clear how well the new NOAA04 and the AGAGE scale agree; it is stated that the new NOAA04 scale now leads to "much closer agreement", but it would be useful to have a more quantitative statement. [Peter Bergamaschi (Reviewer's comment ID #: 19-1)]	text calrified
2-362	A	13:25	13:40	Despite some short explanations about the uncertainty of the global average CH4 abundance the exact calculation remains unclear. In particular striking is the very low uncertainty from the NOAA/CMDL network ("+- 0.60 ppb") compared to the AGAGE network ("+- 44.8 ppb"). [Peter Bergamaschi (Reviewer's comment ID #: 19-2)]	Text calrified
2-363	A	13:25	13:37	What is the global level of methane? An assessment (as opposed to a review) should not have to list the operating rules for indvidual laboratories. [Michael Manton (Reviewer's comment ID #: 157-15)]	Text calrified
2-364	A	13:26	13:26	Double the confidence limits to two standard deviations: 1777.6 ± 1.2" leave out the second decimal point 347 2-347 211 [VINCENT GRAY (Reviewer's comment ID #: 88-15)]	Rejected. These are the instrumental uncertainities
2-365	A	13:28	13:28	Insert before "This network "This figure has been effectivelyunchanged since 1999 and, if trends since 1984 are considered, is expected to fall." 348 2-348 212 [VINCENT GRAY (Reviewer's comment ID #: 88-15)]	Rejected. We have not seen any peer reviewed scientific literature which predicts this
2-366	A	13:28	13:31	I would still like this document to recognize the Tohoku University-Nippon Sanso contributions to CH4 calibration. They were the first to determine accurately the atmospheric abundance of this gas. The more recent NOAA calibration is virtually indistinguishable from the Tohoku calibration, and thus confirms the earlier Japanese work. The AGAGE program CH4 measurements are reported on the Tohoku calibration scale. [Ray Weiss (Reviewer's comment ID #: 284-3)]	Rejected. All reference to calibration techniques removed in line with comment 2-363. Statement inlcuded that AGAGE and NOAA/GMD are intercalibrated.
2-367	A	13:37	13:37	The other networks monitoring CH4 should be mentioned [Govt. of Australia (Reviewer's comment ID #: 2001-157)]	Accepted ..please see response to comment 2-288
2-368	A	13:37	13:37	What is the point a a global archive if we can't get a global estimate?	Noted. A global estimate is provided

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				[Michael Manton (Reviewer's comment ID #: 157-16)]	in this section
2-369	A	13:37	13:40	"by the WMO Global Atmosphere Watch programme" should be replaced by "by the World Data Centre for Greenhouse Gases in the WMO Global Atmosphere Watch programme" from the contrast in this sentence. [Yukitomo TSUTSUMI (Reviewer's comment ID #: 270-5)]	Accepted. Modification made as suggested
2-370	A	13:40	13:40	Add at end. As with carbon dioxide, the measurements take place only over oceans. We therefore have no reliable information on concnetratioins over land. This fact has been highlighted by the recent discovery that methane is emissted from forests (Keppler et al 2006) [VINCENT GRAY (Reviewer's comment ID #: 88-213)]	Rejected. See please see detailed response to comment 2-282
2-371	A	13:42	13:42	References needed. [European Commission (Reviewer's comment ID #: 2008-14)]	Noted inserted Spahni et al (2005 and Petit et al (1999)
2-372	A	13:42	13:42	Please cite the references for the conclusion that CH4 levels are unprecedented in at least the last 650'000 years. And please provide a comment why short-term peaks that cause higher than present-day methane levels can be excluded, although the lifetime of methane is around a decade and the resolution of the ice-cores much less than a decade. [Govt. of Germany (Reviewer's comment ID #: 2011-105)]	Noted inserted Spahni et al (2005 and Petit et al (1999). This is a good point. Although the CH4 lifetime is ~10 years, high concentrations peaks would be sustained for longer than 10 years (they would decay with a time constant of 10 years). These may still be reduced in amplitude or even missed in the record as a result of the air enclosure smoothing process and the measurement resolution in low snow-accumulation ice cores such as the 650000 yr Dome C record (Spahni et al., 2005; Spahni et al., GRL, 2003). Records over more recent millennia from higher accumulation/resolution ice cores in Greenland (eg. Blunier et al., 1995) and Antarctica (Byrd Station, Law Dome, eg. Blunier et al., 1998; MacFarling Meure et al., 2006) are much more likely to detect peaks of decadal duration but show no evidence of concentrations higher than present.

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2-373	A	13:42	13:42	Although this statement is likely correct there are other views which cannot be ruled out eg Nisbet (Nisbet, E. G. (2002). "Have sudden large releases of methane from geological reservoirs occurred since the Last Glacial Maximum, and could such releases occur again?" Philosophical Transactions of the Royal Society of London Series a-Mathematical Physical and Engineering Sciences 360(1793): 581-607) due to the short lifetime of CH4 compared to the resolution available from ice cores and other records. Does it need to be qualified? [William Hare (Reviewer's comment ID #: 99-6)]	Noted please see comment for 2-372. Also note that clathrate bursts are not supported by the recent dD CH4 work of Sowers (Science, 2006)
2-374	A	13:57	13:57	Replace "are clearly" with "may be" [VINCENT GRAY (Reviewer's comment ID #: 88-214)]	Rejected. The global abundance of any gas in the atmosphere is a function of global sources - sinks
2-375	A	14:1	14:1	Delete 'magnitude of an'. If the increase has changed to a decrease the sign as well as the magnitude of the balance between sources and sinks must have changed. [Nathan Gillett (Reviewer's comment ID #: 84-32)]	Accepted. Change made
2-376	A	14:3	14:3	before "soil sinks" add ", destruction in the stratosphere" [William Collins (Reviewer's comment ID #: 45-4)]	Accepted. Change made
2-377	A	14:7	14:7	The total source is well known (+/- 10%)- the strength of each source component and their trends are not [Govt. of Australia (Reviewer's comment ID #: 2001-158)]	Accepted ..excellent point ...changes made
2-378	A	14:7	14:7	Delete "The" [VINCENT GRAY (Reviewer's comment ID #: 88-215)]	Accepted...changes made as per comment 2-377
2-379	A	14:7	14:7	Replace "source is" by "sources are" [VINCENT GRAY (Reviewer's comment ID #: 88-216)]	Accepted...changes made as per comment 2-377
2-380	A	14:7	14:7	Replace "it is" by "they are" [VINCENT GRAY (Reviewer's comment ID #: 88-217)]	Accepted...changes made as per comment 2-377
2-381	A	14:7	14:7	Insert after "animals" "with the recent addition of forests" [VINCENT GRAY (Reviewer's comment ID #: 88-218)]	Rejected. Forests are a biogenic source of methane and are mentioned later in the section
2-382	A	14:7	14:8	It is stated that "As detailed in Chapter 7, Section 7.4 (the global CH4 source) is mostly biogenic and includes … ". This is exactly false. Chapter 7, page 7-3, line 54: "Atmospheric CH4 is dominated by anthropogenic sources." [Dylan Millet (Reviewer's comment ID #: 178-1)]	Noted...wrding changed but.many of the anthropogenic sources are biogenic
2-383	A	14:8	14:8	Biomass burning is usually referred to as a combustion source, not a biogenic source [Govt. of Australia (Reviewer's comment ID #: 2001-159)]	Accepted ...wording changed
2-384	A	14:8	14:9	The main "industrial sources" of CH4 are from fossil fuel mining and distribution, and it	Accepted

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				is worth saying so. (In fact few other "industrial sources" are believed to be significant at all). [Keith Lassey (Reviewer's comment ID #: 140-2)]	
2-385	A	14:9	14:10	"No significant improvements [in bottom-up source estimates] have been published since [the TAR]". Really? How about EDGAR source inventories to mention but one! Several new source estimates are documented in Chapter 7 (Table 7.4.1) which is where the CH4 budget is being discussed, so Chapter 2 should defer to Chapter 7 in this regard and stick with top-down estimates. [Keith Lassey (Reviewer's comment ID #: 140-3)]	Only partially accepted. Reference to EDGAR and Bergamaschi et al 2005 added. Note that the latter shows that national inventories can grossly under estimate emissions. This highlights the problems of the bottom up techniques and empasizes the importance of verifying these with measurements. Text added to clarify this
2-386	A	14:9	:15	There is new work since the TAR on bottom-up methane emission inventories and projections. The authors should consider National Communications submitted to the Climate Secretariat (these are official inventories), available at www.unfccc.net. Also, a new EPA draft report is available at: http://www.epa.gov/nonco2/econ-inv/international.html, which compiles many of these National Communications, containing current and projected (out to 2020) methane emissions done with bottom up inventory methods. [Govt. of United States of America (Reviewer's comment ID #: 2023-66)]	Partially accepted ...please see resposne to comment 2-385
2-387	A	14:10	14:13	It should be mentioned that extrapolating limited measurements to a global source strength is highly uncertain. Also, it's not clear that the new findings would significantly change the overall global CH4 source. Top-down analyses, using atmospheric measurements of total methane concentration, constrain the total amount of emissions from different regions. So if this new source of CH4 from plants is confirmed, then co-located emissions, e.g. from wetlands, must be lower than previously estimated; the total emissions from a given region can't change much. [James S. Wang (Reviewer's comment ID #: 281-7)]	Accepted ...wording changed
2-388	A	14:13	14:13	Insert "..the global CH4 source components (see ...." [Govt. of Australia (Reviewer's comment ID #: 2001-160)]	Accepted
2-389	A	14:14	14:14	The global total plant source has been substantially revised down (to 10-60 Tg/yr) by Kirschbaum et al., 2006 (Kirschbaum, M., Bruhn, D., Etheridge, D., Evans, J., Farquhar, G., Gifford, R., Paul, K., Winters, A., Comment on the Quantitative Significance of Aerobic Methane Release by Plants. Functional Plant Biology, 2006, 33, 521–530) [Govt. of Australia (Reviewer's comment ID #: 2001-161)]	Accepted ...reference inserted
2-390	A	14:14	14:15	The Keppler et al results may be consistent with the space-bourne estimates, but it is	Accepted ...wording changed + new

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				premature to say that they 'substantiate' them. Actually, the upscaling if their lab measurements to global budget number is probably premature as well. It might be best to simply indicate that these new lab results exist, and may be significant, but remain to be confirmed and properly quantified at the global level. [Gavin Schmidt (Reviewer's comment ID #: 227-1)]	reference inserted...please see comment 2-389.
2-391	A	14:14	14:14	"substantiates" - I perceive great scepticism about the Keppler et al result, and think the word "substantiates" is very loaded here. Maybe "lends some support" would be better - I am sure others will give more informed criticism of this. [Keith Shine (Reviewer's comment ID #: 236-23)]	Accepted ...wording changed + new reference inserted...please see comment 2-389.
2-392	A	14:14		has yet" to "has not yet [Junying Sun (Reviewer's comment ID #: 261-8)]	Accepted ...wording changed + new reference inserted...please see comment 2-389.
2-393	A	14:21	14:21	world" should be "world's [Patrick Hamill (Reviewer's comment ID #: 97-6)]	Accepted
2-394	A	14:23	14:26	"Others have argued that predicting future atmospheric burdens is difficult…": (1) sounds very vague (2) seems out of context as the discussion changes here from the analyis of trends and interannual variations of the last years to future scenarios (and then continues again with interannual variations) [Peter Bergamaschi (Reviewer's comment ID #: 19-3)]	Accepted ...text clarified
2-395	A	14:25	14:25	Reolace "variations" by "fall" [VINCENT GRAY (Reviewer's comment ID #: 88-219)]	Accepted ...add variations + fall in growth rate
2-396	A	14:35	14:38	13C/12C anomaly: I would not consider this as a very robust finding: Measurements from 3 other groups in the Southern Hemisphere do not confirm this anomaly: Measurements by CSIRO and university of Washington at Cape Grim [Francey et al., 1999] and measurement by university of Heidelberg at Neumeyer station [Marik, 1998] could not confirm this anomaly. Unfortunately, it seems that all (direct atmospheric) measurement before 1992 had somewhat poorer precision / accuracy. It should be mentioned that there is some uncertainty of these measurements and/or their represenatitveness for the SH. References: Francey et al., A history of d13C in atmospheric CH4 from the Cape Grim Air Archive and Antarctic firn air, J. Geophys. Res., 104, 23631-23643, 1999. Marik, T., Atmospheric d13C and dD measurement to balance the global methane budget, PhD thesis, university Heidelberg, 1998. [Peter Bergamaschi (Reviewer's comment ID #: 19-4)]	Accepted ...reduced and changed text to reflect this
2-397	A	14:38	14:40	"However, as pointed out by Dlugokencky et al. (2001),…often difficult to deconvolve…": Certainly it is difficult, but the sentence sounds very vague; furthermore it does not fit very well here, since the discussion on interannual variations continues in the next paragraph.	Accepted ...text changed

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				[Peter Bergamaschi (Reviewer's comment ID #: 19-5)]	
2-398	A	14:40	14:40	"the real cause or causes of the 1992 CH4 anomaly and linkages to the 1991 Pinatubo eruption are still undetermined", this sentences denies all possible explanations given in the preceeding par. I do not think that this is intended, but if so it needs to be said more clearly. Please clarify. [Thomas Peter (Reviewer's comment ID #: 198-19)]	Accepted ..text clarified
2-399	A	14:47	14:47	Butler et al. (JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 110, D21310, 2005) suggest that global growth rate maxima during biomass burning events is largely due to the increase in CH4 lifetime due to associated CO emissions [Govt. of Australia (Reviewer's comment ID #: 2001-162)]	Accepted
2-400	A	14:55	14:55	Replace "slow down" by "fall" [VINCENT GRAY (Reviewer's comment ID #: 88-220)]	Accepted ...used "reduction"
2-401	A	14:55	14:55	The model results of Wang et al. indicate that the slow down in the growth rate of CH4 may be temporary …" Recommend modifying to: "The model results of Wang et al. indicate that the slow down in the growth rate of CH4 is due to [state reasons] and may be temporary … [Dylan Millet (Reviewer's comment ID #: 178-2)]	Accepted
2-402	A	15:1	15:6	Lassey et al. (2005) examine…: If projections about future CH4 levels are to be discussed in this chapter this should not be based on a single paper. Such a discussion should include the IPCC scenarios and consider also further literature on this topic (e.g. Dentener et al., 2005). Furthermore, a very important question in this context is the behaviour of natural sources (wetlands, permafrost, CH4 hydrates) in response to climate change; I suggest to refer to chapter 7 here (section 7.4.1.2.) , which discusses the effects of climate. References: Dentener, F., D. Stevenson, J. Cofala, R. Mechler, M. Amann, P. Bergamaschi, F. Raes, and R. Derwent, The impact of air pollutant and methane emission controls on tropospheric ozone and radiative forcing: CTM calculations for the period 1990–2030, Atmospheric Chemistry and Physics, 5, 1731-1755, 2005. [Peter Bergamaschi (Reviewer's comment ID #: 19-6)]	Accepted ...this paragraph removed from this section.
2-403	A	15:3	15:3	comma at wrong position [Thomas Peter (Reviewer's comment ID #: 198-20)]	Noted ...this paragraph removed
2-404	A	15:9	15:9	Double the confidence limits to two standard deviations:to " 715±8ppb", Twice [VINCENT GRAY (Reviewer's comment ID #: 88-222)]	Noted Instrumental precision reported
2-405	A	15:11	15:11	Add at end . This figure is, however, for just one site which may not be representative" 357 2-357 221 [VINCENT GRAY (Reviewer's comment ID #: 88-222)]	Rejected. See please see detailed response to comment 2-282
2-406	A	15:14	15:14	Double the confidence limits to two standard deviations:to " 715 ± 8ppb", and" 1776 plus	Noted Instrumental precision reported

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				or minus 88ppb" [VINCENT GRAY (Reviewer's comment ID #: 88-223)]	
2-407	A	15:15		CMDL" to "GMD [Junying Sun (Reviewer's comment ID #: 261-9)]	Accepted
2-408	A	15:16	15:16	Double the confidence limits to two standard deviations:to " 0.48 ± 0.10 Watts per sq meter" 360 2-360 224 [VINCENT GRAY (Reviewer's comment ID #: 88-9)]	Noted Instrumental precision reported
2-409	A	15:26	15:26	eliminate period [Thomas Peter (Reviewer's comment ID #: 198-21)]	Accepted
2-410	A	15:27	15:30	These statements were left hanging, I did not really know what to do with them. [Thomas Peter (Reviewer's comment ID #: 198-22)]	Accepted. reworded
2-411	A	15:29	15:29	"Line-by-line" and "GCM" need to be defined. [Dylan Millet (Reviewer's comment ID #: 178-3)]	Some qualifers/defintions added
2-412	A	15:32	18:40	In this section and elsewhere the adjective "linear" and the phrase "increasing linearly" are used rather loosely in describing trace gas increases with time. I appreciate what is intended, but it would be wiser to say "approximately linear" or "increasing approximately linearly" so as not to imply that these trends are straight lines. [Ray Weiss (Reviewer's comment ID #: 284-5)]	Accepted. "Approximately" linearly used where relevant on pgs. 16-18.
2-413	A	15:34	15:34	Suggestion: "behind CO2, CH4, and CFC-12" [Govt. of Finland (Reviewer's comment ID #: 2009-12)]	Accepted. Gases reordered.
2-414	A	15:34		Note to myself. Please refer to A. L. Hirsch et al, GBC, 20, GB1008, doi:10.1029/2004GB002443,2006 (Inverse modeling of the global nitrous oxide surface flux from 1998-2001). [Ronald Prinn (Reviewer's comment ID #: 202-4)]	Accepted. Text and reference added.
2-415	A	15:36	15:36	Double the confidence limits to two standard deviations:to " 270 ± 14ppb" [VINCENT GRAY (Reviewer's comment ID #: 88-225)]	Reject. By convention all GHG precisions are 1 sigma but check that we say this at beginning of section 2.3.
2-416	A	15:47	15:47	High resolution ice core data are now available…. [Govt. of Australia (Reviewer's comment ID #: 2001-163)]	Reject. They are not high time or space resolution relative to surface air measurements.
2-417	A	15:47	15:47	prefered reference is: MacFarling Meure et al., 2006 (MacFarling Meure, C., Etheridge, D., Trudinger, C., Steele, P., Langenfelds, R., van Ommen, T., Smith, A. and Elkins, J. The Law Dome CO2, CH4 and N2O Ice Core Records Extended to 2000 years BP. Geophysical Research Letters, in press) [Govt. of Australia (Reviewer's comment ID #: 2001-164)]	Accepted. We quote as "MacFarling Meure et al, 2006", and adjust the reference list-also check if it has appeared in GRL.
2-418	A	15:47	15:47	N2O from ice cores are only available for the last 2000 years??	Accepted. "are now available"

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				[Joyce Penner (Reviewer's comment ID #: 197-4)]	replaced by "have been reported"
2-419	A	15:47		The author's name is mis-spelled: it should be "MacFarling Meure". [Keith Lassey (Reviewer's comment ID #: 140-4)]	See 2-417 reply.
2-420	A	15:48	2:48	…show relatively small changes (+/- 8 ppb) [Govt. of Australia (Reviewer's comment ID #: 2001-165)]	Accepted. "Relatively small changes" replaces "little change".
2-421	A	15:51	15:51	Double the confidence limits to two standard deviations:to " 270 ±14ppb" [VINCENT GRAY (Reviewer's comment ID #: 88-226)]	Rejected, see 2-415 reply.
2-422	A	15:52	15:52	Double the confidence limits to two standard deviations:to "319 ± 0.8ppb" and "0.06 ± 0.4ppbv" [VINCENT GRAY (Reviewer's comment ID #: 88-227)]	Rejected, see 2-415 reply.
2-423	A	15:56	16:23	While this par gives much detailed info, the broad picture is missing. E.g., is N2O mainly of terrestrial or oceanic origin? [Thomas Peter (Reviewer's comment ID #: 198-23)]	Will be covered in text added in reply to 2-414.
2-424	A	15:56		Understanding will necessarily have "evolved" since the TAR, but more to the point has it markedly improved? [Keith Lassey (Reviewer's comment ID #: 140-5)]	Accepted: "improved" replaces "evolved".
2-425	A	16:0	16:0	The discussion on relatively recent precipitation changes migh benefit from linking those observations to longer time records that are/should be discussed in section 6. A use ful reference would be: Treydte et al., 2006, Nature, p. 1179, doi=10.1038/nature04743 and references therein. [Rolf Müller (Reviewer's comment ID #: 181-37)]	Reject. Comment is misplaced.
2-426	A	16:4	16:4	Double the confidence limits to two standard deviations:to "0.9 ±0.4"and "0.8 ± 0.6" [VINCENT GRAY (Reviewer's comment ID #: 88-228)]	Accepted, but check whether already 2 sigma or whether we use 90% range.
2-427	A	16:43	16:45	The relative atmospheric abundances of the HFCs have changed since 1998. It would therefore be preferable to list these HFCs by their current decreasing order of mole fractions, as given in Table 2.1 and Figure 2.7, namely HFC-134a > HFC-23 > HFC-152a = HFC-125 [Govt. of Belgium (Reviewer's comment ID #: 2003-1)]	Accepted regarding ordering, but for lines 16.46 and 16.47 which refer to 2003 onwards. Lines 16.43 to 16.45 refer to 1998 deliberately since that was TAR date.
2-428	A	16:45	16:47	It would be preferable to give here the latest available atmospheric levels (see Table 2.1), rather than those observed in 2003 [Govt. of Belgium (Reviewer's comment ID #: 2003-2)]	Accepted-will use 2005 values and ordered as in 2-427 reply.
2-429	A	17:6	17:6	The lifetime of HFC-23 is given as 260 years here and 270 years in Table 2.14. Although the difference is hardly significant, the two values should be harmonized [Govt. of Belgium (Reviewer's comment ID #: 2003-3)]	Accepted, with 260 becoming 270 here.
2-430	A	17:20		insert "electrical" before "insulating", to clearly distinguish it from thermal insulation. [Danny Harvey (Reviewer's comment ID #: 101-8)]	Accepted. Also change line 17.21 "electrical" to "power" to avoid

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					repetition>
2-431	A	17:33	17:33	The following sentence provides the basis for the RF calculation beginning at zero concentrations:...........than to halogen loading. Reconstructions from firn air of the complete atmospheric histories of chlorofluorocarbons (CFCs -11, -12, -113, -114, -115), hydrochlorofluorocarbons (HCFCs -22, -141b, -142b), halons (H -1211, -1301), CH3CCl3 (methyl chloroform) and CCl4 (carbon tetrachloride) have shown essentially zero concentrations of these compounds before the early 1900s (Sturrock et al., 2002) (Sturrock, G. A., Etheridge, D. M., Trudinger, C. M., Fraser, P. J., and Smith, A. M. (2002). Atmospheric histories of halocarbons from analysis of Antarctic firn air: Major Montreal Protocol species. Journal of Geophysical Research - Atmospheres, 107 (D24): 4765, doi:10.1029/2002JD002548.) [Govt. of Australia (Reviewer's comment ID #: 2001-166)]	Noted. We do not have space to list all the evidence for preindustrial levels of halocarbons. This subject is well covered in the IPCC/TEAP report where this paper is referenced among others.
2-432	A	17:46	17:46	Why not present the lifetimes as 45 and 85 years? [Guus Velders (Reviewer's comment ID #: 276-10)]	Accepted. We now use the Table 2.14 values of 45 and 85 years.
2-433	A	18:2	18:19	The discussion of methyl chlorofom would be clearer if the authors could clarify whether the higher concentrations are thought to be due to some nations continuing to produce the material, emissions as a byproduct of other chemical industries, or release of previously manufactured materials. [Steven Baughcum (Reviewer's comment ID #: 16-2)]	Noted. Not enough space to address possible causes but they are discussed in the papers referenced here.
2-434	A	18:8	18:8	Why is the CH3CCl3 concentration given relative to the beginning of the AGAGE measurements? Why not relative to its maximum concentration in the beginning of the 1990s? [Guus Velders (Reviewer's comment ID #: 276-11)]	Accepted. Now given relative to its peak value.
2-435	A	18:12	18:19	Since the recent emissions of methyl chloroform from various regions are reviewed here, it would be appropriate to include significant releases from Japan, namely 1.4-3.3 Gg/yr in the 2001-2003 time-frame, as reported by Yokouchi et al, J. Geophys. Res. 110, D06301, doi:10.1029/2004JD005320, 2005 [Govt. of Belgium (Reviewer's comment ID #: 2003-4)]	Noted. We will need to subtract the industry estimates for Japan. If industry data are available to do that we will cite this paper.
2-436	A	18:12	18:14	I don't understand the relevance of this sentence. Explain more clearly. [Nathan Gillett (Reviewer's comment ID #: 84-33)]	Accepted. Change "evident from" to "supported by analyses of".
2-437	A	18:21	18:21	Not clear exactly which emissions are meant here. [European Commission (Reviewer's comment ID #: 2008-15)]	Noted. There is no mention of emissions on this line. Perhaps the comment refers to line 19.21 covered in reply to 2-448.
2-438	A	18:21	19:21	Specify which emissions exactly are here referred to, so that the text reads "Prinn et al. (2001) also estimated the XXXX emissions required to …." [Govt. of Germany (Reviewer's comment ID #: 2011-106)]	Accepted. This refers to line 19.21 and is addressed in reply to 2-448.

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2-439	A	18:30	18:31	It is stated that the emissions of HCFCs-141b and 142b started increasing quickly in the early 1990s, but it would be appropriate to point out that estimated releases of these two compounds peaked in 2002 and 2000 respectively (http://www.afeas.org/prodsales_download.html) and that, as a result of existing regulations on ozone-depleting substances, a sharp decline is expected within the coming decade (see, for instance, IPCC/TEAP 2005, Table 11.6) [Govt. of Belgium (Reviewer's comment ID #: 2003-5)]	Accepted in part. AFEAS estimates do not agree well with observations for these species. Referring to observationall-derived emissions in Fig. 2.4 in TEAP we add "then began to decrease after 2000" at the end of this sentence.
2-440	A	18:33	18:37	A truer comparison of the effects on global warming of the annual changes in each of the non-CO2 greenhouse gases (Figure 2.8) would be obtained by multiplying the annual change in concentration of each gas by its radiative efficiency (Table 2.14). The impact would be made even clearer to the reader if each of the resulting products (in W/m2) were expressed as a percentage of the overall current radiative forcing [Govt. of Belgium (Reviewer's comment ID #: 2003-6)]	Rejected. This proposed "truer"approach ignores the lifetimes of these species which are critical to evaluating the future effects of these changes in each of the GHGs. This was the point of including this graph.
2-441	A	18:34	18:34	I don't believe the CF4 line on this Figure 2.8 - it looks like digitisation noise or something [Keith Shine (Reviewer's comment ID #: 236-24)]	Accepted. We will add a caution into the Fig. 2.8 (now 2.7) caption.
2-442	A	18:42	20:21	This section goes into a lot of detail about how OH concentrations in the atmosphere are measured. However, almost no information is given on how OH affects LLGHGs, and thus on any possible implications for radiative forcing. Further, OH does not seem to be accounted for in GHG budgets elsewhere in the chapter. I suggest that considerably less detail is given on how OH is measured, and more detail is given on how it may relate to GHGs and radiative forcing. [Nathan Gillett (Reviewer's comment ID #: 84-34)]	Accepted in part. Note that the importance of OH is stated in the ES lines 4.23-4.24 and its major role in removing CH4 in section 2.3. We add "including CH4, and all HFCs, and HCFCs" after "trace gases" and "therefore" after "It" on line 18.45.
2-443	A	18:42	24:2	the sections 2.3.5 and 2.3.6 on OH and ozone are for my taste too detailed and too long. It would be helpful if the information was presented in a Figure or Table instead of text. [Claudia Marcolli (Reviewer's comment ID #: 158-6)]	Accepted in part. The importance for RF of both these species is very significant. Both sections will be shortened
2-444	A	18:51	18:53	While the accuracy of … do not match [that] for CH3CCl3 …". Replace "those" with "that". 635 2-635 6 [Keith Lassey (Reviewer's comment ID #: 140-6)]	Rejected. There are two things being matched here.
2-445	A	18:53	18:55	Insert the verb "is" before "capable". [Keith Lassey (Reviewer's comment ID #: 140-7)]	Accepted.
2-446	A	19:5	19:5	"weighted" by what? [Keith Shine (Reviewer's comment ID #: 236-25)]	Noted. The weighting is usually by the temperature-dependent rate constant multiplied by the CH3CCl3 concentration but other choices are also made and there is not room to

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					pusue this here. We already refer the reader to the Lawrence et al (2001) paper on line 19.11 to cover this.
2-447	A	19:18	19:18	"emissions"->"CH3CCl3 emissions". It is necessary to clarify what the emissions are of. [William Collins (Reviewer's comment ID #: 45-5)]	Accepted. "CH3CCl3" added.
2-448	A	19:21		I would insert "CH3CCl3" before "emission" for clarity [Danny Harvey (Reviewer's comment ID #: 101-9)]	Accepted. "CH3CCl3" added.
2-449	A	19:48	20:7	"... when Bousquet et al. estimated both OH and methyl chloroform emissions (constrained by their uncertainties as reported by McCulloch and Midgley (2000), the OH variations are reduced by 65%." Inversion results are very sensitive to the error estimates used. Section 2.3.4, paragraph 3 states that "emissions of CH3CCl3 determined from industry data (McCulloch and Midgley, 2001) may be too small in recent years." Does this imply that the reported errors are also too small? If so this would bias the inversion towards varying OH rather than the CH3CCl3 emisions. [Dylan Millet (Reviewer's comment ID #: 178-4)]	Noted. Inversions would be needed to verify that this effect is significant. In any case it would not change the conclusions on lines 20.4 and 20.5.
2-450	A	19:48	20:7	This paragraph is too complicated, and needs a synthesis or assessment statement. If the papers discussed in this paragraph represent a very uncertain state of knowledge, then consider to delete the paragraph, or at least write a more concise, shorter paragraph. [Govt. of United States of America (Reviewer's comment ID #: 2023-67)]	Patially accepted. Paragraph is important and clear and no other reviewer has objected. However, paragraph will be shortened
2-451	A	19:56	19:56	Delete the word "remarkably", which smacks of salesmanship. The words " agree well" already convey the intended meaning. [Ray Weiss (Reviewer's comment ID #: 284-6)]	Accepted: "remarkably" replaced by "very".
2-452	A	19:57	19:57	Presumably the word "concentrations" should be inserted after "OH" [Govt. of Belgium (Reviewer's comment ID #: 2003-7)]	Accepted.
2-453	A	20:26	20:26	Insert "about" aftter "of" [VINCENT GRAY (Reviewer's comment ID #: 88-229)]	Rejected, no explanation for suggested change given
2-454	A	20:28		causes;" to "causes, [Junying Sun (Reviewer's comment ID #: 261-10)]	Accepted
2-455	A	20:32	20:32	The statement that tropospheric O3 changes have influenced stratospheric O3 is not based on observations and should not be highlighted at this point. [Rolf Müller (Reviewer's comment ID #: 181-24)]	Taken into account, the sentence has been changed by including 'tropospheric ozone precursors' instead of 'tropospheric ozone'
2-456	A	20:37	20:37	Eliminate word "updated" to avoid repition on next line. [Thomas Peter (Reviewer's comment ID #: 198-24)]	Accepted
2-457	A	20:38	20:39	Bodeker et al (ACP, 2005) show that the trend from EP-TOMS disagrees with Dobsons by up to 18 DU, about 5%, at some locations. This sentence may be true on the global	Taken into account by deletion of 'the spatial and temporal characteristics of'

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				average, but it does not say "global average", indeed it refers to "spatial .. changes" [Howard K. Roscoe (Reviewer's comment ID #: 219-2)]	??
2-458	A	20:40	20:40	remove "reasonable" [Rolf Müller (Reviewer's comment ID #: 181-25)]	Accepted
2-459	A	20:43	20:48	What is stated here is not consistent with what is stated on this issue in Ch. 7., p. 57., l. 24-34. My suggestion is to stick with the findings of SROC 2005, perhaps updated with a few recent references. [Rolf Müller (Reviewer's comment ID #: 181-16)]	Rejected, but the Weatherhead and Andersen, 2006 reference is included which support the statement given in 20:43 to 20:48 ???
2-460	A	20:45	20:45	The Newchurch et al paper is on upper stratospheric ozone and is thus not appropriate for a discussion of total column ozone changes. [Rolf Müller (Reviewer's comment ID #: 181-26)]	Taken into account by including the Weatherhead and Andersen, 2006 reference
2-461	A	20:46	20:48	Perhaps the authors might want to include a reference to Weatherhead and Anderson, Nature, May 4, 2006, page 39 ff. [Patrick Hamill (Reviewer's comment ID #: 97-7)]	Accepted
2-462	A	20:48	20:49	Add: It is even very unlikely that the ozone-layer will recover to the pre 1980 state due to the changes that are expected to occur in atmospheric transport, temperature and importantant trace gases (Nature 441, 39-45 (4 May 2006) doi:10.1038/nature04746 The search for signs of recovery of the ozone layer Elizabeth C. Weatherhead and Signe Bech Andersen) [Hugo De Backer (Reviewer's comment ID #: 57-1)]	Rejected, this chapter deals with changes in atmospheric components and radiative forcing over the industrial era. Future changes where climate-chemistry feedbacks are involved are discussed in Chapter 7.
2-463	A	20:54	20:55	Say somewhere here that Arctic ozone losses are smaller because of warmer mean temperatures in the vortex inhibiting the formation of PSCs. At present the text explains why the variability is higher in the Arctic, but does not explain why the mean depletion is smaller. [Nathan Gillett (Reviewer's comment ID #: 84-35)]	Accepted
2-464	A	20:54	20:54	Local ozone losses in the Antarctic have been shown to be substantially larger than this - indeed, local depletion exceeding 90 or even 99% is frequently seen as shown for example in Solomon et al., JGR, 2005, 110, DOI10.1029. To avoid mentioning only my own work, let me also note the papers on this by Deshler et al. (2003 if I recall), and of course the discussions on it in WMO/UNEP ozone assessments. This can be quite important because the cooling is controlled by the ozone loss and so a careful choice of number is needed here. [Susan Solomon (NOAA) (Reviewer's comment ID #: 247-2)]	Taken into account by deleting second part of sentence
2-465	A	20:56	20:56	remove "dynamical" [Rolf Müller (Reviewer's comment ID #: 181-27)]	Accepted

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2-466	A	21:1	21:5	This is true, but the opposite mechanism, climate change leads to enhanced planetary wave activity and thus to *warmer* polar vortices should be refected here. Further it should be noted that the upcoming WMO ozone assessment will likely make the point (based on observations) that while variability in polar artic temperatures has increased, the cold winters have become colder. This statement (which is mainly based on two papers in review: Rex et al, GRL, Tilmes et al., GRL) should be reflected in this discussion as well. [Rolf Müller (Reviewer's comment ID #: 181-28)]	Rejected, climate feedback impact on chemistry belongs to chapter 7.
2-467	A	21:5	21:5	References for this statement would be: Tilmes, S., R. Müller, J.-U. Grooß, and J.M. Russell III, Ozone loss and chlorine activation in the Arctic winters 1991-2003 derived with the tracer-tracer correlations, Atmos. Chem. Phys., 5, 2181-2213, 2004. AND Rex, M., R.J. Salawitch, P. von der Gathen, N.R.P. Harris, M.P. Chipperfield, and B.Naujokat, Arctic ozone loss and climate change, Geophys. Res. Lett., 31, L04116, doi:10.1029/2003GL018844, 2004. [Rolf Müller (Reviewer's comment ID #: 181-29)]	Accepted
2-468	A	21:7	22:19	I am somewhat uncomfortable about this bit of the report, but maybe you have done the best you can. My problem comes from the split between "stratosphere" and "troposphere" which in the past has really been interpreted as "halocarbons" versus "tropospheric pollutants". That line has got muddier and muddier as we now know that NOx-mediated ozone increases go on well into the lower stratosphere. There is a danger that a reader will lose sight of this in the paragraph starting on line 30 on page 21, and not realise that the stratospheric ozone change is not solely related to halocarbons. Or else someone trying to understand the halocarbon component might miss the important bit on page 22, line 14. I think all the words are there, but some reordering or consolidation would help. I was a co-author on the Gauss paper and never really woke up to this general issue until it arose as part of the ozone assessment. [Keith Shine (Reviewer's comment ID #: 236-26)]	Taken into account, a better description of the difference in approach since TAR in the beginning of the ozone section is given. The converse also holds viz., tropospheric ozone changes in upper trop. getting mixed in with downward propagation of ozone anomalies. Then, there is the issue of feedbacks. Subject is getting complicated.
2-469	A	21:20	21:21	Over what period? [Nathan Gillett (Reviewer's comment ID #: 84-36)]	Accepted, time period included
2-470	A	21:20	21:21	I think this should read: "The only study to assess this finds that 50% of the RF related to stratospheric ozone changes between 20 N–60 N is attributable to dynamics…" [Thomas Peter (Reviewer's comment ID #: 198-25)]	Accepted
2-471	A	21:21	21:21	Replace 'feedbacks' with 'changes'. It's not clear whether that these changes in dynamics are a response to anthropogenic climate change, or are just internal variability. [Nathan Gillett (Reviewer's comment ID #: 84-37)]	Accepted
2-472	A	21:21	21:28	The word "thus" in the sentence 'The conclusion that stratospheric ozone forcings based on observations are thus more an upperbound of stratospheric ozone RF' seems not fully supported by the above sentences. What are the causes of 'stratospheric ozone changes	Accepted

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				themselves?. [Guus Velders (Reviewer's comment ID #: 276-12)]	
2-473	A	21:23	21:23	Remove the part starting :"but are not" ore at least replace "due to chemistry" by "due to chemical ozone loss". [Rolf Müller (Reviewer's comment ID #: 181-30)]	Accepted, the latter option has been included
2-474	A	21:23	21:25	Explain why this is the case. [Rolf Müller (Reviewer's comment ID #: 181-31)]	Rejected, space limits
2-475	A	21:26	21:28	You should state the reason given in the Hansen et al. (2005) that possibly explains the difference between their value and the Ramaswamy (2001) value. [John S. Daniel (Reviewer's comment ID #: 54-1)]	Accepted – short sedntence will be added.
2-476	A	21:26	21:26	repeat --> repeated [Thomas Peter (Reviewer's comment ID #: 198-26)]	Accepted
2-477	A	21:32	21:32	insert "annually averaged global mean" after 2000 [Rolf Müller (Reviewer's comment ID #: 181-32)]	Accepted
2-478	A	21:32	21:32	"on" should probably read "one", but even then the sentence is not really elegant. [Thomas Peter (Reviewer's comment ID #: 198-29)]	Rejected, cannot find "on" in line 32. Could it be ment line 26? Sentence on line 26 reworded
2-479	A	21:33	21:34	Typos, should be "changes" x2, "differs" [Howard K. Roscoe (Reviewer's comment ID #: 219-3)]	Accepted
2-480	A	21:34	21:34	period missing [Thomas Peter (Reviewer's comment ID #: 198-27)]	Accepted
2-481	A	21:34		"differ" to "differ." 982 2-982 11 [Junying Sun (Reviewer's comment ID #: 261-27)]	Accepted
2-482	A	21:43	21:43	This appears to be the first place in the body of the chapter where an uncertainty range is stated for an RF value. If the chapter does not adopt the +/- two standard deviations that is the scientific norm for uncertainty range, the reader needs to be reminded that the range is only +/- one standard deviation. [Lenny Bernstein (Reviewer's comment ID #: 20-49)]	Consistency in the chapter. Confidence will be explained on 1st use
2-483	A	21:43	21:43	Double the confidence limits to two standard deviations:to "-0.03 ± 0.14" [VINCENT GRAY (Reviewer's comment ID #: 88-230)]	5-95% confidence used
2-484	A	21:43	21:44	All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. However, if this change is not made, the text needs to clearly state that the uncertainty range for RF is +/- one standard deviation. This information must accompany each and every use of this limited uncertainty range. [Jeff Kueter (Reviewer's comment ID #: 137-46)]	5-95% confidence used. Obs now used for RF best estimate
2-485	A	21:45	21:46	I am very sceptical, that increases in lower strat. O3 are a result of increasing tropospheric	Accepted, the text is changed from

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				ozone. As long as there is no evidence from observations that this is the case (and I dont believe there is any) , I suggest to not emphasise this point. Transport in these models is likely too diffusive so that this observation might be a complete model artefact. This is not saying anything particularly bad about the models, it is just that transport across the tropical tropopause is a very difficult process to represent in a model. Further, convection is transporting ozone poor air from the boundary level rapidly to altitudes of about 14-15 km. Again this process is probably not well represented in state-of-the art models. [Rolf Müller (Reviewer's comment ID #: 181-33)]	'tropospheric ozone' to 'tropospheric ozone precursors'
2-486	A	21:47	21:47	replace "O3 destruction compounds" by "ozone loss due to the use of Montreal protocol gases" [Rolf Müller (Reviewer's comment ID #: 181-34)]	Taken into account, Montreal protocol gases included after ozone destruction
2-487	A	21:50	21:50	Typo, should be "water" [Howard K. Roscoe (Reviewer's comment ID #: 219-4)]	Accepted
2-488	A	21:52	21:53	This should reference Section 2.3.7, not 2.3. However, 2.3.7 does not dicsuss the pre-1980 trend. Section 3.4.2.4 from a different chapter does, but certainly does not conclude that "these increases… may not be justified". This biassed sub-sentence should be deleted. [Howard K. Roscoe (Reviewer's comment ID #: 219-5)]	Accepted," may not be justified' changed to 'are uncertain'
2-489	A	21:53	21:53	see Section 2.3" should read "see Section 2.3.7 [Thomas Peter (Reviewer's comment ID #: 198-28)]	Accepted
2-490	A	21:55	21:57	Section 2.3.6.1. Simply because there are no new RF calculations from observations, it does not follow that you should take the results of a set of CTMs to replace a previous estimate based on observations. I feel you are giving to much weight to these CTMs that demonstrate such large variations in tropospheric and stratospheric ozone changes. It is fine to discuss the results from the Gauss paper, but you do not have to adopt the model results as undisputed truth for the RF estimate. [John S. Daniel (Reviewer's comment ID #: 54-2)]	Taken into account, a new estimate is given and a better description of the difference in approach since TAR in the beginning of the ozone section is given
2-491	A	21:55	21:55	Double the confidence limits to two standard deviations:to "-0.03 ± 0.14" [VINCENT GRAY (Reviewer's comment ID #: 88-231)]	Taken into account, a 5-95% confidence level is given. Obs estimate now used for RF
2-492	A	21:57	22:2	Section 2.3.6.1. There needs to be much more explanation here or the forcing range needs to be changed. The Gauss paper does not provide forcing values that can be related solely to halocarbons; hence, it is not clear at all where the range of -0.03 to -0.15 comes from. In fact, with no new forcing estimate for the effect of halocarbons on ozone (since the Gauss work does not provide this), there should be no update to the Ramaswamy et al. (2001) value. [John S. Daniel (Reviewer's comment ID #: 54-3)]	Taken into account, a new estimate is given and a better description of the difference in approach since TAR in the beginning of the ozone section is given

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2-493	A	21:57	22:2	do the authors mean ozone forcing instead of ozone change (because they give it the units Wm-2)? [Claudia Marcolli (Reviewer's comment ID #: 158-7)]	Accepted. Seperate paragraph added to defend stance. Only upper limit on RF now given
2-494	A	22:1	22:2	I don't think you can say that the halogen chemistry contribution is likely to be more negative. What's the basis for this? If you are using the models, then you have to accept their errors, which suggest it could be either more positive or more negative. If you are basing the statement on a model/data comparison, then doesn't the lack of knowledge of the chemistry-induced contribution to changes near the tropopause imply that we just don't know how big it could be? [Susan Solomon (NOAA) (Reviewer's comment ID #: 247-1)]	Accepted, in this paragraph as well as in the beginning of the ozone section a better description of the difference in approach since TAR has been given. Text now reworded.
2-495	A	22:4	24:2	This section contains many typos and grammar errors. It would benefit from thorough proofreading. [Nathan Gillett (Reviewer's comment ID #: 84-38)]	Accepted
2-496	A	22:4		Section 2.3.6.2 attempts to discuss long-term trends in tropospheric O3 without mention of the Monsouris measurements in the 19th century and the recent recalibration of the technique. A whole pragraph should be devoted to this important measurement [Howard K. Roscoe (Reviewer's comment ID #: 219-6)]	The 19th century measurements were briefly mentioned (SOD 23:40) with a refernce to Pavelin et al (1999), and assessed as "very uncertain and semi-quantitative" based on suggestions by the reviewers to FOD. Given the page limitations, the propoesd addition is rejected (cf. also comment 2-516)
2-497	A	22:7	22:7	Double the confidence limits to two standard deviations:to "-0.35 ± 0.30" [VINCENT GRAY (Reviewer's comment ID #: 88-232)]	Changed to 5-95% confidence intevals.
2-498	A	22:7	22:8	"… the (ozone) RF estimate has to be based on model simulations." The role of satellite data should be mentioned here. [Dylan Millet (Reviewer's comment ID #: 178-5)]	Rejected. RF of tropospheric ozone can not be assessed by satelite measurements as a significant fraction of the change took place before the satelite period. Recent trends based on TOMS data are mentioned (SOD 23:5) and ozone precursors (SOD 22:27)
2-499	A	22:15	22:17	This sentence does not make sense to me. [Nathan Gillett (Reviewer's comment ID #: 84-39)]	Accepted. Cf. reply to comment 2-468, 2-500
2-500	A	22:15	22:17	the sentence structure has to be worked out [Claudia Marcolli (Reviewer's comment ID #: 158-8)]	Accepted. Cf. reply to comment 2-468 and 2-499
2-501	A	22:16	22:16	You state on the previous page that stratospheric ozone RF is based on models??? [Joyce Penner (Reviewer's comment ID #: 197-5)]	Yes. This is a change since since the TAR.

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2-502	A	22:17	22:19	Including the tropsospheric ozone change due to changes in transport across the tropopause arising from stratospheric ozone changes mixes direct and indirect effects, as stratospheric ozone changes indirectly in part due to circulation changes and in part due to temperature changes that change chemical sources and sinks. [Adrian Simmons (Reviewer's comment ID #: 242-28)]	Rejected. The results are based on CTMs driven by equal circiulation in both periods (pre-industrial and current), so it is only the direct effect that is included.
2-503	A	22:30	2:33	the sentence structure has to be worked out [Claudia Marcolli (Reviewer's comment ID #: 158-9)]	Accepted, cf. comment 2-504.
2-504	A	22:32	22:32	a varied on" should be "a varied one [Govt. of Finland (Reviewer's comment ID #: 2009-13)]	Accepted
2-505	A	22:35	22:35	"ozone in the free tropospheric ozone", delete second ozone. [Govt. of China (Reviewer's comment ID #: 2006-29)]	Accepted
2-506	A	22:35	22:35	an extra "ozone" in "ozone in the free troposheric ozone" [Govt. of Finland (Reviewer's comment ID #: 2009-14)]	Accepted
2-507	A	22:35	22:35	Over Europe, ozone in the free troposphere increased until the late 1980s…". Modify to: "… increased from [years] until the late 1980s… [Dylan Millet (Reviewer's comment ID #: 178-6)]	Rejected. Including a start year indicates that we have knowledge about ozone changes before that year, which we don't.
2-508	A	22:47	23:2	Should also cite Parrish, D.D., E.J. Dunlea, E.L. Atlas, S. Schauffler, S. Donnelly, V. Stroud, A.H. Goldstein, D.B. Millet, M. McKay, D.A. Jaffe, H.U. Price, P.G. Hess, F. Flocke, and J.M. Roberts (2004), Changes in the photochemical environment of the temperate North Pacific troposphere in response to increased Asian emissions, J. Geophys. Res., 109, D23S18, doi:10.1029/2004JD004978. [Dylan Millet (Reviewer's comment ID #: 178-7)]	Accepted
2-509	A	22:47	23:2	Trends are dependent on the time period examined. The specific time frames for these different analyses need to be stated. [Dylan Millet (Reviewer's comment ID #: 178-8)]	Accepted.
2-510	A	22:56	22:56	Small trends'- should this be 'no significant trends'? If not, say which sign the trends are. [Nathan Gillett (Reviewer's comment ID #: 84-40)]	Changed to insignificant.
2-511	A	23:4		Define 'black carbon'. How does it differ from 'carbon'? [Nathan Gillett (Reviewer's comment ID #: 84-54)]	Rejected. This sentence does not mention black carbon
2-512	A	23:7	23:7	Double the confidence limits to two standard deviations:to "-0.35 ± 0.30" [VINCENT GRAY (Reviewer's comment ID #: 88-233)]	Changed to 5-95% confidence intevals.
2-513	A	23:8	23:8	Double the confidence limits to two standard deviations:to "0.35± 0.30" [VINCENT GRAY (Reviewer's comment ID #: 88-234)]	Changed to 5-95% confidence intevals.
2-514	A	23:19		Refer the reader to Fig 2.11 here. [Nathan Gillett (Reviewer's comment ID #: 84-41)]	Rejected. A reference is given in line 27.

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2-515	A	23:24	23:24	Double the confidence limits to two standard deviations:to "0.032 ± 0.008" [VINCENT GRAY (Reviewer's comment ID #: 88-235)]	Accepted.
2-516	A	23:41	23:41	The incorrect "very uncertain semi-quantitative" must be removed (see previous comment). If you are referring to the chemical-soaked papers that changed colour, then include them in a follow on discussion to the fully quantitative measurements of the previous comment. Even the chemical-soaked papers have been partially recalibrated and do not deserve "very uncertain", see the journal paper resulting from Richard Wayne's lecture to the Royal Institution. [Howard K. Roscoe (Reviewer's comment ID #: 219-7)]	Rejected. Reviewer was contacted and no information provided on Wayne refererence. A literature search was fruitless.
2-517	A	23:48	23:49	"The uncertainties in the estiamated RF by tropospheric ozone originates from two factors: The models used …". Statement is not strictly correct. The source of error is not the model per se but inaccurate representation of specific processes within the model. [Dylan Millet (Reviewer's comment ID #: 178-9)]	Rejected. The sentence on line 49 in SOD 'CTM/GCM model formulation' covers the comment by the reviewer.
2-518	A	23:48	24:2	We have conducted measurements of the surface radiative forcing from tropospheric ozone under clouds (Evans et al, 1999); from these we have also calculated the radiative trapping (Evans et al, 2004). The results are in general agreement with model simulations for tropospheric radiative forcing. this is also of interest for comparison with section 2.3.6. [Wayne F.J. Evans (Reviewer's comment ID #: 65-6)]	Rejected. The section focuses on top of the atmosphere RF and not on surafce RF. In addition the RF discussed is due to ozone cahnge since 1750, which are not captured by these measurement.
2-519	A	23:48	24:2	W.F.J. Evans, C.R. Ferguson and E. Puckrin , "Radiative Forcing of Tropospheric Ozone." 5 pp, Proceedings for the 15th Symposium on Global Change and Climate Variations, 15th Symposium on Global Change and Climate Variations, 2004 AMS Annual Meeting, Washington State Convention and Trade Center, Seattle, WA , 11–15 January (2004a). .W.F. J. Evans and E. Puckrin, Comparison of Solar Variability Effects with Surface Radiative Forcing of CO2, Adv. Space Res., 33, pp. 1073-1076, (2004b). W.F.J. Evans and E. Puckrin, Remote Sensing of Greenhouse Gases and Radiative Fluxes, Proceedings of SPIE Vol : 5268, pp 42-47, Photonics East, Conference 5268, Providence, R.I., October 28-29, (2003). W.F.J. Evans and E. Puckrin, Remote Sensing Measurements of Tropospheric Ozone by Ground-Based Thermal Emission Spectroscopy, J. Atmos. Sci., 56, pp 311-318, (1999). [Wayne F.J. Evans (Reviewer's comment ID #: 65-7)]	See comment 2-518
2-520	A	24:7		the word "therefore" functions as a conjunctive adverb here and should be preceded by a semicolon and followed by a comma. The rule is: independent clauses of a compound sentence not joined by a coordinating conjunction (and, but, or, nor, for, yet, so) must be joined by a semicolon. Check all other uses of "therefore"	accepted

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				[Danny Harvey (Reviewer's comment ID #: 101-10)]	
2-521	A	24:18	24:19	Say somewhere that this upward trend is based on the balloon measurements only. [Nathan Gillett (Reviewer's comment ID #: 84-42)]	Discussed in Chapter 3 Rejected
2-522	A	24:18	24:18	eliminate "from" [Thomas Peter (Reviewer's comment ID #: 198-30)]	accepted
2-523	A	24:18	24:18	Half the sentence is missing - there is some limited evidence from what? [Howard K. Roscoe (Reviewer's comment ID #: 219-8)]	accepted
2-524	A	24:18	24:19	This biassed statement is certainly not a fair summary of Section 3.4.2.4. It is more than "some limited evidence", and the statement as written implies that a change in the last 6 years means that a change of the opposite sign in the previous 20 years or 40 years must be a measurement artefact, which is obviously an illogical tenet. [Howard K. Roscoe (Reviewer's comment ID #: 219-9)]	Accepted. Text reworded
2-525	A	24:27	24:28	This sentence implies chaos, whereas Fueglistaler et al and Fueglistaler & Haynes have now put the subject on a much more secure footing. [Howard K. Roscoe (Reviewer's comment ID #: 219-10)]	Partly accepted. Text reworded . We still do not know about qantification
2-526	A	24:38	24:38	factor" --> "a factor [Thomas Peter (Reviewer's comment ID #: 198-31)]	accepted
2-527	A	24:43	24:43	Double the confidence limits to two standard deviations:to "0.07 $\pm$ 0.02" [VINCENT GRAY (Reviewer's comment ID #: 88-236)]	Text reworded for 5-95% confidence
2-528	A	24:51	24:51	several typing errors [Govt. of Finland (Reviewer's comment ID #: 2009-15)]	accepted
2-529	A	24:51	24:51	Please check spelling. [Govt. of Germany (Reviewer's comment ID #: 2011-107)]	accepted
2-530	A	24:51	24:51	spelling (multiple) [Thomas Peter (Reviewer's comment ID #: 198-32)]	accepted
2-531	A	24:51		vapoue changeare unqiauntified" to "vapour change … [Junying Sun (Reviewer's comment ID #: 261-12)]	accepted
2-532	A	24:51		Sentence needs to be reformatted. [Govt. of United States of America (Reviewer's comment ID #: 2023-68)]	accepted
2-533	A	24:53	24:14	A reference should be included to section 3.4.4 here. This section contains a much more comprehensive discussion of observed radiative changes. [Nathan Gillett (Reviewer's comment ID #: 84-43)]	accepted
2-534	A	24:53	25:14	a) This review is about what has been left out and not about what has been included. With reference to section 2.3.8. The concept of radiative forcing has been well defined and well modeled. But this does not mean that such model results represent the real world. The effect of clouds on radiative forcing by greenhouse gases is extremely complex. The water	Rejected. Surface forcing is already alluded to briefly, but is not the main focus of the chapter. For information, please note that water vapor is held

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				vapor interference effect has not been properly accounted for in the calculations of radiative forcing and hence GWPs; for example, our work on this indicates that the GWP of methane is closer to 17 than 24. For example in the tropics, water vapor interference reduces the CO2 surface radiative forcing to only 3 W/m2 (Evans and Puckrin, 2004). Work on our measurements of surface radiative forcing is not reviewed or even mentioned in the report. [Wayne F.J. Evans (Reviewer's comment ID #: 65-1)]	fixed under the RF definition, which differs from the measurements situation. Unfortunately, this renders an ambiguity and possibly misinterpretation to a comparison of computed RF with measurements of the type described.
2-535	A	24:53	25:15	Review of radiative forcing in AR4 a) This review is about what has been left out and not about what has been included. With reference to section 2.3.8. The concept of radiative forcing has been well defined and well modeled. But this does not mean that such model results represent the real world. The effect of clouds on radiative forcing by greenhouse gases is extremely complex. The water vapor interference effect has not been properly accounted for in the calculations of radiative forcing and hence GWPs; for example, our work on this indicates that the GWP of methane is closer to 17 than 24. For example in the tropics, water vapor interference reduces the CO2 surface radiative forcing to only 3 W/m2 (Evans and Puckrin, 2004). Work on our measurements of surface radiative forcing is not reviewed or even mentioned in the report. We have made measurements of the surface radiative forcing from many of the GHG (Puckrin et al 2004b). In a comparison of IMG nadir measurements with upward viewing surface radiative forcing measurements (Evans and Puckrin, 2003) we have found a good agreement in the two types of measurements; this belies the report statement in 2.3.8 that " they do not conform to the RF definition". Since the measurements generally support the models of global warming, I do not understand the bias from modelers against making all the experimental measurements that can be made- we might even learn something new about global warming. Progress in the history of science has been largely derived from observations. We have conducted measurements of the surface radiative forcing from tropospheric ozone under clouds (Evans et al, 1999); from these we have also calculated the radiative trapping (Evans et al, 2004). The results are in general agreement with model simulations for tropospheric radiative forcing. this is also of interest for comparison with section 2.3.6. Although few and still in their infancy these measurements represent the real world rather than the model world. They represent a different type of knowledge than model simulations. I refer you to the paper by Puckrin et al (2004b) for a comparison of GCM codes with measured surface radiative forcing measurements which finds general agreement for CO2 and H2O but significant differences for CH4, N2O and CFCs. The	Rejected. Surface forcing is already alluded to briefly, but is not the main focus of the chapter. Unfortunately a lot of the suggested references are not peer reviewed so we cannot include them. We think our current section gives the right thrust and introduction to the disipline. It is not the in the scope to suggest research pathways. See also 2-534.

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				fine paper by Harries et al (2002) which does a satellite comparison of the changes from the IRIS instrument in 1970 with IMG measurements of radiative trapping in 1997 has been included briefly. The excellent recent paper by Philipona et al (2005) uses broad band IR surface measurements to show an increase in radiative forcing from greenhouse gases over a mountain site in Switzerland has been included. This type of experimental information demonstrating that mankind has caused an increase in the radiative balance of the planet is much more convincing than theoretical model simulations of it to many engineers and corporate executives. I believe that you should increase the short summary on the measurement of radiative forcing from greenhouse gases and the bright future for progress that will be made with new satellite and ground based systems. references Philipona, R. et al, Anthropogenic greenhouse forcing and strong water vapor feedback increase temperature in Europe, GEOPHYSICAL RESEARCH LETTERS, VOL. 32, L19809, doi:10.1029/2005GL023624(2005). W.F.J. Evans, C.R. Ferguson and E. Puckrin , "Radiative Forcing of Tropospheric Ozone." 5 pp, Proceedings for the 15th Symposium on Global Change and Climate Variations, 15th Symposium on Global Change and Climate Variations, 2004 AMS Annual Meeting, Washington State Convention and Trade Center, Seattle, WA , 11–15 January (2004a). .W.F. J. Evans and E. Puckrin, Comparison of Solar Variability Effects with Surface Radiative Forcing of CO2, Adv. Space Res., 33, pp. 1073-1076, (2004b). W.F.J. Evans and E. Puckrin, Remote Sensing of Greenhouse Gases and Radiative Fluxes, Proceedings of SPIE Vol : 5268, pp 42-47, Photonics East, Conference 5268, Providence, R.I., October 28-29, (2003). W.F.J. Evans and E. Puckrin, Remote Sensing Measurements of Tropospheric Ozone by Ground-Based Thermal Emission Spectroscopy, J. Atmos. Sci., 56, pp 311-318, (1999). E. Puckrin, W.F.J. Evans, J. Li, and H. Lavoie, Comparison of clear-sky surface radiative fluxes simulated with radiative transfer models, Canadian Journal of Remote Sensing, Pages 903-912, Volume 30, Number 6, December (2004b). JOHN E. HARRIES, HELEN E. BRINDLEY, PRETTY J. SAGOO & RICHARD J. BANTGES, Increases in greenhouse forcing inferred from the outgoing long wave radiation spectra of the Earth in 1970 an [Wayne F.J. Evans (Reviewer's comment ID #: 65-9)]	
2-536	A	24:55	25:3	We have made measurements of the surface radiative forcing from many of the GHG (Puckrin et al 2004b). In a comparison of IMG nadir measurements with upward viewing surface radiative forcing measurements (Evans and Puckrin, 2003) we have found a good agreement in the two types of measurements; this belies the report statement in 2.3.8 that	Rejected. Surface forcing is already alluded to briefly, but is not the main focus of the chapter. Unfortunately a lot of the suggested references are not

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				" they do not conform to the RF definition". Since the measurements generally support the models of global warming, I do not understand the bias from modelers against making all the experimental measurements that can be made- we might even learn something new about global warming. Progress in the history of science has been largely derived from observations. We have conducted measurements of the surface radiative forcing from tropospheric ozone under clouds (Evans et al, 1999); from these we have also calculated the radiative trapping (Evans et al, 2004). The results are in general agreement with model simulations for tropospheric radiative forcing. this is also of interest for comparison with section 2.3.6. [Wayne F.J. Evans (Reviewer's comment ID #: 65-2)]	peer reviewed so we cannot include them. We think our current section gives the right thust and introduction to the disipline. See also 2-534.
2-537	A	24:56	25:3	Although few and still in their infancy these measurements represent the real world rather than the model world. They represent a different type of knowledge than model simulations. I refer you to the paper by Puckrin et al (2004b) for a comparison of GCM codes with measured surface radiative forcing measurements which finds general agreement for CO2 and H2O but significant differences for CH4, N2O and CFCs. [Wayne F.J. Evans (Reviewer's comment ID #: 65-3)]	Rejected. Surface forcing is already alluded to briefly, but is not the main focus of the chapter. Unfortunately a lot of the suggested references are not peer reviewed so we cannot include them. We think our current section gives the right thust and introduction to the disipline. It is not our job to suggest research pathways. See also 2-534.
2-538	A	25:4	25:14	The fine paper by Harries et al (2002) which does a satellite comparison of the changes from the IRIS instrument in 1970 with IMG measurements of radiative trapping in 1997 has been included briefly. The excellent recent paper by Philipona et al (2005) uses broad band IR surface measurements to show an increase in radiative forcing from greenhouse gases over a mountain site in Switzerland has been included. This type of experimental information demonstrating that mankind has caused an increase in the radiative balance of the planet is much more convincing than theoretical model simulations of it to many engineers and corporate executives. I believe that you should increase the short summary on the measurement of radiative forcing from greenhouse gases and the bright future for progress that will be made with new satellite and ground based systems [Wayne F.J. Evans (Reviewer's comment ID #: 65-4)]	Rejected. Surface forcing is already alluded to briefly, but is not the main focus of the chapter. Philipona et al. 2005 is already included. Unfortunately a lot of the suggested references are not peer reviewed so we cannot include them. We think our current section gives the right thust and introduction to the disipline. It is not our job to suggest research pathways. See also 2-534.
2-539	A	25:4	25:14	references Philipona, R. et al, Anthropogenic greenhouse forcing and strong water vapor feedback increase temperature in Europe, GEOPHYSICAL RESEARCH LETTERS, VOL. 32, L19809, doi:10.1029/2005GL023624(2005).	Rejected. Surface forcing is already alluded to briefly, but is not the main focus of the chapter. Unfortunately a lot of the suggested references are not

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				W.F.J. Evans, C.R. Ferguson and E. Puckrin , "Radiative Forcing of Tropospheric Ozone." 5 pp, Proceedings for the 15th Symposium on Global Change and Climate Variations, 15th Symposium on Global Change and Climate Variations, 2004 AMS Annual Meeting, Washington State Convention and Trade Center, Seattle, WA , 11–15 January (2004a). .W.F. J. Evans and E. Puckrin, Comparison of Solar Variability Effects with Surface Radiative Forcing of CO2, Adv. Space Res., 33, pp. 1073-1076, (2004b). W.F.J. Evans and E. Puckrin, Remote Sensing of Greenhouse Gases and Radiative Fluxes, Proceedings of SPIE Vol : 5268, pp 42-47, Photonics East, Conference 5268, Providence, R.I., October 28-29, (2003). W.F.J. Evans and E. Puckrin, Remote Sensing Measurements of Tropospheric Ozone by Ground-Based Thermal Emission Spectroscopy, J. Atmos. Sci., 56, pp 311-318, (1999). E. Puckrin, W.F.J. Evans, J. Li, and H. Lavoie, Comparison of clear-sky surface radiative fluxes simulated with radiative transfer models, Canadian Journal of Remote Sensing, Pages 903-912, Volume 30, Number 6, December (2004b). references Philipona, R. et al, Anthropogenic greenhouse forcing and strong water vapor feedback increase temperature in Europe, GEOPHYSICAL RESEARCH LETTERS, VOL. 32, L19809, doi:10.1029/2005GL023624(2005). W.F.J. Evans, C.R. Ferguson and E. Puckrin , "Radiative Forcing of Tropospheric Ozone." 5 pp, Proceedings for the 15th Symposium on Global Change and Climate Variations, 15th Symposium on Global Change and Climate Variations, 2004 AMS Annual Meeting, Washington State Convention and Trade Center, Seattle, WA , 11–15 January (2004a). .W.F. J. Evans and E. Puckrin, Comparison of Solar Variability Effects with Surface Radiative Forcing of CO2, Adv. Space Res., 33, pp. 1073-1076, (2004b). W.F.J. Evans and E. Puckrin, Remote Sensing of Greenhouse Gases and Radiative Fluxes, Proceedings of SPIE Vol : 5268, pp 42-47, Photonics East, Conference 5268, Providence, R.I., October 28-29, (2003). W.F.J. Evans and E. Puckrin, Remote Sensing Measurements of Tropospheric Ozone by Ground-Based Thermal Emission Spectroscopy, J. Atmos. Sci., 56, pp 311-318, (1999). E. Puckrin, W.F.J. Evans, J. Li, and H. Lavoie, Comparison of clear-sky surface radiative fluxes simulated with radiative transfer models, Canadian Journal of Remote Sensing, Pages 903-912, Volume 30, Number 6, December (2004b). [Wayne F.J. Evans (Reviewer's comment ID #: 65-5)]	peer reviewed so we cannot include them. We think our current section gives the right thust and introduction to the disipline. It is not our job to suggest research pathways. See also 2-534.
2-540	A	25:6	25:6	Delete "an" [VINCENT GRAY (Reviewer's comment ID #: 88-237)]	accepted

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2-541	A	25:13	25:14	suggest ' having a direct linkage' [ian Enting (Reviewer's comment ID #: 63-5)]	accepted
2-542	A	25:16	25:16	This section fails to mention the most important aerosols, which are ordinary clouds. It is no excuse to say that they are "considered" to be a "feedback" to carbon dioxide concentrations since this is a mere defect of current models [VINCENT GRAY (Reviewer's comment ID #: 88-238)]	Rejected. This section considers the radiative forcing i.e. the radiative forcing caused by the anthropogenic perturbation. Cloud radiative forcing and cloud feedback are something entirely different. Both the direct and indirect effect (i.e. the effect of aerosols upon clouds) ARE considered.
2-543	A	25:16	46:2	I think the aerosol section would benefit from being made more easily understandable to non-specialists. [Nathan Gillett (Reviewer's comment ID #: 84-44)]	Noted. This is difficult given the complexity of the research. What we have tried to do is include some more basic cartoon type of material e.g. Figure 2.12 for the less specialist reader.
2-544	A	25:16	46:2	New version has been greatly improved in descriptions of uncertainties of RF due to aerosol from both satellite estimate and model biases, and also combined estimates.Also the scientific understanding of aerosol effect is clearly stated in the relevant figures. [Xueliang Guo (Reviewer's comment ID #: 93-3)]	Noted. Thanks.
2-545	A	25:16		Somewhere in this section 2.4 the extensive survey of 24 experts by G. Morgan et al (Elicitation of Expert Judgments of Aerosol Forcing, Climatic Change, 2006, doi:10.1007/s10584-005-9025-y) on all categories of aerosol radiative forcing should be referenced and discussed where the uncertainties are assessed. I do not think it can be ignored by the IPCC given the list of expert participants. [Ronald Prinn (Reviewer's comment ID #: 202-3)]	Rejected. The author of the chapter cannot comment under the rules.
2-546	A	25:18		Section 2.4.1. For an assessment (rather than a review), it is not clear that a summary of the TAR is required. [Michael Manton (Reviewer's comment ID #: 157-17)]	Rejected. Many reviewers have commented favourably on the inclusion of the state of the science at the TAR and the changes since then. We therefore think that it is appropriate to include the summary of the TAR.
2-547	A	25:20	25:20	Delete"anthropogenic" It is wrong to assume that humans are resposible for all changes in aerosols. Changes in natural aerosols have to be investigated [VINCENT GRAY (Reviewer's comment ID #: 88-239)]	Rejected. Only anthropogenic aerosols exert a radiative forcing.

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2-548	A	25:24	25:24	"Key" … I think the extinction optical depth is much more key than the specific extinction coefficient :-) [Keith Shine (Reviewer's comment ID #: 236-27)]	Noted. The optical depth is not an intrinsic optical parameter of an aerosol distribution, but the product of the specific extinction and the column mass amount (or loading). We state the key parameters are the 'aerosol optical properties ..... and the geographic distribution'. However, we now include the 'column loading' statement explicitly.
2-549	A	25:29	25:30	Give some examples of partially absorbing aerosols. [Nathan Gillett (Reviewer's comment ID #: 84-45)]	Rejected. Space constraints.
2-550	A	25:38	25:40	"The surface forcing will be approximately the same as the direct RF at the TOA for scattering aerosols, but for partially absorbing aerosols the surface forcing may be many times stronger than the TOA direct RF (e.g. Ramanathan et al., 2001b and references therein)." The referencing for this finding is incomplete. The following papers showed this precise conclusion on the regional and global scales, respectively, prior to the Ramanathan et al. (2001b) paper and were not cited in that paper: On the regional scale: Figure 7 and Section 7, paragraphs 3-5 (text) of Jacobson, M. Z., Studying the effects of aerosols on vertical photolysis rate coefficient and temperature profiles over an urban airshed, J. Geophys. Res., 103, 10,593-10,604, 1998 compare and discuss vertical profiles of forcing (irradiance change) due to aerosols at a location with strong absorption (Claremont) and weak absorption (Long Beach): On the global scale: Figure 4(a)-(o) and Section 6 (text) of Jacobson, M. Z., Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001 show and discuss vertical profiles of forcing for many individual scattering or absorbing aerosol components and all components. [Mark Jacobson (Reviewer's comment ID #: 116-2)]	Rejected. As the reviewer knows, the surface forcing and the top of the atmosphere forcing due to aerosols have been studied for some time. What we require here is a generic reference that indicates the importance from both measurement and modelling studies. Many references could have been chosen, but the Ramanathan et al. (2001) reference and references therein are a more well-known and authoritative study. Please note this is intended to be an assessment, not a survey.
2-551	A	25:40	25:49	We were extremely careful in the TAR to use wording that was specific about cloud affects, rather than picking up on "popular" names like "cloud albedo effect" and "cloud lifetime effect" which are ambiguous at best and wrong at worst. Cloud albedo can change through changes in precipitation efficiency if the liquid water path changes, so the term "cloud albedo effect" as used here is innaccurate. Changes in precipitation efficiency do	Accepted. Part of the reason has been the demand that the terms be easily understandable to the non-specialist. We have inserted some text that tries to include the approximate

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				not necessarily lead to changes in cloud lifetime so that term is also innacurate. Also, there are a number of morphological changes that can occur to clouds as a result of changes in precipitation efficiency, so the term "cloud lifetime effect" is not inclusive of these effects. [Joyce Penner (Reviewer's comment ID #: 197-6)]	equivalence of the terms of reference (e.g. first, cloud albedo, and Twomey effects). However, we do not wish this section to grow too much owing to space constraints.
2-552	A	25:45	25:45	Left parenthesis missing in front of "e.g., Penner" [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-29)]	Accepted.
2-553	A	25:45		Of importance is also the morphology or shape resolved over size. Please add. [Caroline Leck (Reviewer's comment ID #: 144-13)]	Rejected. We think that the morphology is really encompassed sufficiently by "size, chemical composition, mixing state and ambient environment."
2-554	A	25:45		Add morphology/shape cf. Comment 14 and 13. [Caroline Leck (Reviewer's comment ID #: 144-16)]	Rejected. As above.
2-555	A	25:45		Please omit Penner et al., 2001. Its not a very recent reference and many others would be much more appropriate to use. [Caroline Leck (Reviewer's comment ID #: 144-17)]	Rejected. The Penenr et al reference is the relevant section in the IPCC TAR. It is useful to back-refer to what has changed and what has not since the TAR.
2-556	A	26:9	26:10	Ackerman et al. (1999) should be listed as well as Penner et al. (2003) (Ackerman, A.S., O. B. Toon, D. E. Stevens, A. J. Heymsfield, V. Ramanathan, and E. J. Welton, Reduction of tropical cloudiness by soot, Science, 288, 1042-1047, 2000., Penner, J.E., S.Y. Zhang, and C.C. Chuang, 2003: Soot and smoke aerosol may not warm climate, J. Geophys. Res., 108, D21, Art. No. 4657, doi: 10.1029/2003JD003409.) [Joyce Penner (Reviewer's comment ID #: 197-7)]	Accpeted in part. The Ackerman reference is included.
2-557	A	26:9	26:9	Given that Hansen et al (1997) coined the term "semi-direct", the omission of this reference here seems unfortunate to me. [Keith Shine (Reviewer's comment ID #: 236-28)]	Accepted. Included.
2-558	A	26:20		Section 2.4.2. This section is more of a review than an assessment. [Michael Manton (Reviewer's comment ID #: 157-18)]	Noted. We think that this section nicely sums up the developments since TAR. It advances the ideas of deriving the direct radiative effect from satellite estimates (not just the direct radiative effect over oceans).
2-559	A	26:20		Developments." to "Developments [Junying Sun (Reviewer's comment ID #: 261-13)]	Accepted.

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2-560	A	26:22	26:22	Insert after "properties "of the different kinds of aerosols" 376 2-376 240 [VINCENT GRAY (Reviewer's comment ID #: 88-13)]	Accepted, but slightly reworded.
2-561	A	26:22	26:23	Recent advances from surface based measurements include but for chemical properties, knowledge of morphology/shape and state of mixture, of the individual airborne particles at a given size or within a given size range. This advance is of great importance in order to reduce the uncertainties in model estimates on radiative forcing and the new information is not just some integral or average property over a large number of particles as would be determined by bulk analysis only. Recent advances also hold an improved understanding of the properties of the biogenic/organic aerosol and its relative importance to the inorganic aerosol. [Caroline Leck (Reviewer's comment ID #: 144-14)]	Noted. It is difficult to include this additional information while keeping the section concise.
2-562	A	26:24	34:26	I fully agree with the stressed limitations to why there is difficult to make sensible validations of the model calculations. However, it should be emphasized that the presently shown large uncertainty to simulate future scenarios of climate change relates to an insufficient understanding of several strong feedback mechanisms (involving for an example the land, ocean, sea ice and clouds) within the climate system of study, and therefore to an inadequate description of these processes in our models. It is important to realize that climate projections or climate impact assessments never can be better than our understanding of the processes that control the climate system itself. Credible projections about a changing climate can only be based on knowledge, which is a limiting factor at most locations, and not on improved model resolution. [Caroline Leck (Reviewer's comment ID #: 144-15)]	Noted. While we agree with the sentiments, we have to consider only the radiative forcing aspects here. The feedback issues are dealt with elsewhere in Ch7, and Ch9.
2-563	A	26:28		Please be consistent with the use of in-situ throughout the Chapter. [Caroline Leck (Reviewer's comment ID #: 144-9)]	Accepted. In situ now used throughout.
2-564	A	26:30	26:34	Is the time-scale really a significant problem here? [Govt. of Finland (Reviewer's comment ID #: 2009-16)]	Rejected. Yes timescales are an issue – for example aircraft observations are not routine and thus a climatology cannot be derived for comparison with model estimates.
2-565	A	26:42		Other satellite aerosol retrieval results should be included in this part. Although current state-of-art operational aerosol retrieval algorithm by Kaufman et al. (1997) for MODIS data has provided many scientific results, other retrieval algorithms have been also used for aerosol retrieval for different satellite sensors such as ones on stationary satellites (GMS, GOES, NOAA/AVHRR, METEOSAT, etc), ocean color sensors (OCTS, SeaWiFS, etc.), POLDER, and MERIS. Recently a newly developed aerosol retrieval algorithm called as BAER (Bremen Aerosol Retrieval) by von Hoyningen-Huene et al.	Noted. Our aim here is to provide an assessment, rather than a review of teh satellite retrievals. They therfore have to be capable of providing well validated estimates that can be brought to bear against the problem global direct radiative forcing. So far only 4

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				(2003) has been used for SeaWiFS and other satellites over the bright-reflecting surface to retrieve aerosol properties. Validations of BAER using SeaWiFS data were reported by Kokhanovsky et al. (2004), Lee et al. (2004), and Lee et al. (2005). The present contribution is focused on MERIS and MODIS L1 data to investigate the effects of forest fires and urban pollution on the aerosol optical thickness (AOT) over Europe and Northeast Asia [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-1)]	studies have attempted this (section 2.1.4.1.3). Thus while the AODs may prove useful for validation, more weight has to be given to those products that are capable of providing independent assessments of the combined DRF due to aerosols.
2-566	A	26:54		Recently the modified BAER (Lee et al., 2004, Lee et al., 2005) algorithm has been used to retrieve the AOT from SeaWiFS and MODIS data over Northeast Asia region. Since this area is impacted by the largest anthropogenic and natural aerosol emission sources in the world, their contribution to the global scale aerosol radiative forcing is of scientific interest. Spatial distribution of AOT as derived using SeaWiFS and MODIS data shows that it varies up to 0.7 for Asian dust cases (Lee et al., 2004). Carbonaceous aerosol from anthropogenic and biomass burning in Northeast Asia, however, is much more important in this area, where AOT reaches over 3.0 occasionally (Lee et al., 2005). [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-2)]	See 2-565. Lee et al. Paper included as study adressess constraint on RF by observations
2-567	A	27:7	27:9	The industrial aerosol seems to have only a model effect on T_aer compared to mineral dust, based on 2.13. A comment to this effect might be helpful. [Nathan Gillett (Reviewer's comment ID #: 84-46)]	Noted. Not quite sure what the reviewer means by 'model' does he mean 'modest'? The reviwer's comment is not clear ...... as it's the only comment on this Figure we will leave the text and the figures the same.
2-568	A	27:11	27:11	grammatical issue: 'ocean glint' is surface characteristic, not a 'surface' [ian Enting (Reviewer's comment ID #: 63-6)]	Accepted.
2-569	A	27:18	27:18	AVHRR)" should be "AVHRR [Hongbin Yu (Reviewer's comment ID #: 299-1)]	Accepted.
2-570	A	27:18		AVHRR)" to "AVHRR [Junying Sun (Reviewer's comment ID #: 261-14)]	Accepted.
2-571	A	27:28	27:	add the following text to the end of paragraph: "The aerosol optical depth (AOD) products generated from the AVHRR (Mitshchenko et al. 1999) and TOMS (Torres et al. 2002) were compared and their synergy was explored (Jeong and Li 2005). While the two products exhibit common spatial features, considerable discrepancies exist in the magnitude of AOD. Taking advantage of different sensitivities of the two products to aerosol particle size and absorption, the global aerosols were classified into 8 types. Applying different spectral conversation functions for different aerosol types, a new integrated AOD at a common wavelength was generated over both ocean and land (Jeong and Li 2005)." Jeong, M.-J., Z.	Noted. Too detailed for inclusion. The reviewer's paper Jeong et al 2005 is already quoted in this regard, but the text cannot be added succinctly.

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				Li, 2005: Quality, Compatibility and Synergy Analyses of Global Aerosol Products derived from the Advanced Very High Resolution Radiometers and Total Ozone Mapping Spectrometers,, J. Geophy. Res., 110, D10S08, doi:10.1029/ 2004JD004647. [Zhanqing Li (Reviewer's comment ID #: 147-1)]	
2-572	A	27:32	27:32	Angstrom exponent, alpha..." instead of "Angstrom exponent, A… [Tiziano Colombo (Reviewer's comment ID #: 46-6)]	Accepted. The Angstrom exponent is now defined in Table 2.2.
2-573	A	27:32		What is the Angstrom exponent, and what is its significance? [Nathan Gillett (Reviewer's comment ID #: 84-47)]	Accepted. The Angstrom exponent is now defined in Table 2.2.
2-574	A	27:40		Add "Jeong and Li, 2005" after "Kaufman et al., 2005" [Zhanqing Li (Reviewer's comment ID #: 147-2)]	Perhaps, space permitting.
2-575	A	27:51	27:51	alpha" instead of "A [Tiziano Colombo (Reviewer's comment ID #: 46-7)]	Accepted. The angstrom exponent is now defined in Table 2.2.
2-576	A	27:53	27:53	alpha" instead of "A [Tiziano Colombo (Reviewer's comment ID #: 46-8)]	Acceptted. The angstrom exponent is now defined in Table 2.2.
2-577	A	28:8	28:9	Without giving a specific example this sentence is quite pointless [Ina Tegen (Reviewer's comment ID #: 263-1)]	Rejected. The reader will have to go to the cited references owing to length constraints.
2-578	A	28:8		Before "while some systematic …" add "Jeong et al. (2005) demonstrated that discrepancies between the AODs derived from MODIS and AVHRR by using two different aerosol models (Kaufman et al. 2002; Mishchenko et al. 1999) are substantial." [Zhanqing Li (Reviewer's comment ID #: 147-3)]	Noted. Jeong et al reference added.
2-579	A	28:9	28:9	add "; Kaufman et al., 2005c" after "Bellouin et al., 2005" [Hongbin Yu (Reviewer's comment ID #: 299-2)]	Accepted.
2-580	A	28:12		add "Although aerosol retrievals from dedicated instruments such as the MODIS and POLDER are more reliable than those from the historical sensors such as the AVHRR and TOMS, their compatibility is essential in establishing a long record for studying the long term trend. At present, the various AOD products available show similar features in spatial distribution and, to some extent, in its seasonal variation. However, the magnitudes of their differences are more than the spatial and temporal variability (Jeong et al. 2005, Jeong and Li 2005, Myhre et al. 2005). While the causes for the differences are understood reasonably well, reconciliation of the differences and generation of consistent and integrated products are still far from being achieved. It is highly desired to first reconcile differences among the AOD products derived from the modern sensors (e.g. MODIS and MISR) and then use them as anchors to bridge the long-term products from historical sensors (e.g. AVHRR and TOMS)."	Rejected for two reasons. 1) space constraints. 2) we cannot be prescriptive and suggest that 'it is highly desireable to …..' as this can be interpreted as a plea for research into this specific area.

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				[Zhanqing Li (Reviewer's comment ID #: 147-4)]	
2-581	A	28:12		One part missing in current assessment report is that the surface albedo from enhanced satellite sensors is providing a boundary condition for the aerosol forcing calculation. I would suggest to add a paragraph here: "New satellite-borne instruments, such as MODIS and MISR, are charactering the surface optical properties at multiple wavelengths and angles and at spatial resolutions as fine as 1 km (Moody et al., 2005; Schaaf et al., 2002; Martonchik et al., 1998). These new datasets have provided better lower boundary conditions to the radiative transfer models and reduce the uncertainty in the estimate of aerosol direct effect (e.g., Yu et al., 2004). The MODIS retrieved angular dependence of the surface reflection, i.e., a separation of direct beam and diffuse light contribution, also provides an unprecendented dataset for examining how the modifications of the directional and spectral composition of incident solar radiation by aerosols could alter the surface reflection and the solar energy budget, adding to the aerosol direct effect (Yu et al., 2004). " [Hongbin Yu (Reviewer's comment ID #: 299-3)]	Noted. Although we agree with the reviewers sentiments, space constraints simply do not permit this detail here.
2-582	A	28:13	28:13	I think you need to spell out that these are (at least I think) all shortwave DRE's [Keith Shine (Reviewer's comment ID #: 236-29)]	Accepted.
2-583	A	28:14	28:22	The concept of DRE is useful to distiguish total aerosol effect from anthropogenic forcing (RF), this should actually be introduced when the concept of RF is explained (section 2.2) [Ina Tegen (Reviewer's comment ID #: 263-2)]	Noted. However, we keep the DRE explanation here as it is important to re-iterate the difference between DRE and DRF.
2-584	A	28:21		regions;" to "regions, [Junying Sun (Reviewer's comment ID #: 261-15)]	Rejected.
2-585	A	28:22	28:22	add "or clouds are optically thin" after "(see Section 2.4.5.4)". [Hongbin Yu (Reviewer's comment ID #: 299-4)]	Accepted.
2-586	A	28:22		At the end of the paragraph, add "The aerosol DRF under cloudy conditions should not be ignored for absorbing aerosols which enhance atmospheric absorption and reduce or even reverse the aerosol cooling effect of the atmosphere-surface system, noting that absorbing aerosols under cloudy conditions have a warming effect at the TOA and cooling effect at the surface (Li and Trishchenko 2001)" [Zhanqing Li (Reviewer's comment ID #: 147-5)]	Rejected. Only passing reference is made to this effect here owing to space constraints.
2-587	A	28:28	28:28	"RF efficiency" … surely it is "DRE efficiency"? [Keith Shine (Reviewer's comment ID #: 236-30)]	Accepted.
2-588	A	28:28		viz." to "i.e. [Junying Sun (Reviewer's comment ID #: 261-16)]	Accepted.
2-589	A	28:37	28:37	Double the confidence limits to two standard deviations:to "-0.8 ± 0.4"	Rejected. This is the uncertainty that is

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				[VINCENT GRAY (Reviewer's comment ID #: 88-241)]	estimated in Bellouin et al. from a Monte Carlo technique. We cannot rewrite a paper ……
2-590	A	28:37	28:37	It appears that "-0.8+-0.2W/m2" should be "-1.9+-0.3W/m2". Please also change relevant discussion accordingly. [Hongbin Yu (Reviewer's comment ID #: 299-5)]	Rejected. This is not the case, the Bellouin study makes an assumption that there is no radiative forcing in cloudy sky regions.
2-591	A	28:42	28:42	Double the confidence limits to two standard deviations; "factor of two" to "factor of four" [VINCENT GRAY (Reviewer's comment ID #: 88-242)]	Rejected. These are the uncertainties reported in the papers. Each set of authors use a different methodology for estimating the uncertainty, and some do not include further sources of uncertainty such as those due to negative/positive radiative forcings from cloudy regions. We cannot rewrite the papers …….
2-1315	B	28:48		2.4.3 L48: suggest that the World Meteorological Organisation Global Atmosphere Watch (WMO GAW) aerosol optical depth Network be also mentioned alongside AERONET. Reference: Wehrli, C. (2005) GAWPFR: A network of Aerosol Optical Depth observations with Precision Filter Radiometers . in 'WMO GAW Experts workshop on a global surface based network for long term observations of column aerosol optical properties, GAW Report No. 162, WMO TD No. 1287 (2005). [Govt. of Ireland (Reviewer's comment ID #: 2025-2)]	Rejected. As of 2003, there are only eight of the filter radiometer sites – more than an oreder of magnitude less than the AERONET sites. Although these instruments are capable of providing high quality data, they cannot provide the same sort of spatial coverage as AERONET …..
2-1316	B	28:50		2.4.3 L50: Should add 'EARLINET ASOS (Advanced Sustainable Observation System) ' following on from EARLINET, [Govt. of Ireland (Reviewer's comment ID #: 2025-3)]	Rejected. Not deemed necessary.
2-592	A	28:53	28:53	since 1993' is unclear- in the figure caption it is stated that AERONET stations are shown that operate since 1996, this should be clarified [Ina Tegen (Reviewer's comment ID #: 263-3)]	Accepted. The wording is dropped and the caption expanded.
2-593	A	29:3	28:29	May be determined' - add 'at particular wavelengths' [Ina Tegen (Reviewer's comment ID #: 263-4)]	Accepted.
2-594	A	29:5	29:5	"omega zero" parameter is not defined [Tiziano Colombo (Reviewer's comment ID #: 46-9)]	Rejected. Defined in 2.4.1.
2-595	A	29:7	29:7	add "A climatology of the aerosol direct effect based on the AERONET aerosols has been derived (Zhou et al., 2005)". [Hongbin Yu (Reviewer's comment ID #: 299-6)]	Accepted.

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2-596	A	29:13	29:15	ADNET-Asia lidar network. It should add Chinese Lidar stations, such as Hefei, Anhui. [Govt. of China (Reviewer's comment ID #: 2006-30)]	Rejected. The cited work by Murayama includes findings from Hefei operated by the AOIFM institute.
2-597	A	29:17	45:37	A significant omission from this section is any discussion of the development of global aerosol microphysics models for predicting aerosol size distributions and, therefore, cloud condensation nuclei (CCN) concentrations. This has been a widespread activity pursued by many research groups since the IPCC TAR. Such a discussion could logically fit into one of several sections: Section 2.4.4 "Advances in Modeling", Section 2.4.6.2 "Estimates of the RF due to albedo effect from GCMs", and/or Section 2.4.6.5 "Uncertainties in RF due to model biases". For example, Section 2.4.6.5 (page 2-45, line 6) cites Feingold (2003), which demonstrates how aerosol size distribution is a significant determinant of the cloud albedo effect. It seems appropriate to mention the considerable resources that have been dedicated to developing a prognostic representation of the aerosol size distribution in global models. [Govt. of United States of America (Reviewer's comment ID #: 2023-69)]	Yes, point well taken. The discussion on the effects of aerosols on cloud microphysics is mostly done in Chapter 7, with mechanisms and processes that are relevant to radiative forcing included briefly in this chapter 2. We have a disuccion son Feingold papers as well as McFiggans review and others papers included in chapter 2.
2-598	A	29:17	45:37	As rightly pointed out, the majority of radiative forcing estimates invoke empirical parameterizations of the relationship between cloud droplet number concentration (CDNC) and aerosol (often sulfate) mass (e.g. Boucher and Lohmann, 1995). A weakness of this approach was first pointed out by Kiehl et al. (2000), where it was shown that a variety of such empirical relations have been proposed and give significantly different estimates of the cloud albedo effect. Given the fact that such variability is the empirical relationship is likely real (i.e. not measurement error), they suggested taking a more mechanistic approach to predicting CCN in global models. A further drawback of the empirical approach is that aerosol mass is a poor surrogate for CCN concentrations. For example, using a global aerosol microphysics model, Adams and Seinfeld (2003) found differences of a factor of two in CCN concentrations between two simulations with (essentially) the same aerosol mass concentrations because of different underlying aerosol microphysical processing. A related disadvantage of the empirical approach is that the aerosol microphysical pathways that produce CCN are treated implicitly, making the method something of a "black box". While such simplicity may be useful in many applications, explicit aerosol microphysics allows one to single out specific physical mechanisms and their uncertainties (e.g. nucleation) in sensitivity tests to evaluate how such uncertainties translate into uncertainties in the indirect effect itself. [Govt. of United States of America (Reviewer's comment ID #: 2023-70)]	Noted. The text emphasizes the point of the empirical relationships between aerosol concentrations and droplet number concentrations. This of course is critically important to the indirect effect forcing, as explicitly mentioned in the text.
2-599	A	29:17	45:37	As an indicator of the amount of work in this area, I am including a (probably incomplete) list of publications related to global aerosol microphysics modeling since the TAR. These	Noted. It is important to make clear that is not possible to cite all

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				include papers discussing model development, application to the indirect effect, and algorithm intercomparison. Note that few, if any, of these publications were published in time to be cited in the IPCC TAR report, so that this indeed represents a new trend in global aerosol modeling. [Govt. of United States of America (Reviewer's comment ID #: 2023-71)]	publications in a particular area. WE cite the most important and relevant for the forcing values used in this chapter, together with the relevant papers dealing with the processes.
2-600	A	29:17	45:37	Adams, P.J., and J.H. Seinfeld, Predicting global aerosol size distributions in general circulation models, Journal of Geophysical Research, 10.1029/2001JD001010, 2002. [Govt. of United States of America (Reviewer's comment ID #: 2023-72)]	Noted.
2-601	A	29:17	45:37	Adams, P.J., and J.H. Seinfeld, Disproportionate impact of particulate emissions on global cloud condensation nuclei concentrations, Geophysical Research Letters, 10.1029/2002GL016303, 2003. [Govt. of United States of America (Reviewer's comment ID #: 2023-73)]	Noted.
2-602	A	29:17	45:37	Boucher, O., and U. Lohmann, The Sulfate-Ccn-Cloud Albedo Effect - a Sensitivity Study with 2 General-Circulation Models, Tellus Series B-Chemical and Physical Meteorology, 47 (3), 281-300, 1995. [Govt. of United States of America (Reviewer's comment ID #: 2023-74)]	Noted.
2-603	A	29:17	45:37	Easter, R.C., S.J. Ghan, Y. Zhang, R.D. Saylor, E.G. Chapman, N.S. Laulainen, H. Abdul-Razzak, L.R. Leung, X.D. Bian, and R.A. Zaveri, MIRAGE: Model description and evaluation of aerosols and trace gases, Journal of Geophysical Research-Atmospheres, 109 (D20), 2004. [Govt. of United States of America (Reviewer's comment ID #: 2023-75)]	Noted.
2-604	A	29:17	45:37	Ghan, S.J., R.C. Easter, E.G. Chapman, H. Abdul-Razzak, Y. Zhang, L.R. Leung, N.S. Laulainen, R.D. Saylor, and R.A. Zaveri, A physically based estimate of radiative forcing by anthropogenic sulfate aerosol, Journal of Geophysical Research-Atmospheres, 106 (D6), 5279-5293, 2001. [Govt. of United States of America (Reviewer's comment ID #: 2023-76)]	Noted.
2-605	A	29:17	45:37	Gong, S.L., and L.A. Barrie, Simulating the impact of sea salt on global nss sulphate aerosols, Journal of Geophysical Research-Atmospheres, 108 (D16), 2003. [Govt. of United States of America (Reviewer's comment ID #: 2023-77)]	Noted.
2-606	A	29:17	45:37	Gong, S.L., L.A. Barrie, J.P. Blanchet, K. von Salzen, U. Lohmann, G. Lesins, L. Spacek, L.M. Zhang, E. Girard, H. Lin, R. Leaitch, H. Leighton, P. Chylek, and P. Huang, Canadian Aerosol Module: A size-segregated simulation of atmospheric aerosol processes for climate and air quality models - 1. Module development, Journal of Geophysical Research-Atmospheres, 108 (D1), 2003. [Govt. of United States of America (Reviewer's comment ID #: 2023-78)]	Noted.
2-607	A	29:17	45:37	Herzog, M., D.K. Weisenstein, and J.E. Penner, A dynamic aerosol module for global chemical transport models: Model description, Journal of Geophysical Research-	Noted.

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				Atmospheres, 109 (D18), 2004. [Govt. of United States of America (Reviewer's comment ID #: 2023-79)]	
2-608	A	29:17	45:37	Jacobson, M.Z., Analysis of aerosol interactions with numerical techniques for solving coagulation, nucleation, condensation, dissolution, and reversible chemistry among multiple size distributions, Journal of Geophysical Research-Atmospheres, 107 (D19), 2002. [Govt. of United States of America (Reviewer's comment ID #: 2023-80)]	Noted.
2-609	A	29:17	45:37	Jung, C.H., Y.P. Kim, and K.W. Lee, Multicomponent aerosol dynamics model with gas/particle transport and modal approach, Environmental Engineering Science, 21 (4), 437-450, 2004. [Govt. of United States of America (Reviewer's comment ID #: 2023-81)]	Noted.
2-610	A	29:17	45:37	Kiehl, J.T., T.L. Schneider, P.J. Rasch, M.C. Barth, and J. Wong, Radiative forcing due to sulfate aerosols from simulations with the National Center for Atmospheric Research Community Climate Model, Version 3, Journal of Geophysical Research-Atmospheres, 105 (D1), 1441-1457, 2000. [Govt. of United States of America (Reviewer's comment ID #: 2023-82)]	Noted.
2-611	A	29:17	45:37	Pierce, J.R., and P.J. Adams, Global evaluation of CCN formation by direct emission of sea salt and growth of ultrafine sea salt, Journal of Geophysical Research, 111, 10.1029/2005JD006186, 2006. [Govt. of United States of America (Reviewer's comment ID #: 2023-83)]	Noted.
2-612	A	29:17	45:37	Rodriguez, M.A., and D. Dabdub, A modeling study of size- and chemically resolved aerosol thermodynamics in a global chemical transport model, Journal of Geophysical Research-Atmospheres, 109 (D2), 2004. [Govt. of United States of America (Reviewer's comment ID #: 2023-84)]	Noted.
2-613	A	29:17	45:37	Spracklen, D.V., K.J. Pringle, K.S. Carslaw, M.P. Chipperfield, and G.W. Mann, A global off-line model of size-resolved aerosol microphysics: I. Model development and prediction of aerosol properties, Atmospheric Chemistry and Physics, 5, 2227-2252, 2005. [Govt. of United States of America (Reviewer's comment ID #: 2023-85)]	Noted.
2-614	A	29:17	45:37	Stier, P., J. Feichter, S. Kinne, S. Kloster, E. Vignati, J. Wilson, L. Ganzeveld, I. Tegen, M. Werner, Y. Balkanski, M. Schulz, O. Boucher, A. Minikin, and A. Petzold, The aerosol-climate model ECHAM5-HAM, Atmospheric Chemistry and Physics, 5, 1125-1156, 2005. [Govt. of United States of America (Reviewer's comment ID #: 2023-86)]	Noted.
2-615	A	29:17	45:37	Vignati, E., J. Wilson, and P. Stier, M7: An efficient size-resolved aerosol microphysics module for large-scale aerosol transport models, Journal of Geophysical Research-Atmospheres, 109 (D22), 2004. [Govt. of United States of America (Reviewer's comment ID #: 2023-87)]	Noted.

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2-616	A	29:17	45:37	Wilson, J., C. Cuvelier, and F. Raes, A modeling study of global mixed aerosol fields, Journal of Geophysical Research-Atmospheres, 106 (D24), 34081-34108, 2001. [Govt. of United States of America (Reviewer's comment ID #: 2023-88)]	Noted.
2-617	A	29:17	45:37	Zhang, Y., R. Easter, S. Ghan, and H. Abdul-Razzak, Impact of aerosol size representation on modeling aerosol-cloud interactions, Journal of Geophysical Research, 107 (D21), Art. No. 4558, 2002. [Govt. of United States of America (Reviewer's comment ID #: 2023-89)]	Noted.
2-618	A	29:22	29:23	Please include information which species are modeled [Ina Tegen (Reviewer's comment ID #: 263-5)]	Rejected. The next sentence refers to the tables which explicitly state the aerosol components that are considered.
2-619	A	29:23		See comment #4. [Caroline Leck (Reviewer's comment ID #: 144-5)]	*species* is a common, name for atoms, molecules, molecular fragments and ions as entities being subjected to a chemical process or to a measurement. Generally, a chemical species can be defined as an ensemble of chemically identical molecular entities that can explore the same set of molecular energy levels on a characteristic or delineated time scale. The term may be applied equally to a set of chemically identical atomic or molecular structural units in a solid array.
2-620	A	29:37		What is done for the meteorological fields in Experiment B, using prescribed emissions for 1750? [Adrian Simmons (Reviewer's comment ID #: 242-29)]	Noted. Meteorologcial fields correspond to year 2000. A new citation has been added to refer to a more detailed description of the experiment B and PRE.
2-621	A	29:51	29:52	The tendency to use similar emission data for anthropogenic aerosols may lead to too low assumed uncertainties in those estimates. [Ina Tegen (Reviewer's comment ID #: 263-6)]	Rejected. The uncertainty is mulitplied by the emissions uncertainty as stated in 2.4.5.
2-622	A	29:55	29:56	I do not understand this sentence [Claudia Marcolli (Reviewer's comment ID #: 158-10)]	Accepted. Sentence clarified.
2-623	A	30:8		Specify the wavelength associated with the optical depths in the sentence. [Govt. of United States of America (Reviewer's comment ID #: 2023-90)]	Accepted.

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2-624	A	30:18	30:18	Before "Liu et al. (2005)…", add "Yu et al. (2003) integrate MODIS aerosol optical depths into model simulations with an optimum interpolation approach, deriving an annual cycle of optical depth with a global coverage that is better correlated with AERONET measurements than are either satellite retrievals or model simulations alone. On global average, this integration increases the model-calculated clear-sky direct effect by about 40% (Yu et al., 2004)". [Hongbin Yu (Reviewer's comment ID #: 299-7)]	Rejected. Too detailed, but the Yu et al. 2003 reference is now included.
2-625	A	30:20	30:26	In the Bates studey the cause of the bias is identified. [Joyce Penner (Reviewer's comment ID #: 197-9)]	Noted.
2-626	A	30:24	30:26	Change sentence to "With the use of constrained quantities (extensive and intensive parameters) the calculated uncertainty in DCF was 25% less than the structural uncertainties used in the TAR global estimates of direct aerosol climate forcing." This is the revised conclusion from that paper and is now published in ACP. [Timothy Bates (Reviewer's comment ID #: 14-1)]	Rejected. The reviewer's comment uses DCF which we do not understand …..
2-627	A	30:26	30:28	But, fitting using assimilation methods does not tell you what aspect of the model is biased (I.e. sources, sinks, optical properties, meteorology) [Joyce Penner (Reviewer's comment ID #: 197-8)]	Noted. However it is not clear to which part of the text this comment is going.
2-628	A	30:28		General Comments on section 2.4.5: During the past 5 years, a government level of Sino-Japan Joint Project on 'Aeolian Dust Experiment on Climate Impact' (ADEC) has been implemented (e.g., see Mikami et al., 2006) and a lot of achievements and new findings have been reached (e.g., see Special Issue on ADEC, Journal of the Meteorological Society of Japan, Vol. 83A, March 2005, and total of 20 papers are there). It has somehow similar importance with several international aerosol projects such as the ACE-Asia, especially for the Asian Dust, it should be reflected in the related parts of the AR4 of IPCC WG1. During ADEC field campaigns, three sky-radiometers were operated in the dust source region (Aksu, Qira, and Shapotou in China), two in a downwind area in China (Beijing and Qindao), and four in Japan (Naha, Fukuoka, Nagoya, and Tsukuba) (Uchiyama et al., 2005b). Based on the ADEC data, sensitivity experiments of direct RF caused by MD for the optical and physical properties of MD were conducted using one dimensional radiative transfer model, the Streamer-based Radiative Transfer Model for ADEC Sciences (SARTMAS) (Aoki et al., 2005b) and using a k-distribution model for solar and thermal radiation transfer (Shi et al., 2005). Aoki et al. (2005b) simulated the atmospheric and dust profiles with a chemical transport model MASINGAR (Tanaka et al., 2005) at four locations: the Sea of Japan, the desert in Tarim Basin, the Sahara Desert, and snow in Siberia. The experiment results confirmed that the sensitivity of instantaneous RF in the shortwave (SW) region at the top of the	Accepted in part, ADEC is now properly referenced.

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				atmosphere (TOA) to the refractive index strongly depends on surface albedo. Namely, the effect of the difference in the MD model on instantaneous RF is significant over high albedo surfaces and is relatively small over the sea because the multiple reflections between the atmosphere (dust) and surface enhance light absorption by dust particles over high albedo surfaces. Over desert surfaces, the instantaneous RF in SW at TOA produced both positive and negative values within the possible refractive index range of MD. The diurnally averaged RF in SW at TOA also produced both positive and negative values in the possible range of desert albedo. It was found that for small dust particles with an effective radius of less than 0.6 ?m, RFs by MD changed depending on the difference in surface type even if the broadband albedo was the same. The vertical positional relationship of cloud cover to dust layer was also very important for RF at TOA in all spectral regions over desert and sea surfaces. However, the effect of cloud cover was generally small over snow surface because cloud albedo was close to the underlying snow albedo. Shi et al. (2005) performed numerical sensitivity experiments to evaluate the impact of optical characteristics on the RF. The experiments involved the effects of refractive indices, SSA, asymmetry factor and optical depth of MD. They used an updated data set of refractive indices of ADEC-2 model in Aoki et al. (2005b), which represents East Asian dust, and the data set by Woodward (2001)*. The main differences between the two optical models are: (1) the real part of refractive index of the ADEC-2 model is slightly larger than that of the Woodward model (Woodward, 2001) at most wavelengths from solar to infrared bands; and (2) the imaginary part of refractive index of the ADEC-2 model is generally smaller than that of the Woodward model over solar wavelengths. Shi et al. used a k-distribution model (Shi, 1998) to calculate RF. Numerical simulation was conducted using the large dust event on 4 to 15 April 2001. The daily dust concentration was provided by the NARCM model by Gong et al. (2003). Their results indicate that the ADEC-2 model has stronger scattering and weaker absorption, which leads to higher negative forcing at the top of the atmosphere (TOA) as compared with the Woodward model (Fig. 20). SSA is a primary factor in determining whether RF due to MD is positive or negative in the atmosphere (Aoki et al., 2005b). Recently, SSA for Saharan dust has been found to have a higher value (Kaufman et al., 2001, Haywood et al., 2001, 2003) than that previously reported (Shettle and Fenn, 1979, Hess et al., 1998, Sokolik and Golitsyn, 1993). However, data on SSA features in dust source regions in East Asia are lacking to date. Uchiyama et al. (2005a) retrieved SSA from sky-radiometer data in the ADEC network sites from the Taklimakan Desert to Japan. The averaged SSAs during ADEC IOP1, April 2002, at Aksu, Qira, Shapatou, Qingdao, Naha, Fukuoka, Nagoya, and Tsukuba were 0.955, 0.933, 0.914, 0.942, 0.944, 0.953, 0.933, and 0.973, respectively. In	

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				addition to the retrieved SSA analysis, in situ measurements of SSA by Particle Soot Absorption Photometer (PSAP; Radiance Research) and Nephelometer (M903; Radiance Research) were carried out at Qira, Beijing, and Tsukuba (Uchiyama et al., 2005a). SSA measured by PSAP and M903 were between 0.91 and 0.93 at Qira, between 0.80 to 0.88 at Beijing, and between 0.8 and 0.9 at Tsukuba. The SSA measured by PSAP and M903 at Qira were consistent with the SSA inferred from the sky-radiometer. This means that unpolluted aeolian dust has lower absorption than originally believed. The SSA derived from PSAP and M903 at Beijing and Tsukuba is lower than the SSA inferred from the sky-radiometer. This is partly because the SSA derived from PSAP represents information of dust particles near the surface and, hence, represents the dust particles mixed with absorbing aerosols during long range transport within the boundary layer. References Aoki, T., Tanaka, T. Y., Uchiyama, A., Chiba, M., Mikami, M., Yabuki, S., and Key, J., 2005b. Sensitivity Experiments of Direct Radiative Forcing by Mineral Dust using Spectrally Detailed Radiative Transfer Model. J. Met. Soc. Japan. 83A, 315-331. Gong, S.L., Zhang, X. Y., Zhao, T. I., Mckendry, I. G., Jaffe, D. A., and Lu, N. M., 2003. Characterization of soil dust aerosol in China and its transport and distribution during 2001 ACE-Asia: 2. Model simulation and validation. J. Geophys. Res., 109, D9, doi:10.1029/2002JD002633. Haywood, J., Francis, P., Osborne, S., Glew, M., Loeb N., Highwood, E., Tanré, D., Myhre, G., Formenti, P., and Hirst, E., 2003. Radiative properties and direct radiative effect of Saharan dust measured by the C-130 aircraft during SHADE: 1. Solar spectrum. J. Geophys. Res., 108, 8577, doi:10.1029/2002JD002687. Hess, H., Koepke, P., and Schult, I., 1998. Optical Properties of Aerosols and Clouds: The Software Package OPAC, Bull. Am. Met. Soc., 79, 831-844. Kaufman, Y. J., Tanré, D., Dubovik, O., Karnieli, A., and Remer, L. A., 2001. Absorption of sunlight by dust as inferred from satellite and ground-based remote sensing. Geophys. Res. Lett., 28, 1479-1482. Mikami et al. 2005: Aeolian Dust Experiment on Climate Impact, 2006: An Overview of Japan-China Joint Project ADEC, Global Planetary Change, (accepted). Shettle, E. P., and Fenn, R. W., 1979. Models for the aerosols of the lower atmosphere and the effects of humidity variations on their optical properties, AFGL-TR-79-0214, Air Force Geophysics Laboratory, 94pp. Shi, G.-Y., 1998. On the k-Distribution and Correlated k-distribution Models in the Atmospheric Radiation Calculations. Scientia Atmospherica Sinica (Special Issue Dedicated to the 70th Anniversary of the Founding of the Institute of Atmospheric Physics, Chinese Academy of Sciences). 22, 555-576. Shi, G-Y., Wang, H., Wang, B., Li, W., Gong, S., Zhao, T. and Aoki, T., 2005. Sensitivity	

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				experiments on the effects of optical properties of dust aerosols on their radiative forcing under clear sky condition. J. Met. Soc. Japan. 83A, 333-346. Sokolik, I., and Golitsyn, G., 1993. Investigation of optical and radiative properties of atmospheric dust aerosols. Atmos. Environ., 27A, 2509-2517. Tanaka, T. Y., Kurosaki, Y., Chiba, M., Matsumura, T., Nagai, T., Yamazaki, A., Uchiyama, A., Tsunematsu, N., and Kai, K., 2005. Possible transcontinental dust transport from north Africa and the middle east to east Asia. Atmos. Environment. 39, 3901-3909. Uchiyama, A., Yamazaki, A.,Togawa, H., Asano, J., 2005a. Absorption Property of Aeolian Dust as inferred from Sky-radiometer and Ground-Based Measurement. Proceedings of the Fourth ADEC Workshop. Uchiyama, A., Yamazaki, A., Togawa, H., and Asano, J., 2005b. Characteristics of Aeolian dust observed by sky-radiometer in the ADEC Intensive Observation Period 1(IOP1). J. Met. Soc. Japan. 83A, 291-305. Woodward, S., 2001. Modeling the atmospheric life cycle and radiative impact of mineral dust in the Hadley Center climate model. J. Geophys. Res., 106, 18155–18166. [Guangyu Shi (Reviewer's comment ID #: 234-1)]	
2-629	A	30:30	30:31	"integrated over 24 hours" is confusing in this context. I assume that the annual global-mean quantity (including all times of day) is meant. [Claudia Marcolli (Reviewer's comment ID #: 158-11)]	Accpeted - reworded
2-630	A	30:34	:35	Define what is meant by "structural uncertainty". Add "structural uncertainty" and "value uncertainty" to the Glossary and refer to the Glossary when those terms are first used in the chapter. [Govt. of United States of America (Reviewer's comment ID #: 2023-91)]	Accepted in part. New text inserted. Box in TS referenced
2-631	A	30:37	30:39	Do the RFs of the different aerosols add linearly? [Nathan Gillett (Reviewer's comment ID #: 84-48)]	Noted. They do not necessarily add linearly, but we do present model estimates of the total radiative forcing too.
2-632	A	30:41	31:40	In that the modeling studies use the time history of the sulfate emissions, it seems to me it needs to be mentioned that the likely lifetime of the SO2 (and thus of the fraction that became sulfate) was greatly changed when emissions shifted from being near the surface to being emitted from tall stacks--a transition that occurred roughly in the second quarter of the 20th century. This would allow/lead to a really sharp increase in sulfate forcing in the Northern Hemisphere, and this remains, I believe, an inadequately addressed issue even though it could be crucial to the detection-attribution studies. [Michael MacCracken (Reviewer's comment ID #: 152-255)]	Accepted in part. The recently used emission scenarios (such as that from AeroCom) take into account stack heights and their regional, seasonal and present versus preindustrial variability. SO2 emissions are emitted accordingly at appropriate altitudes. Any effect on RF as defined for present day is thus addressed.

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					However, the time evolution of sulphate forcing is less well known. A remark has been added.
2-633	A	30:42	30:43	This sentence is imprecise. I would prefer: Atmospheric sulfate aerosol may be considered as consisting of sulfuric acid particles that are partly or even totally neutralized by ammonia and are present as liquid droplets or partly crystallized. [Claudia Marcolli (Reviewer's comment ID #: 158-12)]	Accpeted.
2-634	A	30:42	30:42	Current wording is not chemically correct: "chemical compounds H2SO4, NH4HSO4, and (NH4)2SO4". Suggest to change to: "chemical compound H2SO4 mixed with NH3 up to full neutralization (i.e., (NH4)2SO4)" [Scot Martin (Reviewer's comment ID #: 168-1)]	Noted. New wording put in.
2-635	A	30:42	30:43	Atmospheric sulphate aerosol consists mainly of the chemical compounds H2SO4, NH4HSO4, and (NH4)2SO4 in either the aqueous or crystalline form." One could say bluntly that this statement is wrong. There is no either liquid or cystalline. Either such mixtures are fully liquid or, if a part crystallizes, they will occur in mixed phase, i.e. there will always be some liquid. Reason is that a stoichiometric sulfate or bisulfate mixture will hardly ever occur in nature (and only those could become fully solid). Furthermore, the most likely solid to occur is neither the sulfate nor the bisulfate but the letovicite, e.g. Colberg et al., Atmos. Chem. Phys., 3, 909–924, 2003. Better: "Atmospheric 'sulphate aerosol' consists of mixtures of the H2SO4/NH3/H2O system, either in liquid form or as mixed-phase aerosol, most likely containing crystals of letovicite, (NH4)3H(SO4)2, and possibly some ammonium sulfate, (NH4)2SO4, or ammonium bisulfate, NH4HSO4. [Thomas Peter (Reviewer's comment ID #: 198-33)]	Noted. New wording is included – the wording suggested by the reviewer is just too detailed.
2-636	A	30:43		Sulphates does also form by aerosol condensational growth (mass transfer of gas phase sulfuric acid on preexisting aerosol). Please add!! [Caroline Leck (Reviewer's comment ID #: 144-18)]	Accepted.
2-637	A	30:46	30:47	It is not clear whether the 2 % from biomass buring come in additon to the 72 % of fossil fuel buring or is a part of it. The way it is written the latter would be the case, but this does not make sense and then also the percentages do not add up 100 %. [Thomas Peter (Reviewer's comment ID #: 198-34)]	Accepted and reworded.
2-638	A	30:46		Insert 'of total sulphate emissions' after 72%. Otherwise it looks like the percentage is of anthropogenic emissions. [Nathan Gillett (Reviewer's comment ID #: 84-49)]	Accepted and reworded.
2-639	A	30:48	30:57	A more recent SO2 histroical inventory is that by Stern, D.I., 2005:Global sulfur emissions from 1850 to 2000, Chemosphere, 58, 163-175. Global SO2 appears to have decreased since about 1980. (Steve Smith inventory (private communication) which is also newer also decreases since 1980).	Reference now included.

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				[Joyce Penner (Reviewer's comment ID #: 197-10)]	
2-640	A	30:50	30:52	correct English is "have been reduced" or "have decreased", but not "have reduced" [Danny Harvey (Reviewer's comment ID #: 101-11)]	Accepted.
2-641	A	30:53	30:55	The world appears to be divided into the USA, Europe, Asia and developing countries here. These categories seem to overlap (does 'developing countries' include developing countries in Asia?). I would suggest referring to continents only. [Nathan Gillett (Reviewer's comment ID #: 84-50)]	Noted. Difficult – the inventories that have been developed overlap .....
2-642	A	30:54	30:55	should read "from developing countries in other regions (Boucher and Pham, 2002)" [Danny Harvey (Reviewer's comment ID #: 101-12)]	Accepted.
2-643	A	31:7	31:7	"omega zero" parameter is not defined [Tiziano Colombo (Reviewer's comment ID #: 46-10)]	Rejected. It is defined in section 2.4.1.
2-644	A	31:8	31:8	Current wording is confusing because it does not specify that relative humidity dependence only applies to aqueous particles. Suggest to change "fRH" to "fRH, of aqueous sulfate particles". [Scot Martin (Reviewer's comment ID #: 168-2)]	Rejected. This is obvious.
2-645	A	31:12		Define 'accumulation mode mass' or use a simpler term. [Nathan Gillett (Reviewer's comment ID #: 84-51)]	Accepted. Changed to sub-micron.
2-646	A	31:14		What does 'internally mixed' and 'externally mixed' mean? [Nathan Gillett (Reviewer's comment ID #: 84-52)]	Rejected. Internal mixtures and external mixtures are standard terms for all aerosol researchers. Also the effects of this mixing are stated explicitly in the next sentence. See TAR chapters on Aerosols and RF where they were earlier discussed.
2-647	A	31:21	31:21	Double the confidence limits to two standard deviations:to "-0.5 ± 0.66" [VINCENT GRAY (Reviewer's comment ID #: 88-243)]	Rejected. However, the uncertainty limits are adjusted to the 5-95% confidence interval for consistency with the rest of the report.
2-648	A	31:21	31:40	There has been clear progress since the TAR, but it is not clear how the revised estimate of uncertainty has been actually calculated. It may be less than 0.8 W/m2 by why 0.2 W/m2? I note that line 39 says it is a 'suggested' value. [Michael Manton (Reviewer's comment ID #: 157-19)]	Accepted. We realise that the logic was not clear – we now are more explicit about how this is calculated.
2-649	A	31:25	31:25	remove the minus sign from the standard deviation [Govt. of Finland (Reviewer's comment ID #: 2009-17)]	Accepted.
2-650	A	31:25	31:25	Insert before "As in TAR" " (95% confidence limts of 0.3) 380 2-380 244 [VINCENT GRAY (Reviewer's comment ID #: 88-17)]	Rejected. The TAR did not specifically attempt to quantify the

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					90% confidence interval.
2-651	A	31:27	31:27	Change "hygroscopic growth," to "hygroscopic growth, physical state," [Scot Martin (Reviewer's comment ID #: 168-3)]	Rejected. Not necessary to include this here.
2-652	A	31:30	31:30	Quinn, P.K., T.S. Bates, T. Baynard, A.D. Clarke, T.B. Onasch, W. Wang, M.J. Rood, E. Andrews, J. Allan, C.M. Carrico, D. Coffman and D. Worsnop (2005), Impact of particulate organic matter on the relative humidity dependence of light scattering: A simplified parameterization, Geophys. Res. Lett., 32, L22809, doi:101029/2005GL024322. [Timothy Bates (Reviewer's comment ID #: 14-2)]	Rejected. This really is not relevant here, section is on sulphate aerosol.
2-653	A	31:30	31:30	The hygroscopicity will also be highly dependent on the mixing state. Quinn, P.K., T.S. Bates, T. Baynard, A.D. Clarke, T.B. Onasch, W. Wang, M.J. Rood, E. Andrews, J. Allan, C.M. Carrico, D. Coffman and D. Worsnop (2005), Impact of particulate organic matter on the relative humidity dependence of light scattering: A simplified parameterization, Geophys. Res. Lett., 32, L22809, doi:101029/2005GL024322. [Timothy Bates (Reviewer's comment ID #: 14-3)]	Noted. We include mixing state, but we cannot include a full reference for each point that is mentioned owing to space constraints. We mention the hygroscopicity explicitly.
2-654	A	31:30	31:30	Myhre's analysis is complemented by another study in 2004 concerning a sensitivity study of aerosol physical state on direct radiative forcing. Change "RF." to "RF. Martin et al. (2004) determined that the physical state of sulfate aerosols provides an uncertainty of up to 24% for global aerosol direct radiative forcing." (Martin et al. 2004 is listed in the references on page 2-95.) [Scot Martin (Reviewer's comment ID #: 168-4)]	Noted. We now include reference to the mixing state.
2-655	A	31:34	31:34	Add at end (95% confidence, 0.4) [VINCENT GRAY (Reviewer's comment ID #: 88-245)]	Noted. Reworded.
2-656	A	31:39	31:39	Double the confidence limits to two standard deviations:to "-0.24 ± 016" [VINCENT GRAY (Reviewer's comment ID #: 88-246)]	Noted. 90% confidence interbvals are used.
2-657	A	31:45		To make this more easily understandable to a wide audience, it might be helpful to define 'organic' here ie. 'Organic aerosols are a complex mixture of chemical compounds containing carbon-carbon bonds….'. [Nathan Gillett (Reviewer's comment ID #: 84-53)]	Accepted.
2-658	A	31:54	31:56	Emmisions of particulate organic matter are given in Ito and penner (not OC, which usually has units of Tg C/yr). Year 2000 fossil fuel POM emissions were 3.1 Tg/yr and year 2000 biofuel emissions were 11.4 Tg/yr. [Joyce Penner (Reviewer's comment ID #: 197-11)]	Accepted. Reworded accordingly.
2-659	A	31:56	31:56	Atmospheric aging processes are omitted in the discussion. Suggest to change "7.5 Tg C yr-1" to "7.5 Tg C yr-1. Moreover, hygrosopic, chemical, and optical properties of OC particles continue to change during a particle's atmospheric lifetime because of chemical	Accepted.

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				processing by gas-phase oxidants such as O3, OH, and NO3 (Kanakidou et al. 2005)." (Kanakidou et al. 2005 is listed in the references on page 2-90.) [Scot Martin (Reviewer's comment ID #: 168-5)]	
2-660	A	32:9	32:9	"Observational evidence suggests that some organic aerosol compounds from fossil fuels are relatively weakly absorbing but do absorb solar radiation at some wavelengths (e.g., Bond et al., 1999; Bond, 2001)" The measured radiative properties and modeled effects of UV- and visible-absorbing organic compounds were previously discussed and quantified in Jacobson, M. Z., Isolating nitrated and aromatic aerosols and nitrated aromatic gases as sources of ultraviolet light absorption, J. Geophys. Res., 104, 3527-3542, 1999 [Mark Jacobson (Reviewer's comment ID #: 116-3)]	Accepted. This study is a good one that nicely demonstrates the absorption of OC and is now referenced.
2-661	A	32:19		See comment #4. [Caroline Leck (Reviewer's comment ID #: 144-6)]	*species* is a common, name for atoms, molecules, molecular fragments and ions as entities being subjected to a chemical process or to a measurement. Generally, a chemical species can be defined as an ensemble of chemically identical molecular entities that can explore the same set of molecular energy levels on a characteristic or delineated time scale. The term may be applied equally to a set of chemically identical atomic or molecular structural units in a solid array.
2-662	A	32:30	32:31	the presence of organic films as the reason for a suppressed rate of water uptake is not experimentally established for atmospheric aerosols. the sentence should be removed or changed to: However, if hygroscopic aerosol particles such as sulfate were coated by organic films the rate of water uptake during cloud activation might be suppressed. [Claudia Marcolli (Reviewer's comment ID #: 158-13)]	Rejected. The sentences seem identical in meaning ......
2-663	A	32:35	32:35	Secondary organic carbon is highly simplified in the global models …". True. Recommend rewording to "Secondary organic carbon is poorly understood and consequently highly simplified in the global models. [Dylan Millet (Reviewer's comment ID #: 178-10)]	Rejected. It is not necessarily poorly understood. It is the sheer complexity that makes the parameterisations highly simplified.
2-664	A	32:38	32:38	This comment addresses "time since emission", but also statements on lines 27 to 29, pg. 2-32 and the discussion in 2.4.5.7 (Combined aerosols). Most microscopic imaging	Accepted in part. Some of the models used for assessing the direct radiative

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				results of aged aerosol particles reveal an internal mixture of sea salt, mineral dust, black carbon, sulfate and organic components (Posfai and colleagues). This means that aerosols close to the source may be external mixtures, far from the source internal mixtures of sometimes absorbing aerosol components such as black carbon and mineral dust particles. To what extent do aerosol models capture the complexity of this situation? Quantitative estimates should be included if possible. This comment addresses "time since emission", but also statements on lines 27 to 29, pg. 2-32 and the discussion in 2.4.5.7 (Combined aerosols. Most microscopic imaging results of aged aerosol particles reveal an internal mixture of sea salt, mineral dust, black carbon, sulfate and organic components (Posfai and colleagues). This means that aerosols close to the source may be external mixtures, far from the source internal mixtures of sometimes absorbing aerosol components such as black carbon and mineral dust particles. To what extent do aerosol models capture the complexity of this situation? Quantitative estimates should be included if possible. [Michel J. ROSSI (Reviewer's comment ID #: 220-5)]	effect are now capable of describing multi-component mixing. Some description of model advances in describing aerosol microphysics is added in chapter 2.4.4. However, it is not obvious for what parameter quantitive estimates should be included..
2-665	A	32:45	32:45	Double the confidence limits to two standard deviations:to "-0.40 ± 0.40" [VINCENT GRAY (Reviewer's comment ID #: 88-247)]	Rejected ... although we use the 90% confidence interval.
2-666	A	32:46	32:46	Double the confidence limits to two standard deviations:to "-0.10 ± 0.20" [VINCENT GRAY (Reviewer's comment ID #: 88-248)]	Rejected ... although we use the 90% confidence interval.
2-667	A	32:48	32:54	The AeroCom RF of FF OC is -0.04 and non AeroCom is -0.06 W/m2 How do you derive a RF that is more negative than either number (I.e. -0.10 W/m2)?? [Joyce Penner (Reviewer's comment ID #: 197-12)]	Accepted. We now state -0.05Wm-2 through a more rigorous assessment of the results.
2-668	A	32:54	32:54	Double the confidence limits to two standard deviations:to "-0.18 ± 0.20" [VINCENT GRAY (Reviewer's comment ID #: 88-249)]	Noted. See comments on 90% confience intervals.
2-669	A	32:54	32:54	Delete "relatively" [VINCENT GRAY (Reviewer's comment ID #: 88-250)]	Accepted. Entire sentence deleted.
2-670	A	32:54	32:54	I don't quite understand the error bar on this, as it implies zero is a possible value - none of the previous text supports such a value, as far as I can tell. [Keith Shine (Reviewer's comment ID #: 236-31)]	Noted. The statistics suggest -0.05 with a standard deviation of 0.03. 1.645*0.03 gives 0 as a 90% lowest limit (although admittedly a Gaussian distribution is assumed).
2-671	A	33:1	33:1	I think you need to spell out what BCPOM means - for a while I convinced myself it was a typo for BBPOM … maybe BC+POM would be clearer. [Keith Shine (Reviewer's comment ID #: 236-32)]	Accepted. An extra line is included in the table caption.
2-672	A	33:5	33:5	"ESTIMATE OF global current day …"! [Keith Shine (Reviewer's comment ID #: 236-33)]	Accepted.
2-673	A	33:6		form" to "from	Accepted.

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				[Junying Sun (Reviewer's comment ID #: 261-17)]	
2-674	A	33:12	33:13	There is no paper to support "significant increases were reported in India and China". However,in papers of Streets et al., 2001, 2003 (in the references p2-106, L37 and L38). "BC emissions in China in 1995 were 1342 Gg (Atmos. Environ., 2001, 35, p4281)" and "We estimate 1.05 Tg for China in 2000 (JGR. 2003, 108 (D21), p30-9)". Streets et al. estimated that BC emissions could fall to 1224 Gg by 2020 and mentioned "this 9% decrease in BC emissions can be contrasted with the expected increase of 50% in energy use; the reduction will be obtained because of a transition to more advanced technology, including greater use of coal briquettes in place of raw coal in cities and towns. Atmos. Environ., 2001, 35, p4281." Therefore, the wrong description about increased BC emission in China should be deleted. [Govt. of China (Reviewer's comment ID #: 2006-31)]	Rejected. The Novakov et al paper is quite clear about this. See Figure 1 of the Novakov et al (2003) paper. This paper is now more clearly linked as the reference qualifying this statement. The trends we refer to concern the recent past, not the future (as discussed in Streets et al.).
2-675	A	33:22	33:22	structure' should be 'structures' [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-30)]	Accepted.
2-676	A	33:23	33:27	"The Indian Ocean Experiment (INDOEX, Ramanathan et al., 2001b and references therein) focused on emissions of aerosols from the Indian sub-continent, and showed the importance of absorption by aerosol in the atmospheric column. Their observations showed that the local surface forcing (-23 W/m2) was significantly stronger than the local RF at the top of the atmosphere (-7 W/m2)." The conclusion, "the importance of absorption by aerosol in the atmospheric column" was shown prior to INDOEX in Jacobson, M. Z., Studying the effects of aerosols on vertical photolysis rate coefficient and temperature profiles over an urban airshed, J. Geophys. Res., 103, 10,593-10,604, 1998 Figure 7a of that paper shows a reduction in surface solar radiation of -40 W/m2 and a slight increase in the upper atmosphere at Claremont, California, modeled from 1987 Southern California Air Quality Study initial data. At a less-polluted site (Long Beach), the surface reduction was -12 W/m2 and about -1 W/m2 at the TOA. The importance of aerosol absorption in reducing the surface solar irradiance relative to upper atmosphere irradiance is described on pages 10,601, Section 7, paragraphs 3-5 of the paper. The strong surface forcing due to aerosols containing absorbing black carbon at Riverside, California, was also previously calculated (and compared with data from other studies) in Jacobson, M. Z., Development and application of a new air pollution modeling system. Part III: Aerosol-phase simulations, Atmos. Environ., 31A, 587–608, 1997 The abstract of the paper states, "Finally, the presence of aerosols reduced peak daytime surface solar radiation by approximately 6.4% (55 W/m2)." Table 6 of the paper compares this result with data from other studies. Figure 71 shows a comparison of the	Rejected. We feel that the INDOEX study by Ramanathan AND REFERENCES THEREIN) provide a more comprehensive set of references for interested reader to look at. There are any number of modelling and measurement studies that show the effects of aerosol absorption upon the surface radiation balance, and we have to decide upon the most relevant and useful.

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				diurnal variation of modeled surface solar radiation with and without aerosols and compares the results with data. [Mark Jacobson (Reviewer's comment ID #: 116-4)]	
2-677	A	33:28	33:28	eliminate "is" [Thomas Peter (Reviewer's comment ID #: 198-36)]	Accepted.
2-678	A	33:28	33:28	delete "is" [Michel J. ROSSI (Reviewer's comment ID #: 220-6)]	Accepted.
2-679	A	33:40	33:40	I am irritated by the formulation "Non-AeroCom and AeroCom studies", as it gives one particular initiative too much weight (unless AeroCom is running under the auspices of IPCC and this is explicitly intended). May be "Several studies, including those performed within AeroCom, …" is the politically more correct and for "non-AeroComians" more acceptable formulation? AeroCom gets in any case 47 citation within this chapter, demonstrating its big success, and the 9 citations of "Non-AeroCom" should be avoided. [Thomas Peter (Reviewer's comment ID #: 198-37)]	Accepted. Formulations are revised.
2-680	A	33:40	33:55	Here the distinction between Aerocom and Non-Aerocom results is confusing rather than helpful. [Ina Tegen (Reviewer's comment ID #: 263-7)]	Accepted. Formulations are revised
2-681	A	33:41	33:41	The AeroCom models give the weaker RF estimate not stronger. [Joyce Penner (Reviewer's comment ID #: 197-13)]	Noted. A better analysis is now presented.
2-682	A	33:45	33:45	I didn't understand what is meant by a 34-38% split. [Daniel Murphy (Reviewer's comment ID #: 183-18)]	Noted.
2-683	A	33:52	33:53	The recommended value for the direct forcing of fossil-fuel black carbon (+0.2) is skewed by a preponderance of models that do not treat the internal mixing or evolution of the mixing state of black carbon and have a variety of other simplifications, such as not treating discretized size resolution of aerosol particles, chemical reaction in aerosol particles, and high-resolution spectral radiative transfer through aerosols and size-resolved clouds. Further, the most detailed calculation of the direct forcing of BC, which accounted for the evolution of its mixing state among 16 size distributions, Jacobson, M. Z., Strong radiative heating due to the mixing state of black carbon in atmospheric aerosols, Nature, 409, 695-697, 2001 was not even included in Table 2.5 (please see comment to Table 2.5, below). A direct forcing of fossil-fuel BC of +0.25 W/m2 would be more consistent with results from that study (+0.27 W/m2) and other detailed studies (e.g., Liao and Seinfeld, 2005). [Mark Jacobson (Reviewer's comment ID #: 116-5)]	Noted. Table 2.5 does include the study of Jacobson (2001) and quotes a FFBC of +0.27Wm-2. The uncertainty range of +0.20 (90% confidence 0.05 to 0.35Wm-2) easily encompasses ALL of the model estimates with the exception of the Hansen et al, 2005 study. The reviwer must recognise that there are a number of critical parameters in the GCM besides the accuracy of detailed aerosol scheme (e.g. surface reflectance characteristics, cloud amount, cloud overlap, relative humidity distributions) etc that must also be

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					accurately represented for the definitive radiative forcing estimate due to black carbon aerosols.
2-684	A	33:52	33:52	State that the mean for the non AeroCom results is 0.25 [Joyce Penner (Reviewer's comment ID #: 197-14)]	Accepted.
2-685	A	34:1	34:15	This section seems to overlap with the previous three sections - I didn't fully understand the rationale for including a separate section on biomass burning aerosols. Why not then include separate sections for e.g. transport emissions, or power generation emissions? Does this separation of biomass burning aerosols mean that its constituent parts e.g. sulphate aerosol from biomass burning are not accounted for in the previous separate sections? Biomass burning aerosols are not treated separately in figure 2.25. [Nathan Gillett (Reviewer's comment ID #: 84-55)]	Noted. The rationale for this is clearly stated. Also, see TAR chapters on Aerosols and RF.
2-686	A	34:7	:12	Agree with the authors that biomass aerosol emissions are essentially uncontrolled, and have less potential for coming under control compared to other aerosol emission sources. However, it would still be useful for IPCC to break out, if feasible, the separate BC and OC radiative forcing associated with biomass burning. This could be one estimate in a table, rather than keeping track of the separate forcings all throughout the report. [Govt. of United States of America (Reviewer's comment ID #: 2023-92)]	Noted. We think that the tables are quite complex enough. Infact there have been several criticisms that they are too complex. Adding another two columns to the table to break out the BBOC and BBBC would make the tables even more unwieldy.
2-687	A	34:17	34:39	There is too much detail in this paragraph, particularly on descriptions of particular research projects. [Nathan Gillett (Reviewer's comment ID #: 84-56)]	Noted. Biomass burning has recieved considerable attention since IPCC TAR. However, some of the wording has been reduced.
2-688	A	34:23		after "Ichoku et al., 2003)". add "Biomass burning aerosols produced by boreal forest fires tend to have weaker absorption than those from tropical fires (single scattering albedo greater than 0.9) (Li and Kou, 1998; Wong and Li 2002). During a peak month of biomass burning in the Canadian boreal forest region, the aerosol DRF at the surface accounts for one third of a total reduction of solar energy by both clouds and aerosols (Li and Kou 1998)." Li, Z., and L. Kou, 1998: Atmospheric direct radiative forcing by smoke aerosols determined from satellite and surface measurements, Tellus (B), 50, 543-554. Wong, J., and Z. Li, 2002, Retrieval of optical depth for heavy smoke aerosol plumes: uncertainties and sensitivities to the optical properties, J. Atmos. Sci., 59, 250-261. [Zhanqing Li (Reviewer's comment ID #: 147-7)]	Accepted in part. The Wong and Li reference is included.
2-689	A	34:29		after "Kirchstetter, 2004" add Vant-Hull et al., 2005. Vant-Hull, B., Z. Li, B. F. Taubman, R. Levy, L. Marufu, F.-L. Chang, B. G. Doddridge,	Rejected. Not relevant to SAFARI2000.

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				and R. R. Dickerson (2005), Smoke over haze: Comparative analysis of satellite, surface radiometer, and airborne in situ measurements of aerosol optical properties and radiative forcing over the eastern United States, J. Geophys. Res., 110, D10S21, doi:10.1029/2004JD004518. [Zhanqing Li (Reviewer's comment ID #: 147-6)]	
2-690	A	34:30	34:30	Are here near-source Aeronet sites meant? [Ina Tegen (Reviewer's comment ID #: 263-8)]	Accepted.
2-691	A	34:43	34:44	"The AeroCom and non-AeroCom models suggest an average global mean RF from biomass burning aerosols of +0.05 W m-2 and +0.07 W m-2". The discussion of a biomass burning aerosol forcing and climate effects does not include the paper, Jacobson, M. Z., The short-term cooling but long-term global warming due to biomass burning, J. Clim., 17 (15), 2909-2926, 2004 which accounted for the net effects of many particle components and gases from biomass burning: BC, OM, K+, Na+, Ca2+, Mg2+, NH4+, H+, Cl-, H2SO4, HSO4-, SO42-, CO, CH4, NOx, So2, C2H6, C2H4, C3H8, C3H6, size resolution of aerosols, evolution of size-resolved clouds from size-resolved aerosols, and many other features. The effect of CO2 was accounted for separately in the paper. The overall direct plus indirect (and other feedbacks) irradiance change at the tropopause due to biomass burning was calculated as -0.84 W/m2 (and about -0.6 W/m2 at the TOA) (Figure 7f of the paper). [Mark Jacobson (Reviewer's comment ID #: 116-6)]	Rejected. This section is about accurately diagnosing the DIRECT RADIATIVE FORCING due to biomass burning aerosols. There is no way of extracting this information from the paper that is mentioned here other than -0.84Wm-2 is estimated as the direct plus indirect radiative forcing due to biomass burning aerosols. The temperature reponse to the forcing which is more the focus of the paper is beyond the scope of the chapter.
2-692	A	35:0		Section 2.4.5.4: Is all biomass burning considered as anthropogenic here? This should be discussed. Boreal fires should be considered as largely natural emissions. [Ina Tegen (Reviewer's comment ID #: 263-10)]	Accepted. Wording altered accordingly.
2-693	A	35:0		Section 2.4.5.5.: Role of nitrate aerosol size in uncertainties of radiative forcing estmates? [Ina Tegen (Reviewer's comment ID #: 263-12)]	Noted. We now include more detail on the formation of nitrate aerosol, but we do not include a detailed description of the size distribution owing to length constraints.
2-694	A	35:2	35:7	See comment 7. Why the distinction between Aerocom and non-Aerocom here? [Ina Tegen (Reviewer's comment ID #: 263-9)]	Noted. AEROCOM emissions are prescribed.
2-695	A	35:5		Koch." to "Koch [Junying Sun (Reviewer's comment ID #: 261-18)]	Accepted.
2-696	A	35:7	35:8	What is the main reason of the difference compared to the TAR results? The reassessment of single scattering albedo for biomass burning?	Accpeted. The reason now stated.

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				[Ina Tegen (Reviewer's comment ID #: 263-11)]	
2-697	A	35:8		The value of 0.0 for the best estimate of radiative forcing for biomass burning aerosols does not appear to be a mean of the previously stated results - the mean of these appears to be positive. How was this result derived? [Nathan Gillett (Reviewer's comment ID #: 84-57)]	Accepted. This is changed in the revised text.
2-698	A	35:13	35:13	The direct forcing given for nitrate from Jacobson (2001) is incorrectly stated as -0.02 W/m2. The value should be -0.05 W/m2. [Mark Jacobson (Reviewer's comment ID #: 116-7)]	Accepted.
2-699	A	35:20		In Section 2.4.5.5 add a statement about the consequences for radiative forcing of the chemical interaction between sulfate and nitrate: Nitric acid will not form ammonium nitrate aerosol unless the sulfate is fully neutralized. This has two consequences for aerosol radiative forcing. First, radiative forcing by nitrate aerosol is often sensitive to emissions of ammonia as well as emissions of NO and other precursors of nitric acid. Emissions of ammonia are expected to increase [7.4.2.1]. Second, the radiative forcing from reductions in sulfate (e.g. by control of SO2 emissions) will be partially compensated in some regions by increases in nitrate (West et al., Marginal direct climate forcing by atmospheric aerosols Atmos. Environ., 1998; Liao and Seinfeld, 2005). [Govt. of United States of America (Reviewer's comment ID #: 2023-93)]	Accepted. Wording added accordingly.
2-700	A	35:21	35:22	To shorten the chapter delete "laboratory studies ... (e.g., Tang et al., 1995). If you don't want to take the space to explain the hygroscopicity of ammonium nitrate then don't mention at all. [Daniel Murphy (Reviewer's comment ID #: 183-19)]	Noted. We actually include another more up-to -date reference.
2-701	A	35:22	35:22	Many authors besides the cited work (Tang et al. 1995) have investigated nitrate aerosols. Change "(e.g., Tang et al., 1995)" to "(e.g., Tang et al., 1995; Martin, 2005, and refs therein)." (Martin, S.T., 2000: Phase Transitions of Aqueous Atmospheric Particles, Chem. Rev., 100, 3403-3453.) [Scot Martin (Reviewer's comment ID #: 168-6)]	Accpeted. Reference now included.
2-702	A	35:22	35:22	Some additional important comments on nitrate aerosols and their relation to sulfate are warranted. Before "In the AeroCom," insert "In addition, laboratory studies show that nitrate has an inhibition effect on the crystallization of sulfate aerosols (Martin et al. 2003). If crystallized, however, ammonium nitrate evaporates more readily than in its aqueous form, so the predicted RF by nitrate depends strongly on its physical state (Martin et al. 2004)." ((a) Martin, S.T., Schlenker, J.C., Malinowski, A., Hung, H.M., and Rudich, Y., 2000: "Crystallization of atmospheric sulfate-nitrate-ammonium particles," Geophys. Res. Lett., 30, 2102. (b) Martin et al. 2004 is listed in the references on apge 2-95.) [Scot Martin (Reviewer's comment ID #: 168-7)]	Rejected. This is too detailed for the very brief paragraph that we include on nitrate aerosol. The space constraints are strict.

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2-703	A	35:23		See comment #4. [Caroline Leck (Reviewer's comment ID #: 144-7)]	Rejected. Chemical species is a commonly used term.
2-704	A	35:31	36:44	In view of the sometimes highly variable absorbing properties of mineral dust will deposition on snow and ice affect the albedo and the resulting RF of the atmosphere over such "contaminated" areas? Saharan dust has different absorbing properties than Gobi Desert dust, and atmospheric transport may deposit dust on the Himalayas, Alps, etc. Often there is a mineral dust signature in ice cores. [Michel J. ROSSI (Reviewer's comment ID #: 220-10)]	Noted. The Ch2 of the IPCC 4AR assesses the current knowledge of the research. It does not postulate or comment on what future studies should be performed.
2-705	A	35:53	35:53	Regarding the best guess of 0-20% anthropogenic burden: The total atmospheric burden of mineral dust, natural and anthropogenic, is very important. Although we are interested mainly in RF or the anthropogenic fraction (0-20% "only") it is likely that perhaps a significant part of the natural dust aerosol interacts with black carbon or organic aerosol (of anthropogenic origin) which "amplifies" the absorbing properties of the natural portion of the mineral dust aerosol. Albeit a secondary effect, it could be significant regarding the large absolute burden of mineral dust even though the anthropogenic fraction of mineral dust may be low. I realize this may be a secondary effect but such non-linear effects may have to be taken into account in view of the large influence of the mixing state of atmospheric aerosol regarding RF. [Michel J. ROSSI (Reviewer's comment ID #: 220-9)]	Noted. The Ch2 of the IPCC 4AR assesses the current knowledge of the research. It does not postulate or comment on what future studies should be performed. Although this effect COULD be very important, there are only limited studies on this effect. Bauer and Koch, 2005, is referenced in this respect.
2-706	A	36:4		Add Uchiyama et al., 2005 in the list; "Asian mineral dust (Huebert et al., 2003:Clarke et al., 2004)". [Masao Mikami (Reviewer's comment ID #: 177-1)]	Rejected. The Uchiyama et al, 2005 reference does not show that the dust is considerably less absorbing. It uses an assumed refractive index. The additional information on the absorption properties and the single scattering Albedo are contained in the Mikami paper.
2-707	A	36:14	36:24	Based on the refractive indices measurements over China desert area, Wang et al (2004) estimated that a peak value of radiative forcing due to mineral dust for the spring mean of 2001 is up to 10 Wm-2 over China desert region and - 4Wm-2 over West Pacific Ocean at the top of atmosphere. Shi et al. (2005) performed further numerical sensitivity experiments to evaluate the impact of optical characteristics of mineral dust on its radiative forcing and indicated that the peak value of RF for a strong dust storm occurred during April 4 -15 in 2001 may reach up to 12 Wm-2 over China desert area and -12 Wm-2 over West Pacific Ocean. More important thing is that Shi et al. (2005) found a huge uncertainty in estimating the radiative forcing due to mineral dust comes from the different refractive indices, such as the WMO and ADEC data sets of dust, which are used	Noted. It is difficult to include these references in a coherent way here. This is because we are looking at the peak shortwave effect (which will occur over ocean, low Rs), and the peak longwave effect (which will occur over hot surfaces such as desert). Thus the values reported by Shi et al are not that relevant here. However, the Shi et al reference does

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				in the model (see their Figs.4 and 5).For reference, see "WANG Hong, SHI Guangyu, Aoki Teruo, WANG Biao?ZHAO Tianliang?Radiative forcing due to dust aerosol over east Asia-north Pacific region during spring, 2001, Chinese Science Bulletin 2004 Vol. 49, No. 20 2212—2219. SHI Guangyu, WANG Hong, WANG Biao?LI Wei, GONG Sunling,, ZHAO Tianliang, AOKI Teruo, Sensitivity experiments on the effects of optical properties of dust aerosols on their radiative forcing under clear sky condition, Journal of the Meteorological Society of Japan, 2005,Vol.83, pp.333-346." [Govt. of China (Reviewer's comment ID #: 2006-32)]	provide valuable insight into the dust absorption properties, and even determines a set of refractive indices in place of the WMO 'dust-like' refractive indices. Thus this refernce is included.
2-708	A	36:14	36:24	Add "In situ measurements of SSA by Particle Soot Absorption Photometer (PSAP) and Nephelometer during ADEC (Mikami et al., 2005) indicates that unpolluted Asian dust has lower absorption than originally believed and the dust particles mixed with absorbing aerosols during long range transport within the boundary layer (Uchiyama et al., 2005)." Add references; Mikami, M., G.-Y. Shi, I. Uno, S. Yabuki, Y. Iwasaka, M. Yasui, Te. Aoki, T.Y. Tanaka, Y. Kurosaki, K. Masuda, A. Uchiyama, A. Matsuki, T. Sakai, T. Takemi, M. Nakawo, N. Seino, M. Ishizuka, S. Satake, K. Fujita, Y. Hara, K. Kai, S. Kanayama, M. Hayashi, M. Du, Y. Kanai, Y. Yamada, X.-Y. Zhang, Z. Shen, H. Zhou, O. Abe, T. Nagai, Y. Tsutsumi, M. Chiba, and J. Suzuki, 2005: Aeolian Dust Experiment on Climate Impact: An Overview of Japan-China Joint Project ADEC, Global Planetary Change, doi:10.1016/j.gloplacha.2006.03.001, in press. Uchiyama, A., Yamazaki, A., Togawa, H., and Asano, J., 2005. Characteristics of Aeolian dust observed by sky-radiometer in the ADEC Intensive Observation Period 1(IOP1). J. Met. Soc. Japan. 83A, 291-305. [Govt. of Japan (Reviewer's comment ID #: 2014-32)]	Accepted in part. The wording has been changed slightly to reduce overall length and only the Mikami reference is now included. The Uchiyama et al reference is not included as it does not specifically deal wit the absorption and single scattering properties of mineral dust.
2-709	A	36:14	36:24	Based on the refractive indices measurements over China desert area, Wang et al (2004) estimated that a peak value of radiative forcing due to mineral dust for the spring mean of 2001 is up to 10 Wm-2 over China desert region and - 4Wm-2 over West Pacific Ocean at the top of atmosphere. Shi et al. (2005) performed further numerical sensitivity experiments to evaluate the impact of optical characteristics of mineral dust on its radiative forcing and indicated that the peak value of RF for a strong dust storm occurred during April 4 -15 in 2001 may reach up to 12 Wm-2 over China desert area and -12 Wm-2 over West Pacific Ocean. More important thing is that Shi et al. (2005) found a huge uncertainty in estimating the radiative forcing due to mineral dust comes from the different refractive indices, such as the WMO and ADEC data sets, of dust, which are used in the model (see their Figs.4 and 5). Related references WANG Hong, SHI Guangyu, Aoki Teruo, WANG Biao?ZHAO Tianliang?Radiative	Noted. It is difficult to include these refernces in a coherent way here. This is because we are looking at the peak shortwave effect (which will occur over ocean, low Rs), and the peak longwave effect (which will occur over hot surfaces such as desert). Thus the values reported by Shi et al are not that relevant here. However, the Shi et al reference does provide valuable insight into the dust absorption properties, and even determines a set of refractive indices in place of the

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				forcing due to dust aerosol over east Asia-north Pacific region during spring, 2001, Chinese Science Bulletin 2004 Vol. 49, No. 20 2212—2219. SHI Guangyu, WANG Hong, WANG Biao?LI Wei, GONG Sunling,, ZHAO Tianliang, AOKI Teruo, Sensitivity experiments on the effects of optical properties of dust aerosols on their radiative forcing under clear sky condition, Journal of the Meteorological Society of Japan, 2005,Vol.83, pp.333-346. [Guangyu Shi (Reviewer's comment ID #: 234-2)]	WMO 'dust-like' refractive indices. Thus this refernce is included.
2-710	A	36:26	36:30	This is a prime example of text cluttered with details. The formulation is very compact, and nearly unreadable. Is it really necessary to report every single study in what is supposed to be a synthesis report? [Govt. of Finland (Reviewer's comment ID #: 2009-18)]	Accepted, reworded. However information is needed to provide basis for assessment. A table is not included because of space constraints.
2-711	A	36:28	36:28	The direct forcing due to total anthropogenic and natural dust (shortwave/longwave/net TOA) from Jacobson (2001) should be -0.2/+0.07,-0.13 W/m2 (currently, the first two numbers are missing). [Mark Jacobson (Reviewer's comment ID #: 116-8)]	Accepted.
2-712	A	36:30	36:31	Please provide a number for 'higher … as described above', e.g. 'higher … as 0.96 at solar wavelengths' [Ina Tegen (Reviewer's comment ID #: 263-13)]	Accepetd in part. Reworded.
2-713	A	36:32		What does 'diversities' mean in this context? Is this variances? Or ranges? [Nathan Gillett (Reviewer's comment ID #: 84-58)]	Accepted. Reworded and clarified. Diversity was computed as single standard deviation in the papers mentioned.
2-714	A	36:32		Define what is meant by "diversities". [Govt. of United States of America (Reviewer's comment ID #: 2023-94)]	Accepted. Reworded and clarified. Diversity was computed as single standard deviation in the papers mentioned.
2-715	A	36:34	36:34	Tg (lower case) [Michel J. ROSSI (Reviewer's comment ID #: 220-7)]	Accepted.
2-716	A	36:35		reducing the burden of anthropogenic species in the accumulation mode'. What does this mean? 'Accumulation mode' has not been defined. [Nathan Gillett (Reviewer's comment ID #: 84-59)]	Accepted. Reworded.
2-717	A	36:38	36:44	This paragraph refers to the "anthropogenic RF" from dust radiative effects. In view of the definition of RF as the change since 1750, the word "anthropogenic" perhaps can be dropped. There may also be a radiative effect of dust changes that stem from circulation changes since 1750, but (see question above) has this been estimated? [Adrian Simmons (Reviewer's comment ID #: 242-30)]	Accepted in part. The dust RF based in 1750 could indeed be due to circulation changes. However, virtually nothing is known on the impact of such changes on dust

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					emissions. Some clarification was added.
2-718	A	36:46	46:2	The net total radiative aerosol forcing deduced inversely from observed temperatures needs mention here (see Forest et al, GRL, 2006 and other papers listed in chapter 9.2.1) [Ronald Prinn (Reviewer's comment ID #: 202-2)]	Noted. The format is that authors cannot make recommendations in this draft.
2-719	A	36:46		See comment #4. [Caroline Leck (Reviewer's comment ID #: 144-8)]	See response to comment #4.
2-720	A	36:54	36:56	"The role of non-linear processes of aerosol dynamics for RF has been recently studied in global aerosol models, which account for the internally mixed nature of aerosol particles (Kirkevag and Iversen, 2002; Liao and Seinfeld, 2005; Stier et al., 2005; Takemura et al., 2005)". This set of references is missing the following papers that examined the effect of the mixing state on direct forcing prior to the studies listed: Jacobson, M. Z., A physically-based treatment of elemental carbon optics: Implications for global direct forcing of aerosols, Geophys. Res. Lett., 27, 217-220, 2000. Jacobson, M. Z., Strong radiative heating due to the mixing state of black carbon in atmospheric aerosols, Nature, 409, 695-697, 2001. and the effect of nonlinear feedbacks aerosols of evolving mixing state on climate response: Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, J. Geophys. Res., 107, (D19), 4410, doi:10.1029/ 2001JD001376, 2002 Jacobson, M.Z., The climate response of fossil-fuel and biofuel soot, accounting for soot's feedback to snow and sea ice albedo and emissivity, J. Geophys. Res., 109, D21201, doi:10.1029/2004JD004945, 2004 [Mark Jacobson (Reviewer's comment ID #: 116-9)]	Noted. A reference is now made to another of Jacobson's multicomponent aerosol papers – the 2001, JGR paper. The non-linear feedback on climate response is beyond the scope of the radiative forcing chapter and belongs in chapter 7.
2-721	A	37:3	37:5	"Assuming external or internal mixing of black carbon in organic matter changes the associated RF from -0.78 to -0.48 W/m2 (see compilation in Chung and Seinfeld, 2002)." This range of direct forcing shown (-0.78 to -0.48 W/m2) does not account for all studies cited in Chung and Seinfeld (2002). In addition, the statement, as written, does not mention an additional mixing state, namely that of BC evolved between an external and internal mixture. The direct forcing due to BC from fossil fuels and biomass burning combined was estimated as 0.31 W/m2 for a pure external mixture, 0.62 W/m2 for a pure internal mixture, and 0.55 W/m2 for multiple distributions evolved between an external and internal mixture in Jacobson, M. Z., Strong radiative heating due to the mixing state of black carbon in	Taken into account. The statement has been written more clearly stating that the Chung and Seinfeld paper is a review and that the main finding is that studies show BC exetrts a stronger radiative forcing when mixed internally than externally.

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				atmospheric aerosols, Nature, 409, 695-697, 2001 [Mark Jacobson (Reviewer's comment ID #: 116-10)]	
2-722	A	37:7	37:8	Regarding the effect of heterogeneous reactions on dust and salt: what is the feedback mechanism on the fine mode sulfate? An additional sentence would clarify the situation. [Michel J. ROSSI (Reviewer's comment ID #: 220-8)]	Taken into account. Some new wording has been included.
2-723	A	37:31	37:32	I could not find these numbers (-0.2; +- 0.2) in the Table. Do you mean -0.21 +- 0.018? The rounding of numbers makes it difficult to locate the central estimate in Table. 2.6 [Claudia Marcolli (Reviewer's comment ID #: 158-14)]	Noted. It is standard practice statistically to present numbers rounded appropriately by the standard deviation. Otherwise there is a false impression of both accuracy and precision.
2-724	A	37:39	37:39	The type of measurements on which these estimates are based should be indicated. [Ina Tegen (Reviewer's comment ID #: 263-14)]	Accepted.
2-725	A	37:51	37:52	what do you mean by less different? Do you mean that the antropogenic fraction shows less variation? [Claudia Marcolli (Reviewer's comment ID #: 158-15)]	Taken into account. Reworded
2-726	A	37:53	37:55	Given the uncertainties, 21% and 29% do not seem to differ significantly from each other [Ina Tegen (Reviewer's comment ID #: 263-15)]	Taken into account. Reworded.
2-727	A	38:15	38:15	remove the first relative [Claudia Marcolli (Reviewer's comment ID #: 158-16)]	Accepted.
2-728	A	38:25	38:25	I suppose that "tau_aer" should be "the bias in simulated tau_aer" [Govt. of Finland (Reviewer's comment ID #: 2009-19)]	Taken into account. Reworded.
2-729	A	38:26	38:27	sentence structure: do you mean that "in the regions off Sourthern Africa the biomass burning aerosol above clouds leads to an overall heating?" [Claudia Marcolli (Reviewer's comment ID #: 158-17)]	Taken into account. Reworded.
2-730	A	38:38	38:38	"-2" should be in upper case [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-31)]	Accepted. OK..
2-731	A	38:38		"-2" superscript [Junying Sun (Reviewer's comment ID #: 261-19)]	Accepted. OK..
2-732	A	38:39		minimum uncertainty' - what does this mean? [Nathan Gillett (Reviewer's comment ID #: 84-60)]	Accepted. Reworded.
2-733	A	38:40	38:40	Minus sign missing in front of "2" [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-32)]	Accepted. OK.
2-734	A	38:40		m2" to "m-2 [Junying Sun (Reviewer's comment ID #: 261-20)]	Accepted. OK..

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2-735	A	38:42	38:45	This assessment of errors seems somewhat arbitrary. Is the 0.4 W m-2 uncertainty a 1 sigma range? [Nathan Gillett (Reviewer's comment ID #: 84-61)]	Accepted. Throughout the chapter we are more quantitative
2-736	A	38:48	38:48	an extra "are" [Govt. of Finland (Reviewer's comment ID #: 2009-20)]	Accepted. OK.
2-737	A	38:48	38:48	remove the are after results [Claudia Marcolli (Reviewer's comment ID #: 158-18)]	Accepted. OK.
2-738	A	38:52	38:52	The term "Cloud-Aerosol Interaction" is quite broad and normally both refers to how clouds influence aerosols and how aerosols influence clouds. Here, it is almost exclusively the latter interaction which is treated, even though this is not explicitly stated. This has to be made clearer. I suggest to change the title of the subsection to 'Aerosol influence on clouds' or something like that. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-33)]	OK, accepted and title changed.
2-739	A	38:52		With cloud-aerosol interaction, I respect that a lot of good research has been done with the albedo effect. It may be nice to put in a special section outlining the potential importance of the lifetime effect. A flag here can be very powerful in directing research. And it is appropriate not to leave the whole topic to chapter 7. [Steven Siems (Reviewer's comment ID #: 239-5)]	Noted. This has been considered before, and we are balancing the theme with additional text in chapter 7.
2-740	A	39:0		A recent study by Lohmann, U., and C. Leck, 2005, Importance of submicrone surface active organic aerosols for pristine Arctic Clouds, Tellus 57B, 261-268, shows the complexity and is recommended to be sited. [Caroline Leck (Reviewer's comment ID #: 144-19)]	Rejected. The suggested paper do not explicitly deals with a process influencing global indirect forcing.
2-741	A	39:3	39:3	The aerosol enhancements "have also been hypothesised" to lead to an increase in the lifetime of the cloud. I suggest using "have also been hypothesised" instead of "can". There is almost no observational evidence of aerosol effects on cloud lifetime and models that actually resolve clouds show that aerosol has little effect on the lifetime of warm cumulus clouds (Jiang et al, Aerosol effects on the lifetime of shallow cumulus, GRL 2006, In press, available at www.etl.noaa.gov/~gfeingold) [Graham Feingold (Reviewer's comment ID #: 69-1)]	Accepted. Very good suggestion.
2-742	A	39:3	39:4	albedo effcet", "lifetime" effect, the format should be same such as "albedo effect", "lifetime effect [Junying Sun (Reviewer's comment ID #: 261-21)]	Accepted
2-743	A	39:13	39:13	Before "Cloud", insert "In addition, laboratory studies suggest that gas-phase oxidants such as O3 or OH continue to age organic aerosols and affect their CCN activity (Kanakidou et al. 2005), thus establishing additional feedbacks, e.g., tropospheric ozone production could affect cloud formation at some times and locations." (Kanakidou et al. 2005 is listed in the references on page 2-90.)	Rejected. It is possible that tropospherci ozone production could affect cloud formation at some times and locations, but there is no paper with clear evidence that this could be

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				[Scot Martin (Reviewer's comment ID #: 168-8)]	of global relevance. Feedbacks are chapter 7 issue
2-744	A	39:27	39:28	Explain the evidence concerning potential aerosol modification of clouds from shiptrack observations. Otherwise a reader who is not familiar with the TAR will gain nothing from this discussion. [Nathan Gillett (Reviewer's comment ID #: 84-62)]	Rejected: Already done in earlier IPCC report.
2-745	A	39:27		I believe the second paragraph (line 42) is a much better introduction to section 2.4.6.1 Indeed, work with clouds and aerosol particles go back much further to the cloud-seeding/weather modification work in the 1940s. While this is NOT a popular example to mention, it does give a much more fair representation of the time-line for cloud physics. [Steven Siems (Reviewer's comment ID #: 239-2)]	Accepted. Text is being rephrased.
2-746	A	39:29	39:40	Move text beginning Feigold et al. (2003) to pg 2-40, line 54 [Graham Feingold (Reviewer's comment ID #: 69-2)]	Accepted
2-747	A	39:36	39:36	What is CCN? [European Commission (Reviewer's comment ID #: 2008-16)]	Defined before.
2-748	A	39:36	39:36	Please provide full name for acronym "CCN" at first usage. [Govt. of Germany (Reviewer's comment ID #: 2011-108)]	Accepted
2-749	A	39:36	39:36	It should read Feingold et al. [Thomas Peter (Reviewer's comment ID #: 198-38)]	Accepted
2-750	A	39:37	39:40	the sentence structure has to be worked out [Claudia Marcolli (Reviewer's comment ID #: 158-19)]	Accepted
2-751	A	39:45	39:45	have led to' should be 'has led to' [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-34)]	Accepted
2-752	A	39:50	39:50	change "not completely certain" to "uncertain" [Graham Feingold (Reviewer's comment ID #: 69-3)]	Accepted
2-753	A	39:52		Another and probably better reference than Cziczo et al. (2004a) is Figure 8 in the McFiggans et al. review paper. There is also a Murphy et al. submitted but you probably can't reference that. I put a figure in McFiggans et al. partly so that it would be out in time for the IPCC :). [Daniel Murphy (Reviewer's comment ID #: 183-20)]	Accepted
2-754	A	39:54	39:54	write water soluble instead of just soluble [Claudia Marcolli (Reviewer's comment ID #: 158-20)]	Accepted
2-755	A	39:57	39:57	lead to "a significant increase". This is important because the significant increases are only obtained for equilibrium models and in the absence of other composition factors that tend to counter the surface tension effect.	Accepted

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				[Graham Feingold (Reviewer's comment ID #: 69-4)]	
2-756	A	40:1		Lohmann and Leck (see comment#19) should be added to the list of references. [Caroline Leck (Reviewer's comment ID #: 144-20)]	Rejected. We can not cite all the papers published in each area in the last few years. We selected the ones that we think are the most relevant, even if others have also dealt with similar subject. Please note this is an assessment, not a survey.
2-757	A	40:3	40:3	leading to a reduction in drop number and a broadening of the cloud droplet spectrum, which had not been… [Graham Feingold (Reviewer's comment ID #: 69-5)]	Accepted
2-758	A	40:14	40:34	It is my sense that one of the nicest studies exploring the relationships among clouds structure and the ambient aerosol is that by Twohy et al., 2003. The strength of this study is that it explores aerosol effects over a siginficant range of values in a remarkably consistent dynamical environment and over length and timescales appropriate for large-scale models. From the perspective of the modeling studies the report misses what I think are important early studies to look at these effects, namely the series of Stevens et al., (1996, 1998) and Duda et al., (1999). The Stevens (1998) paper is the first to explore in a dynamically consistent way how the formation of precipitation leads to more broken clouds, less LWP and greater variance. These findings have recently been garnering observational support, i.e., vanZanten et al., 2005a,b and Stevens et al., 2005. But are counter to the later studies, in part because some of the later studies focus on shallow cumulus (whose dynamics are fundamentally different than stratocumulus, e.g., Jiang and Feingold, Xue and Feingold), but also because it depends on how hard you hit it, and the environmental conditions underwhich precipitation takes place (i.e., the Ackerman et al., 2004 study, loc cit.). As it stands now this discussion of the finescale modeling mis-represents the state of our understanding. As regards devising observational studies that can address these situations, that was, in part, the basis of the DYCOMS-II experiment (Stevens et al., 2003), which along with the ACE-II measurements of Brenguier and colleagues provides the clearest basis yet for evaluating aerosol effects on stratocumulus and the basis for using large-eddy simulations to explore these issues. As it currently stands the report seems to better represent the failures, and thus misrepresents the state of the field. References: Twohy, Cynthia H., Markus D. Petters, Jefferson R. Snider, Bjorn Stevens, William Tahnk, Melanie Wetzel, Lynn Russell, Frederic Burnet: Evaluation of the Aerosol Indirect	Accepted partially. We are incluiding some of suggested references, and accepted part of the suggested changes in the revised text.

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				Effect in Marine Stratocumulus Clouds: Droplet Number, Size, Liquid Water Path and Radiative Impact, 2005: J. Geophys. Res. 110, D08203 Duda, D.P., G.L. Stephens, B.B. Stevens, and W.R. Cotton, 1996: Effects of Aerosol on the Broadband Albedo of Marine Stratus: Numerical Simulations. J. Atmos. Sci., 53, 3757-3769. Stevens, Bjorn, Graham Feingold, William R. Cotton and Robert L. Walko. 1996: Elements of the Microphysical Structure of Numerically Simulated Nonprecipitating Stratocumulus. J. Atmos. Sci.,, 53, No. 7, pp. 980–1006. Stevens, Bjorn, William R. Cotton, Graham Feingold and C.-H. Moeng, 1998: Large-Eddy Simulations of Strongly Precipitating, Shallow Stratocumulus-Topped Boundary Layers J. Atmos. Sci., 55, 3616-3638. Stevens, Bjorn, et al., 2003: Dynamics and Chemistry of Marine Stratocumulus -- DYCOMS-II, 2003: Bull. Amer. Meteorol. Soc., 84, 579-593. Stevens, Bjorn, Gabor Vali, Kimberly Comstock, Margreet C. van Zanten, Philip H. Austin, Christopher S. Bretherton and Donald H. Lenschow, 2005: Pockets of Open Cells (POCs) and Drizzle in Marine Stratocumulus Bull. Amer. Meteorol. Soc., 86, 51-57 VanZanten, Margreet C., and Bjorn Stevens, 2005: Observations of the structure of heavily precipitating marine stratocumulus. J. Atmos. Sci., 62, 4327–4342 vanZanten, M.C., B. Stevens, G. Vali and D. Lenschow 2005: Observations of Drizzle in Nocturnal Marine Stratocumulus. J. Atmos. Sci. 62, 88-106 [Bjorn Stevens (Reviewer's comment ID #: 254-5)]	
2-759	A	40:19	40:21	This is not well written. For instance, it goes without saying that "Observations … indicate … an increase in shortwave reflectance of low-level warm cloud with increasing cloud optical thickness", so this should be skipped. It is not clear what is meant by ".... while the LWP showed insignificant changes". With respect to what? The whole sentence needs to be reconsidered and rephrased. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-35)]	Accepted. Text changed.
2-760	A	40:20		Define LWP. This is currently not defined. I guess that this is 'liquid water path', but this isn't obvious. [Nathan Gillett (Reviewer's comment ID #: 84-63)]	Accepted
2-761	A	40:21		LWP" to "Liquid water path (LWP) [Junying Sun (Reviewer's comment ID #: 261-22)]	Accepted
2-762	A	40:24	40:31	Note that Ackerman (Ackerman, A.S., M.P. Kirkpatrick, D.E. Stevens and O.B. Toon, 2004: The impact of humidity above stratiform clouds on indirect aerosol climate forcing, Nature, 432, 1014-1017) determined that the response of the liquid water path to increasing aerosols depends on the precipitation rate and relative humidity above the clouds.	Noted. The response of the liquid water path is also discussed in several of the papers from Feingold discussed indetails in this chapter.

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				[Joyce Penner (Reviewer's comment ID #: 197-15)]	
2-763	A	40:30	40:31	Xue and Feingold (2006) and Jiang and Feingold (2006). Full references: Xue, H., and G. Feingold, 2006: Large eddy simulations of trade-wind cumuli: Investigation of aerosol indirect effects. J. Atmos. Sci., 63, No. 6, 1605-1622. Jiang, H., and G. Feingold, 2006: Effect of aerosol on warm convective clouds: Aerosol-cloud-surface flux feedbacks in a new coupled large eddy model. J. Geophys. Res., 111, D01202, doi:10.1029/2005JD006138. [Graham Feingold (Reviewer's comment ID #: 69-6)]	Accepted.
2-764	A	40:48	40:48	"Fewer number of studies" should be 'Fewer studies' [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-36)]	Accepted
2-765	A	40:52	40:52	the brightening" should be "a brightening [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-37)]	Accepted
2-766	A	40:55	41:8	This paragraph could do a better job in providing a balanced picture of ice nucleation and its role in the climate system. It should probably say, that there is ample of evidence for ice particles far below the level at which homogeneous nucleation may occur, clarifying the importance of IN at low altitudes. Conversely, there is evidence for very high number densities in cirrus clouds above ~ 6 km, pointing to the importance of homogeneous nucleation in combination with high cooling rates in small-scale temperature fluctuations. Important steps since TAR are that (1) our understanding of homogeneous ice nucleation from all kinds of solution has made large advances, and (2) there have been several important papers revealing the importance of homogeneous nucleation at these altitudes. I do not deny that inbetween there are the mixed-phase clouds that are very important, but the others should not be forgotten. One could also discuss whether the two Diehl papers represent a meaningful selection here (also, Lohmann should be spelled with 2 L). [Thomas Peter (Reviewer's comment ID #: 198-39)]	Noted. This text was sent to chapter 7 authors to eventually be included in their text.
2-767	A	40:57	40:57	beginning to "be" addressed… [Graham Feingold (Reviewer's comment ID #: 69-7)]	Accepted
2-768	A	40:57	40:57	aerosol-crystal" should be "aerosol-ice crystal [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-38)]	Accepted
2-769	A	41:2	41:3	Delete "laboratory" as the Cziczo et al. data are from a mountaintop site. A more concise wording is "Cziczo et al. (2004b) measured an unequal partition of organic compounds to the ice phase, with organic-rich particles remaining unfrozen and ..." [Daniel Murphy (Reviewer's comment ID #: 183-21)]	Accepted
2-770	A	41:7	41:8	Diehl and Wurzler 2005 and Lohman and Diehl 2006 are missing from the reference list [Govt. of Finland (Reviewer's comment ID #: 2009-21)]	Accepted
2-771	A	41:10	41:16	Add "Raman lidar measurements revealed a connection between transported Asian	Rejected. This chapter deals mostly

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				aerosols and icy dust clouds (Sakai et al., 2004, Sassen, 2005). These findings suggest that an indirect effect of desert dust on cloud formation and composition may be considerable although quantification of a RF from this mechanism is not achieved at present." Add references; Sakai, T., Nagai, T., Nakazato, M., and Matsumura, T., 2004. Raman lidar measurement of water vapor and ice clouds associated with Asian dust layer over Tsukuba, Japan. Geophys. Res. Lett. 31, L06128, doi:10.1029/2003GL019332. Sassen, K., 2005. Dusty ice clouds over Alaska, Nature, 434, 456. [Govt. of Japan (Reviewer's comment ID #: 2014-33)]	with the forcing, and this paper does not deal explicitly with RF, and indeed mentions that a quantification in not achievable at present.
2-772	A	41:14	41:14	Why "even"? [Thomas Peter (Reviewer's comment ID #: 198-40)]	Text revised.
2-773	A	41:16		Add "Raman lidar measurements revealed a connection between transported Asian aerosols and icy dust clouds (Sakai et al., 2004, Sassen, 2005). These findings suggest that an indirect effect of desert dust on cloud formation and composition may be considerable although quantification of a RF from this mechanism is not achieved at present." [Masao Mikami (Reviewer's comment ID #: 177-3)]	Rejected. Please see the answer to comment number 2-771.
2-774	A	41:18	41:23	This synopsis of the Sherwood paper is misleading. It implies that the HALOE observations in the lower stratosphere show a negative correlation between ice crystal size and relative humidity. In fact the effective diameter observations were for the upper troposphere from AVHRR and so were not coincident in either altitude or location with the HALOE RH data. Rather than expand this section to explain the details, I would suggest deleting this paragraph. This Sherwood paper is already nicely cited on page 2-24 line 22 in the section on changes in stratospheric water vapour. I think that is the more relevant place to cite it. [Daniel Murphy (Reviewer's comment ID #: 183-22)]	Accepted.
2-775	A	41:18		No offense to Steve Sherwood, but this material belongs in Chapter 7 with the discussion on cloud lifetime and related material. [Steven Siems (Reviewer's comment ID #: 239-3)]	Accepted
2-776	A	41:20	41:23	This is a research recommendation, which is beyond the scope of the IPCC reports. [Nathan Gillett (Reviewer's comment ID #: 84-64)]	Accepted. Taken into account.
2-777	A	41:20	41:20	I personally think that the Sherwood paper should not get such an uncritical publicity in this place. First, the correlation between relative humidities and effective radii, both derived from satellite data, is highly speculative (as anybody doing in-situ measurements will immediately agree with). Second, the conntion with biomass burning, presented on the last 10 lines of this paper, is even more speculative, to say the least. Third, Sherwood	Accepted. Reference omitted.

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				did not quantify any trends. Therefore, this citation needs either a sound critical treatment (as done in many other places in this report) or be omitted (which might be preferable). [Thomas Peter (Reviewer's comment ID #: 198-41)]	
2-778	A	41:22	41:22	Regarding the faster evaporation of small ice crystals: if the authors think of the Kelvin effect, I think that ice crystals are not that small that the Kelvin effect needs to be taken into account. On the contrary, I would expect that due to increasing concentration of solutes or trace gases the rate of evaporation will significantly decrease as seen in laboratory studies by Delval, 2004. [Michel J. ROSSI (Reviewer's comment ID #: 220-11)]	Noted.
2-779	A	41:25	43:52	I do not understand why observational and satellite data (despite their uncertainties) along with data-constrained modeling are not used to "temper" the -0.84 +- 0.52 W/m2 GCM modeling result for the ICAE's RF values. Considering the statements on these pages of Ch. 2, most scientific readers would surely say that a lesser maximum negative RF for the ICAE should be stated. With only limited time for my review, I cannot argue for any one specific better maximum -RF. Yet, a "tempering" by about 0.2-0.3 W/m2 seems in order. I.e., the maximum -RF value for the ICAE might better be stated as -1.1 W/m2 or -1.2 W/m2, once data are considered. [Herman Sievering (Reviewer's comment ID #: 240-7)]	Noted.
2-780	A	41:25	43:52	That GCM modeling alone was used to state a minimum -RF for the ICAE (-0.4 W/m2) is problematic. There are many assumptions in these 8 GCM models w.r.t. aerosol characteristics (incl. size distribution, chemical constituents, air-sea & air-land exchange, etc.) that may, despite decades of high quality aerosol research, still be invalid. Some heterogeneous chemical conversion mechanisms that may reduce ICAE's minimum -RF have not been considered in most, perhaps all 8, of these GCMs (e.g., sulfur gas conversion in sea-salt aerosols). Modeled aerosol parameterizations, or lack thereof, may lead to modeling results that differ from eventual data-constrained model results (once crucial data are available; not, yet, the case). Surely, the direct observational and verified satellite data, especially if used in data-constrained models, must be accounted for in establishing the ICAE low- and high-bound W/m2 values. The example of aerosol chemical modeling/mechanism uncertainty mentioned in comment #9 indicates it may be prudent to have zero (or very nearly zero) as the stated minimum RF value for the ICAE. [Herman Sievering (Reviewer's comment ID #: 240-8)]	Noted. The studies included in the revised version were extended.
2-781	A	41:25	46:2	At the opening of 2.4.6.2, I would really recommend expressing caveats about the ability of GCMs to represent clouds (convection) and cloud processes. This is expressed to some extent in 2.4.6.5 but I felt as if this should have come at the beginning. Both McFiggans et al. (2005) and the International Aerosol Precipitation Science Assessment Group (IAPSAG, 2006) discuss this at some length and the wording from those documents may	Accepted

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				be useful (E.g., opening paragraph of section 7, McFiggans et al.). It is important to have these caveats in mind while reading about the new evidence. [Graham Feingold (Reviewer's comment ID #: 69-8)]	
2-782	A	41:26	41:50	On pg 2-39, the albedo effect was linked to Twomey and the "constant LWC" was mentioned. It must be pointed out that when GCMs assess the albedo effect, the constant LWC criterion is relaxed. [Graham Feingold (Reviewer's comment ID #: 69-9)]	Accepted
2-783	A	41:26	45:37	The uncertainty related to our ability fo formulate cloud processes, which is nicely discussed in 2-44 lines 37-46, could usefully be incorporated in our discussion of the observations as well. Why? Our ability to seaparate meteorological from chemical influences on cloudiness in satellite observations depends on an understanding of how clouds respond to changes in the meteorology. Our current lack of sophistication in these respects hinders observational as well as modeling efforts. This point deserves emphasis, ideally by dealing with in a more comprehensive way throughout these sections. The unity of the document would also be enhanced if in these respects connections were made to chapter 8, which currently states that "cloud feedbacks remain the largest source of uncertainty in climate sensitivity estimates," and chapter 7, which in so far as I can deduce, did not address this point at all. [Bjorn Stevens (Reviewer's comment ID #: 254-6)]	Accepted. Suggestion incorporated in the text.
2-784	A	41:48	41:49	give references for the case studies: do you mean model sensitivity studies or field studies? [Claudia Marcolli (Reviewer's comment ID #: 158-21)]	Noted. Taken into account..
2-785	A	42:12	42:12	The words "resulting in the smallest estimate of the cloud albedo indirect effect" are not true for the Chuang et al. (2002) study. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-39)]	Noted. Taken into account..
2-786	A	42:12	42:12	resulting in the smaller (I.e. most negative) estimate of the first indirect effect …. [Joyce Penner (Reviewer's comment ID #: 197-16)]	Accepted
2-787	A	42:14	42:17	Note that Chuang et al. (2002) and Lohmann et al. (1999) also consider changes aerosol size and chemical properties. Chen and Penner (2005) (Chen, Y. and J.E. Penner, 2005: Uncertainty analysis for estimates of the first indirect effect, Atmos. Chem. Phys., 5, 2935-2948, SRef-ID: 1680-7324/acp/2005-5-2935.) compared the parameterization Chuang et al. and Lhopmann et al. used (which was based on the Chuang and Penner 1995 parameterization) to that used by Takemura (which is based on the Ghan et al. 2002 parameterization) and found a forcing of -1.79 W/m2 compared to the base case (based on the Ghan et al. 2002 parameterization) forcing of -1.3 W/m2. Also, the Takemura model has much smaller 1st indirect forcing than the LMDZ model or the CAM-Oslo model even thought the CAM-Oslo model uses the same parameterization for droplet	Noted. Reference added and text changed according suggestion.

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				formation as the Takemura model. This is because the Takemura model has a larger liquid water path than does the CAM-Oslo model (See Penner et al., 2006) (Penner., J.E., J. Quaas, T. Storelvmo, T. Takemura, O. Boucher, H. Guo, A. Kirkevåg, J.E. Kristjánsson, and Ø. Seland, 2006: Model intercomparison of indirect aerosol effects, Atmos. Chem. Physics Discussions, 1579-1617, Sref-ID: 1680-7375/acpd/2006-6-1579.) [Joyce Penner (Reviewer's comment ID #: 197-17)]	
2-788	A	42:16	42:16	It is not clear what "which depends …." refers to. The sentence needs to be restructured. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-40)]	Accepted. Text changed.
2-789	A	42:29	42:29	Insert "two" before "standard" [VINCENT GRAY (Reviewer's comment ID #: 88-251)]	Accepted. Text changed.
2-790	A	42:30	42:30	Replace "deviation" with "deviations" [VINCENT GRAY (Reviewer's comment ID #: 88-252)]	Accepted
2-791	A	42:30	42:30	Double the confidence limits to two standard deviations:to "-1.37 ± 0.28" [VINCENT GRAY (Reviewer's comment ID #: 88-253)]	Noted.
2-792	A	42:30	42:30	replace "btoom" by "bottom" [Michel J. ROSSI (Reviewer's comment ID #: 220-15)]	Accepted
2-793	A	42:33		The sentence "Note however…" needs a further explanation. This level of identification begs the question why? And suggests a problem with the code. This either needs to be further explained or omitted to be consistent with the rest of the discussion. [Steven Siems (Reviewer's comment ID #: 239-4)]	Noted.
2-794	A	42:34	42:34	Double the confidence limits to two standard deviations:to "-0.64 ± 0.32" [VINCENT GRAY (Reviewer's comment ID #: 88-254)]	Noted.
2-795	A	42:34	42:35	Note that the Chen and Penner (2005) best estimate is -1.3 W/m2, but the range is -0.45 to 2.16 W/m2 depending on which different parameterizations are used. This study has the advantage of knowing what causes the different forcings estimates. (Chen, Y. and J.E. Penner, 2005: Uncertainty analysis for estimates of the first indirect effect, Atmos. Chem. Phys., 5, 2935-2948, SRef-ID: 1680-7324/acp/2005-5-2935.) Also, based on the Penner et al. (2006) study, the causes of some of the range is associated with differences in liquid water path in the models. (Penner., J.E., J. Quaas, T. Storelvmo, T. Takemura, O. Boucher, H. Guo, A. Kirkevåg, J.E. Kristjánsson, and Ø. Seland, 2006: Model intercomparison of indirect aerosol effects, Atmos. Chem. Physics Discussions, 1579-1617, Sref-ID: 1680-7375/acpd/2006-6-1579.) [Joyce Penner (Reviewer's comment ID #: 197-18)]	Accepted. New estimates included in the list of results for teh global indirect effect.
2-796	A	42:44	42:44	Insert "that" between "here" and "have" [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-41)]	Accepted
2-797	A	42:47	43:52	I found this section too technical and hard to understand. It should be made more	Accepted. Text was revised to make

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				intelligible to the non-specialist. [Nathan Gillett (Reviewer's comment ID #: 84-65)]	reading easier.
2-798	A	42:47	44:19	I have a concern about the observational constraints on GCM simulations of aerosol-cloud interactions. I am supportive of this approach, provided the observations are robust. However, the evidence of the aerosol effect on clouds from satellite observations (MODIS, POLDER) indicates slopes of drop size vs aerosol that are much smaller than predicted by theory. It has been shown (Feingold 2003, GRL) that this is at least partly due to the use of aerosol optical depth as a proxy for CCN. It is also likely due to biases in drop size and cloud optical depth retrievals in broken clouds (e.g., Marshak et al. JGR 2006), relative humidity effects (Kapustin et al. JGR 2006) and so called "cloud contamination" which results from the poor distinction betweeen hydrated aerosol and cloud. The adiabatic assumption used by Quaas et al. (2005) will also be subject to error. Brenguier and coworkers used this approach successfully, but they selected adiabatic cases carefully. The satellite results that I am familiar with suggest to me that it is far too premature to use them as constraints to GCMs. Again, this is briefly mentioned on pg 44, lines 11-19 but I feel that this should come at the beginning. This is important because the smaller (negative) RF values seem to come from these constrained models and I am not confident in the observational constraints. As an aside, I am not confident at this stage that surface-based remote sensing is up to the task either. I would be more comfortable constraining GCM representation of aerosol-cloud interactions with in-situ observations. [Graham Feingold (Reviewer's comment ID #: 69-10)]	Accepted. Text changed to call attention to this point.
2-799	A	42:47		The materials in Ch7 (P67, line 1-20) could be more suitable to put in Ch2 (§2.4.6.3) [Govt. of China (Reviewer's comment ID #: 2006-34)]	Rejected. We carefully divided critical issues on aerosol-cloud interactions between chapter 7 and chapter 2. Most of the forcing-relevant issues are dealt here in chapter 2.
2-800	A	42:57		nonlinear relationship' - between what and what? [Nathan Gillett (Reviewer's comment ID #: 84-66)]	Accepted. Text revised.
2-801	A	43:1		What is plotted against what on a log-log graph? [Nathan Gillett (Reviewer's comment ID #: 84-67)]	Accepted. Text revised.
2-802	A	43:2		What are the units here? [Nathan Gillett (Reviewer's comment ID #: 84-68)]	Noted.
2-803	A	43:7		more susceptible' - to what? [Nathan Gillett (Reviewer's comment ID #: 84-69)]	Accepted.
2-804	A	43:9	43:11	I didn't understand this. How can there be indirect radiative forcing in preindustrial simulations? Surely the preindustrial simulation is the control against which radiative forcing changes are assessed?	Accepted. Text changed.

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				[Nathan Gillett (Reviewer's comment ID #: 84-70)]	
2-805	A	43:9	43:11	In the Executive Summary on page 5 "indirect effect" refers to the radiative forcing, i.e., the change (mainly due to human activity) over the last 250 years. Here, on the other hand, it appears that "indirect effects" refers to any influence of aerosols on clouds. This is confusing. I suggest replacing "indirect effects" by "aerosol-cloud interaction". [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-43)]	Noted.
2-806	A	43:10	43:10	It is not clear what "which is" refers to. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-42)]	Noted.
2-807	A	43:18	43:18	Double the confidence limits to two standard deviations:to "--0.64 ± 0.32" [VINCENT GRAY (Reviewer's comment ID #: 88-255)]	Rejected. This value was obtained from the paper itself, and represent the author's measurement and standard deviation.
2-808	A	43:19	43:19	Double the confidence limits to two standard deviations:to "--0.37 ± 0.18" [VINCENT GRAY (Reviewer's comment ID #: 88-256)]	Rejected. This value was obtained from the paper itself, and represent the author's measurement and standard deviation.
2-809	A	43:27	43:28	There is a mismatch between "assumptions" and "the fact". A fact is not an assumption. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-44)]	Noted.
2-810	A	43:27	43:30	Note that Ghan S. J., et al. (2006), (Use of in situ cloud condensation nuclei, extinction, and aerosol size distribution measurements to test a method for retrieving cloud condensation nuclei profiles from surface measurements, J. Geophys. Res., 111, D05S10, doi:10.1029/2004JD005752.) examined this issue and found good agreement if the supersaturation was small (as in stratiform clouds). [Joyce Penner (Reviewer's comment ID #: 197-19)]	Accepted. Reference added.
2-811	A	43:29		Define what is meant by "subcloud" or use a better phrase, i.e., "below the cloud". [Govt. of United States of America (Reviewer's comment ID #: 2023-95)]	Noted.
2-812	A	43:45	43:45	Reference to Dufresne et al. (2005) is missing [Graham Feingold (Reviewer's comment ID #: 69-11)]	Accepted
2-813	A	43:50	43:50	depend" should be "depends [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-45)]	Accepted
2-814	A	43:51	43:51	model" should be "models [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-46)]	Accepted
2-815	A	44:5	44:5	In a GCM, the aerosol indirect effect *must* be resolution dependent because the representation of clouds is resolution-dependent, and the updraught velocity which affects droplet activation is resolution-dependent.. [Graham Feingold (Reviewer's comment ID #: 69-12)]	Accepted

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2-816	A	44:5		Replace "of the aerosol indirect effect" with "of the derived aerosol indirect effect". The current sentence implies that the actual aerosol indirect effect is dependent upon the spatial resolution of models and observations. [Govt. of United States of America (Reviewer's comment ID #: 2023-96)]	Noted.
2-817	A	44:11	44:13	This discussion essentially repeats lines 27-30 on p. 43. [Govt. of Finland (Reviewer's comment ID #: 2009-22)]	Noted.
2-818	A	44:15	44:15	The Rosenfeld and Feinglod (2003) reference is missing in the reference list (page 103). [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-47)]	Accepted.
2-819	A	44:21	44:21	I would change "biases" to "limitations", expand on this and place before discussion of results, as mentioned above. [Graham Feingold (Reviewer's comment ID #: 69-13)]	Accepted.
2-820	A	44:22	44:23	"One of the large sources of uncertainties is the poor knowledge of the amount and distribution of anthropogenic aerosols that are used in the model simulations". This sounds as if one would not know what one is using. What is meant is that one does not know what one should use. [Thomas Peter (Reviewer's comment ID #: 198-42)]	Accepted. Text modified.
2-821	A	44:44	44:46	Note that Penner et al (2006) find large differences in modeled liquid water path for different models (Penner., J.E., J. Quaas, T. Storelvmo, T. Takemura, O. Boucher, H. Guo, A. Kirkevåg, J.E. Kristjánsson, and Ø. Seland, 2006: Model intercomparison of indirect aerosol effects, Atmos. Chem. Physics Discussions, 1579-1617, Sref-ID: 1680-7375/acpd/2006-6-1579.) [Joyce Penner (Reviewer's comment ID #: 197-20)]	Noted.
2-822	A	45:4		Add state of mixture and morphology/shape of individual particles resolved over size cf. Comment 13, 14 and 16. [Caroline Leck (Reviewer's comment ID #: 144-21)]	Accepted.
2-823	A	45:6	45:6	Reference to Feingold (2003) is missing. Full reference is: Feingold, G., 2003: Modeling of the first indirect effect: Analysis of measurement requirements. Geophys. Res. Lett., 30, No. 19, 1997, doi:10.1029/2003GL017967. At this point, also add reference to Ervens et al. (2005): Ervens. B., G. Feingold, and S. M. Kreidenweis, The influence of water-soluble organic carbon on cloud drop number concentration. J. Geophys. Res., 110, D18211, doi:10.1029/2004JD005634. Those authors showed that when kinetic limitations to droplet growth, and multiple composition effects are considered together, the effect of composition on droplet concentration is much smaller than implied by equilibrium calculations for a single effect such as surface tension. [Graham Feingold (Reviewer's comment ID #: 69-14)]	Accepted.
2-824	A	45:8		Objection to that several compounds are typically internally mixed. A new perspective on	Rejected. The phrase " several

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				state of mixture and morphology/shape has been derived from a number of recent published studies using Transition Electron Microscopicity (TEM)- a fundamental compliment to the results derived by the aerosol mass spectrometers. Relevant papers are : Leck, C., and E.K. Bigg, 1999, Aerosol production over remote marine areas - A new route, Geophys. Res. Lett., 23, 3577-3581. Bigg, E.K., and C. Leck, 2001, Properties of the aerosol over the central Arctic Ocean, J. Geophys. Res., 106 (D23), 32,101-32,109. Leck, C., M. Norman, E.K. Bigg, and R. Hillamo, 2002, Chemical composition and sources of the high Arctic aerosol relevant for fog and cloud formation, J. Geophys. Res., 10, dio:10.1029/2001JD001463. Leck, C., and E.K. Bigg, 2005a, Biogenic particles in the surface microlayer and overlaying atmosphere in the central Arctic Ocean during summer, Tellus 57B, 305-316. Leck, C., and E.K. Bigg, 2005b, Evolution of the marine aerosol – A new perspective, Geophys. Res. Lett., 32, L19803, doi:10.1029/2005GL023651. Lohmann, U., and C. Leck, 2005, Importance of submicrone surface active organic aerosols for pristine Arctic Clouds, Tellus 57B, 261-268. Pósfai, M., Li, J., Anderson, J.R. and Buseck, P.R. 2003. Aerosol bacteria over the Southern Ocean during ACE-1. Atmos., Res. 66, 231-240. [Caroline Leck (Reviewer's comment ID #: 144-22)]	compounds are typically internally mixed" is based on many studies that shows that after the aerosols are suspended in the atmosphere for a few hours or days, and after being processed incloud droplets, they mostly are internally mixed. This of course does not exclude the fact that a small fraction could be externally mixed, and the text makes this clear.
2-825	A	45:9	45:9	What does "nuances" mean? Please rephrase. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-48)]	Accepted.
2-826	A	45:10	45:11	"The calculation of the cloud albedo indirect effect mostly ignores nuances arising from the particle chemical composition and state of the mixture (external vs. internal). This statement is not correct for the following study, which treated cloud evolution from two size distributions, a fossil-fuel soot emission distribution and an internally-mixed distribution, where particles in the emission distribution entered the internally-mixed distribution upon coagulation, and both were affected by condensation. Clouds grew onto both size distribution. Jacobson, M.Z., Effects of absorption by soot inclusions within clouds and precipitation on global climate, J. Phys. Chem., in press, 2006, www.stanford.edu/group/efmh/jacobson/soot_incl_clouds.htm. [Mark Jacobson (Reviewer's comment ID #: 116-11)]	Accepted. Text modified.
2-827	A	45:17	45:17	"for the observational basis and inferences also" is very poorly phrased and difficult to digest. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-49)]	Noted.
2-828	A	45:20	45:27	The materials in Ch7 (P67, line 20-30) are similar to this part.	Noted. Text changed.

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				[Govt. of China (Reviewer's comment ID #: 2006-33)]	
2-829	A	45:22	45:22	Regarding spectral shape, add earlier references to: Feingold, G., R. Boers, B. Stevens, and W. R. Cotton, 1997: A modeling study of the effect of drizzle on cloud optical depth and susceptibility. J. Geophys. Res., 102, D12, 13,527--13,534. Ackerman, A., O. B. Toon, J. P. Taylor. D. W. Johnson, P. V. Hobbs, and R. J. Ferek (2000): Effects of Aerosols on Cloud Albedo: Evaluation of Twomey's Parameterization of Cloud Susceptibility Using Measurements of Ship Tracks. J. Atmos. Sci.: Vol. 57, No. 16, pp. 2684–2695. [Graham Feingold (Reviewer's comment ID #: 69-15)]	Noted.
2-830	A	45:24	45:26	Chen and Penner (2005) also examine the effect of changes in droplet spectral shape. (Chen, Y. and J.E. Penner, 2005: Uncertainty analysis for estimates of the first indirect effect, Atmos. Chem. Phys., 5, 2935-2948, SRef-ID: 1680-7324/acp/2005-5-2935.) [Joyce Penner (Reviewer's comment ID #: 197-21)]	Noted.
2-831	A	45:46	45:46	Double the confidence limits to two standard deviations:to "--0.9 ± 0.86" [VINCENT GRAY (Reviewer's comment ID #: 88-257)]	Rejected. This value was obtained from the paper itself, and represent the author´s measurement and standard deviation.
2-832	A	45:46	45:46	possible now" should be "now possible [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-50)]	Accepted.
2-833	A	45:49	45:53	Why might the satellite observations provide an upper limit? [Joyce Penner (Reviewer's comment ID #: 197-22)]	Noted. Text changed.
2-834	A	45:53	45:53	This approach represents a step forward, only if the observations are reliable, and I am not confident that the satellite observations are. [Graham Feingold (Reviewer's comment ID #: 69-16)]	Noted. We recognize this point.
2-835	A	46:3		there should be a very brief additional secton (2.4.6.7) that mentions the results of "top-down" estimates of total aerosol radiative forcing, and a cross reference to Section 9.2.1.2 [Danny Harvey (Reviewer's comment ID #: 101-13)]	Rejected. We are very pressed on the length of the text. The chapter is already too large, and adding a new section would be difficult because of size limitations. Addition of such a section entails considerations of several variables that are beyond the scope of this chapter e.g., climate sensitivity.
2-836	A	46:32	46:46	The paragraph includes a quite important concept, that becames more important at regional scales. Whereas at global scales RF may be more relevant. Recent papers suggest that a regigional scales, for example in the Western Mediterranean or south part of North America, land use change history may play a very important role. I suggest to include a	REJECT. Although we agree with the statement made in this comment, and agree that the paper is relevant, it does not add significantly enough to our

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				sentence in the paragraph in that respect, for example: "land-use changes may appear as the main driving factor determining the local atmospheric circulations with potentially important influence at regional scale; for example there is evidence of the loss of summer storms in the mountain ranges of the western Mediterranean Basin as result of local to regional atmosphreic circulations perturbation (M. M. Millán & others, J. Climate, 18, 684-701(2005).). Such concept may be either (may more apropiaded) introduced in the page 49 setion 2.5.5 161 2-161 5 [Govt. of Spain (Reviewer's comment ID #: 2019-13)]	discussion on forcings to warrant inclusion here (especially since contraints on space would mean other text would need to be removed). Morevoer, the comment is not entirely within the scope of the chapter, which is on quantifying the forcing and not the response.
2-837	A	47:29	47:29	What is PNV? I think it has not been defined yet. [Govt. of Finland (Reviewer's comment ID #: 2009-23)]	REJECT. PNV was defined in Introduction to this section.
2-838	A	48:28	48:28	At the end of the paragraph write: "There is even less information on possible effects of changes in managed vegetation except few local investigatons that, in turn, indicate considerable changes in the surface albedo and radiation balance (Mika et al., 2001). [see the two references to Chapter 2, page 96, line 23] (Mika, J., Horváth Sz. and Makra L., 2001: Impact of documented land use changes on the surface albedo and evapotranspiration in a plain watershed. Physics and Chemistry of the Earth, Vol. 26, 601-605) 144 2-144 27 [Govt. of Hungary (Reviewer's comment ID #: 2012-23)]	REJECT. Changes in the nature of managed vegetation are implicit in lines 48:17-48:18 and we do not have the space for additional text on this point.
2-839	A	49:2	49:2	Again, I don't understand this error bar. It implies the term could be negative, but none of the previous text supports this contention. [Keith Shine (Reviewer's comment ID #: 236-34)]	ACCEPT. We will clarify this as part of the whole-chapter revised treatment of uncertainties.
2-840	A	49:16		What do 'internally mixed' and 'externally mixed' mean? [Nathan Gillett (Reviewer's comment ID #: 84-71)]	ACCEPT. This will be clarified. Same reviewer made the same comment concerning DRF of aerosols. These are well-used terms. See TAR chapters on Aerosols and RF.
2-841	A	49:16		in;" to "in: [Junying Sun (Reviewer's comment ID #: 261-23)]	ACCEPT
2-842	A	49:21	49:23	"Jacobson (2004) modeled a decrease in the surface albedo by 0.4% globally and 1% in the Northern hemisphere, which would imply a significant positive global RF estimated at about +0.25 W/m2." This statement has two errors. First, the "decrease in the surface albedo…" should be "decrease in the snow and sea-ice albedo…" Second, the resulting change in RF is approximately +0.06 W/m2, not +0.25 W/m2. There is no report of +0.25 W/m2 in the paper. The +0.06 W/m2 can be obtained by scaling the direct forcing of total ff BC+OM (+0.27 W/m2) using the relative temperature changes	We do not agree with his own interprepation of his own paper. Text in the paper disagrees with this comment. We have dropped all forcing quantification from his paper. "decrease in the surface albedo…" should be "decrease in the snow and sea-ice albedo…"

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				due to BC absorption in snow/ice (+0.06 K) to the temperature change due to ff BC+OM from all processes (+0.27 K). [Mark Jacobson (Reviewer's comment ID #: 116-12)]	
2-843	A	49:27	49:28	A best estimate of 0.10 plusminus 0.30 Wm-2 is given for the radiative forcing of black carbon in snow and ice. Thus the lower limit is -0.20 Wm-2. Are negative values really plausible? [Govt. of Finland (Reviewer's comment ID #: 2009-24)]	ACCEPT. We will clarify this as part of the whole-chapter revised treatment of uncertainties.
2-844	A	49:27	49:27	Double the confidence limits to two standard deviations:to "+-010 ± 0.60" [VINCENT GRAY (Reviewer's comment ID #: 88-258)]	REJECT. We will revise the treatment of uncertainties in a consistent manner throughout the chapter.
2-845	A	49:27	49:28	"giving the Hansen et al. (2005) estimate greater weight due to closer ties to observations and more complete treatment of processes." First, the direct forcing due to BC effects on snow/ice from Jacobson (2004) should be +0.06 W/m2, not +0.25 W/m2. Second, the results of Hansen et al. did not include a more complete treatment of processes than those of Jacobson (2004). For example, the Hansen et al. did not include discretized size-resolved aerosol emission, size-resolved aerosol microphysics, size-resolved evolution of clouds and precipitation from aerosols, interactions of clouds with aerosols, deposition of precipitation-containing BC onto snow and sea ice, radiative transfer through snow and sea ice, radiative transfer calculations for > 600 wavelengths/probability intervals, gas-phase photochemistry that affects condensation onto size-resolved aerosols, subgrid surface treatments or roads, roofs, and vegetation, and many other processes. Both papers contained ties to observations. [Mark Jacobson (Reviewer's comment ID #: 116-13)]	ACCEPT
2-846	A	49:30	49:49	this sub-section should mention that changes in land cover have direct effects on hydrology through evaporation and hence affect precipiation and run off. [Chris Jones (Reviewer's comment ID #: 120-14)]	ACCEPT
2-847	A	49:34		"CH4" 4 is lowerscript [Junying Sun (Reviewer's comment ID #: 261-24)]	ACCEPT
2-848	A	49:47	49:49	An explanation would make this clearer: "In the tropics, reduced evaporation due to deforestation may lead to warming in spite of an increase in surface albedo." [Govt. of Finland (Reviewer's comment ID #: 2009-25)]	ACCEPT
2-849	A	50:1	50:17	Some recent reinterpretation of satalite water vapour over the Mediterranean sea during summer indicate that an unusual accumulation of water vapour, that is being hypothesized	REJECT. This seems to be more within the scope of chapter 7 than this

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				in the past was recycled by the baisin itself but know can migrate somewhere else, carring in coated aerosols. It is being suggested that depending where ends up may influence heavy reains in central Europe or affect the huracane formation if follow the transport track across the Atlantic as described by Porpero and Lamb (Science, 302, 1024-1037 (2003)) (see G. Gangoiti, L. Alonso, M. Navazo, J. A. García, M. M. Millán, J. Geophys. Res.,Acceptted (2005).). I suggest to include the reference. [Govt. of Spain (Reviewer's comment ID #: 2019-6)]	chapter.
2-850	A	50:21	50:28	Dismissing the urban heat island effect because "cities" cover such a small part of the Earth's surface seems simplistic. It does not account for the large heat effects of the transportation sector, a significant portion of which are outside of cities. It also does not account for the sprawl that surrounds most cities, greatly enlarging their area. Finally, it repesents a marked change from the TAR, which said that the urban heat effect could have been as large as 0.12 C during the 20th century. That was up to 20% of the observed temperature rise. More evidence needs to be presented to explain this abrupt change. [Lenny Bernstein (Reviewer's comment ID #: 20-50)]	REJECT. The global HEP figure includes HEP from all human activities including that from transportation and suburbs – we will clarify in the text if space permits. The conclusions of the TAR have been misunderstood by this reviewer.
2-851	A	50:21	50:28	This finding represents a major departure from the TAR, which concluded that the urban heat island effect could have contributed as much as 0.12 C to global average temperature during the 20th century. While AR4 can and should depart from the TAR's conclusions when new information warrents doing so, it should clearly state when it is doing so and provide the reasons for the departure. [Jeff Kueter (Reviewer's comment ID #: 137-47)]	REJECT. The conclusions of the TAR have been misunderstood by this reviewer.
2-852	A	50:22	50:23	There is uncertainty with regard to the meaning of HEP (Human Energy Production) and how it differs from "human activites." Both terms require definition. [Govt. of Japan (Reviewer's comment ID #: 2014-34)]	NOTED. The text will be examined for clarity.
2-853	A	50:27	50:28	I think the phrase "local scales" is inadequate. Having looked back at the SMIC and SCEP reports, etc., it is pretty clear that there can be important effects over metropolitan areas, megalopolises, etc. Saying just "local scales in urban areas" is really understating the importance of this issue. Perhaps instead say major metropolitan regions. [Michael MacCracken (Reviewer's comment ID #: 152-256)]	ACCEPT. This will be rephrased.
2-854	A	50:28	50:28	Insert after "cities" "where surface tremperature is often measured, thus intrioducing an upwards bias in the global average" [VINCENT GRAY (Reviewer's comment ID #: 88-259)]	REJECT. This is not within the scope of this chapter.
2-855	A	50:41	50:44	If this is a "forcing" as distinct from a feedback since it is a "direct response to CO2 rise", then why isn't the 2nd indirect effect of aerosols on clouds considered a forcing?? [Joyce Penner (Reviewer's comment ID #: 197-23)]	NOTED. No action suggested. Reviewer has picked a technical point, we prefer to stick to the nomenclature originating from the cited paper. This does not pose a confusing point to the

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					chapter.
2-856	A	50:49	50:50	It is unclear whether "leaf area" represents the area of ground covered by leaf cover, or total leaf surface area. [Govt. of Japan (Reviewer's comment ID #: 2014-35)]	ACCEPT. This will be clarified.
2-857	A	50:49	50:53	Replace last phrase of the paragraph "The RF due to this process has not been evaluated" with new text. "The global RF due to this process has not been evaluated yet. Estimates of this effect over the North America using data from ISCCP (Zhang et al., 2004) showed substantial decrease in annual mean surface albedo in recent years. The annual mean surface albedo over this region decreased by nearly 0.05 over 1984 to 1999 period (Wang et al., 2006)". References: Zhang, Y., W. B. Rossow, A. A. Lacis, V. Oinas, and M. I. Mishchenko (2004), Calculation of radiative fluxes from the surface to top of atmosphere based on ISCCP and other global data sets: Refinements of the radiative transfer model and the input data, J. Geophys. Res., 109, 27, D19105, doi:10.1029/2003JD004457. Wang, S., A.P.Trishchenko, K.V.Khlopenkov, A.Davidson, 2006: Comparison of IPCC AR4 climate model simulations of surface albedo with satellite products over Northern Latitudes. Journal of Geophysical Research, Atmospheres. Submitted. Revised. [Alexander Trishchenko (Reviewer's comment ID #: 267-1)]	REJECT. This information does not really add to the main message which is that this RF has not been evaluated, so does not warrant taking up space with additional text.
2-858	b	51:3	:13	Text notes that only contrails and cirrus are directly addressed for aviation because other impacts are included earlier (sections 2.3 and 2.4). There is no explicit discussion of aviation in these sections and contrails/cirrus impacts may be relevant to the broader climate change policy discussion. [Govt. of United States of America (Reviewer's comment ID #: 2023-97)]	Noted. Length limitations prevent a more thorough discussion. The comment about other impacts has been removed.
2-859	A	51:4	51:6	Since aviation travel is really part of the transportation sector rather than industrial, the sentence would be more accurate if written as "Like all sectors of human activity, subsonic aircraft operations around the globe contribute directly and indirectly to the RF of climate change" [Steven Baughcum (Reviewer's comment ID #: 16-3)]	Accepted. The word 'industrial has been removed.
2-860	A	51:4	51:4	I do not see any sense to introduce IPCC-1999 instead of (IPCC,1999). I suggest to delete the rest of the sentence after "…,hereinafter designated as IPCC-1999." and use (IPCC,1999) instead of IPCC-1999.87 2-87 18 [Michael Danilin (Reviewer's comment ID #: 55-3)]	Noted. This can be resolved in copyediting.
2-861	A	51:8	51:9	This statement is misleading. There is really no mention of aviation effects in the context of tropospheric ozone and aerosol in those subsections. I found one mention of water injection in section 2.3.7. The Wild et al. and other papers that describe forcing from aircraft NOX are discussed in section 2.10. I actually would prefer to have some	Rejected because of length consideratieons.

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				discussion here. [Malcolm Ko (Reviewer's comment ID #: 135-5)]	
2-862	A	51:9	51:11	While I agree that induced cloudiness is indirect effect, I did not understand why persistent contrail formation is indirect RF effect. This sentence should be re-written. [Michael Danilin (Reviewer's comment ID #: 55-17)]	Rejected. It is indirect because contrail formation requires specific ambient conditions that do not always occur.
2-863	A	51:9	51:12	How do you know that the "induced" cloudiness" caused by aerosols from aircraft should be a forcing rather than a feedback?819 2-819 24 [Joyce Penner (Reviewer's comment ID #: 197-17)]	Noted. For induced cloudiness to occur, no response from the climate system is required. Therefore, induced cloudiness is considered a forcing.
2-864	A	51:12	:13	The phrase "Aviation aerosol also can potentially alter the properties of clouds that form later in air containing aircraft emissions." contradicts the earlier statement that effects of aviation emissions that are not specific to just aviation (such as aerosols) are discussed in other sections. Aviation aerosols need to be considered in context of all other aerosol sources. What % comes from aviation? [Govt. of United States of America (Reviewer's comment ID #: 2023-98)]	Rejected. This effect is simply noted here since no quantitative assessment can be made.
2-865	A	51:17	51:35	When discussing modeling of contrail coverage and optical properties, it would be worthwhile to note that such modeling depends critically on the ability to accurately predict or measure regions of supersaturation with respect to ice and the extent of supersaturation with high spatial resolution. This remains one of the key uncertainties in predicting the impact of line contrails and their impact on cirrus clouds. [Steven Baughcum (Reviewer's comment ID #: 16-4)]	Accepted. Comment added.
2-866	A	51:24	51:24	Actually, recent DLR modeling papers used distance flown instead of fuel used in their contrail calculations. Delete ''with global fuel use" and instead write ''with aircraft-flown distance". [Michael Danilin (Reviewer's comment ID #: 55-19)]	Accepted.
2-867	A	51:24	51:24	Is the "best estimate" a good choice of words? If I read the text I would rather say "most recent estimates". [Malcolm Ko (Reviewer's comment ID #: 135-6)]	Accepted. Changed to 'current best estimate'.
2-868	A	51:24		Using "flight regions" is much better than the previous "flight tracks". However, "remain unchanged" needs to be clarified -- remains unchanged from what? (note that regions or tracks are not perfectly static so it is important to clarify what the authors mean). For example, what are some of the important parameters (e.g. meteorology impacts on fuel burn). [Govt. of United States of America (Reviewer's comment ID #: 2023-99)]	Accepted. Clarified.
2-869	A	51:27	51:27	Add the following sentence after ''…factor of 2.":''This uncertainty range could be larger	Accepted. Sentence on uncertainty

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				because of our poor knowledge of relative humidity near the tropopause and neglecting the ice crystal shape effects." [Michael Danilin (Reviewer's comment ID #: 55-20)]	added above.
2-870	A	51:28	51:28	Add the following after "…0.034 W/m2": "and the best TAR value of 0.02 W/m2". This addition is important since in the current version the best values of the (IPCC,1999) and TAR got mixed up and this confusion managed to propagate into Executive Summary of this Chapter and TS. [Michael Danilin (Reviewer's comment ID #: 55-21)]	Accepted.
2-871	A	51:28		Insert "linearly" before "scaled". [Govt. of United States of America (Reviewer's comment ID #: 2023-100)]	Accepted.
2-872	A	51:35	51:35	Add the following at the end of the last sentence: and relative humidity is poorly known in the upper troposphere" 91 2-91 22 [Michael Danilin (Reviewer's comment ID #: 55-100)]	Accepted. Sentence added earlier in paragraph
2-873	A	51:37	51:37	Table 2-9: Given the title of "radiative forcing terms for global aircraft operation", the forcing from emitted NOx is conspicuously absent. I suggest changing the title to reflect that the intention is to give numbers for contrails and cirrus. [Malcolm Ko (Reviewer's comment ID #: 135-8)]	Accepted. Title changed.
2-874	A	51:39	51:39	How do you know that the "induced" cloudiness" caused by aerosols from aircraft should be a forcing rather than a feedback?820 2-820 25 [Joyce Penner (Reviewer's comment ID #: 197-8)]	Noted. Comment added in this paragraph.
2-875	A	52:13	52:25	It took me a while to catch on. I suggest stating explicitly that the controversy with the Minnis et al. paper is not with the estimated RF, but with the temperature response. Also, would the material fits better in section 2.8? [Malcolm Ko (Reviewer's comment ID #: 135-7)]	Rejected. We prefer to keep the text here but have added a reference to Section 2.8.5.7.
2-876	A	52:13	52:13	The term "aviation cloudiness" is not very clear. Suggest using "aviation-induced cloudiness" as is done subsequently in the report. [Govt. of United States of America (Reviewer's comment ID #: 2023-101)]	Accepted.
2-877	A	52:24	:25	The sentence "In reply, Minnis (2005) highlights the uncertainty in evaluating the regional response to regional forcings in GCMs" needs to be expanded to address what these uncertainties are, how valid they are, and how they are related to his conclusions. The IPCC really needs to consider that it is citing a paper that basically said that aviation could solely account for all global warming -- which logically the IPCC does not agree with as evidenced throughout the AR4. So some clear definitive statements re: Minnis' conclusions must be made if these references are maintained. The panel recommends adding a clear, concluding statement to the paragraph. [Govt. of United States of America (Reviewer's comment ID #: 2023-102)]	Accepted. The concluding sentence has been removed and the word 'incorrectness' has been added to the sentence for emphasis.

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2-878	A	52:27	52:27	Aviation-induced cloudiness" should be "Lack of aviation-induced cloudiness [Govt. of Finland (Reviewer's comment ID #: 2009-26)]	Accepted.
2-879	A	52:33	52:33	The word "weak" sound very strong here. Better "need to be taken with caution". [Thomas Peter (Reviewer's comment ID #: 198-43)]	Rejected. 'Taken with caution' confers too much credibility to these results.
2-880	A	52:44	:46	Need to cite evidence of condensed hydrocarbon [Govt. of United States of America (Reviewer's comment ID #: 2023-103)]	Rejected. No reason given.
2-881	A	52:49	:51	The study by Hendricks et al. (2005) cited showing the potential for significant cirrus modifications by aviation caused by increased numbers of black carbon particles was based on hypothetical scenarios. This should be acknowledged. [Govt. of United States of America (Reviewer's comment ID #: 2023-104)]	Accepted. The word 'modeling' has been added.
2-882	A	52:51	52:52	Why would a response occur far away from flight corridors? Is this due to transport of BC? Explain. [Nathan Gillett (Reviewer's comment ID #: 84-72)]	Accepted. Clarification added.
2-883	A	52:54	52:54	Write ''reduced" instead of ''altered" in order to be more specific. [Michael Danilin (Reviewer's comment ID #: 55-23)]	Accepted. Clarification added.
2-884	A	53:0	58:	I (Pål Brekke, Senior Adviser, Norwegian Space Centre) Believe that the discussion about the Virgo and Acrim irradiance accuracies is still not resolved. The authors of the draft seem to lean towards the results of their own results. It should be considered to let Willson comment on this texts. [Govt. of Norway (Reviewer's comment ID #: 2018-37)]	Criticism that the differences are incompletely understood is acknowledged, but there are works that do indicate this may be more an instrumental rather than of solar origin. These include works other than the authors'. Note that the chapter has been available for a review by anyone - twice. See also response to 2-26. In a solicitation effort undertaken by the chapter in the wake of this comment, Dr. Willson, who did not do reviews of the FOD or SOD, was contacted and requested to assist us in enhancing the text, but a response was not received.
2-885	A	53:7	58:10	The discussion about solar forcing is good, but I think that the crucial question about whether the recent global warming can be due to changes in solar activity is not addressed	Thank you. Accepted. Text in Section 2.7.1.1.2 (second paragraph) modified

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				in a sufficient manner. The fact that the modern instrumental measurements of galactic cosmic rays (GCR), 10.7 cm flux and estimated of the aa-index since the 1950s show no trend. These are after all the most reliable data on the solar activity level. Also, there is the question rgarding GCR and clouds, and no strong evidence for systematic changes in the low cloud cover over the last decades.References can be found in Benestad (2005) GRL vol 32, L15714 and citations therein. [Rasmus E. Benestad (Reviewer's comment ID #: 18-1)]	to include comment about lack of trends ion modern records of GCR, 10.7 cm flux and aa index, as reported by Benestad (2005). The comments are on the mark. We have tried to evaluate strictly based on the available literature. The direct solar irradiance forcing relative to anthropogenic input (whether one takes the TAR or AR4 solar irradiance estimates of the change) is small (this is now compared in 2.9.5). As this chapter is restricted to forcing essentially, we do not discuss the additional relevance in the context of the observed warming. See chapter 9 for discussions of responses due to natural and anthropogenic forcings.
2-886	A	53:12	53:12	The word 'uninterrupted' is misleadng as the record is not homogeneous. [Michael Manton (Reviewer's comment ID #: 157-20)]	Accepted,. Text rewritten,
2-887	A	53:53	53:55	Are the sunspots and faculae the 'source' of the variability? Or are they just indications of solar activity? [Nathan Gillett (Reviewer's comment ID #: 84-73)]	Response: They are sources of variability since their presence on the soalr disk modifies the net photon output due to their contrasts differing from the background quiet Sun. Modified the text to better reflect this.
2-888	A	54:3	54:7	The solar diameter is briefly evoked. The real situation needs a slightly longer text explaining the difference between the seismic radius and the photospheric radius which both are of interest, but have different meanings [Govt. of France (Reviewer's comment ID #: 2010-22)]	Rejected. This is too much detail for the limited avaialable space.
2-889	A	54:9	54:23	This material is on the climate response to solar forcing, and might perhaps fit better in chapter 9. At a minimum reference should be made to the discussion of the detection of solar influence on tropospheric temperature in sections 9.4.4.3 and 9.4.1.4. Stott et al. (2003) and Crooks (2004) are also relevant references here. [Nathan Gillett (Reviewer's comment ID #: 84-74)]	Accpeted. Much of this text has been moved to Chapt 9; which is now referenced..
2-890	A	54:19		40-50 N and S" to"40-50 ºN and ºS	This text moved to Chapter 9 and

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				[Junying Sun (Reviewer's comment ID #: 261-25)]	modified there..
2-891	A	54:24	54:24	(This should be included with the chapter 2 section) The paragraph refers to annual and decadal scale analyses. The R/S analysis by Karner (2003, 2005) encompasses all time scales from daily up to decadal, and yields results pertinent to this paragraph. It should be cited as follows: "Analysis of solar flux data has also shown that total solar irradiance is antipersistent, or dominated by cumulative negative feedbacks, with Hurst exponent <0.5 at all time scales greater than 2 months (Kärner 2004, 2005). Surface and tropospheric temperature data exhibit similar antipersistence, but the stratospheric average temperature data behave independently, exhibiting random-walk behaviour on scales up to 11 years (Kärner 2004, 2005) 743 2-743 37 [Ross McKitrick (Reviewer's comment ID #: 174-25)]	Rejected. Text has moved to Chapter 9, plus space limiations preclude additional text. The proposed addition is very complicated and considerably jargon-istic, and will require more length than imagined to explain how it fits into the context of the message in this sub-section.
2-892	A	54:25	54:25	The title should be "Measurements of solar spectral irradiance variability" as variability is essentially discussed. [Govt. of France (Reviewer's comment ID #: 2010-23)]	Rejected. The Section title is "Variability". Also, the actual measurements are of the irradiance, since the radiometers are abolsutely calibratited. Varibaility is derived from the irradince measurements.
2-893	A	55:0		Very much emphasize is put on the entire solar forcing debate on the shoulders of a single paper (Wang 2005). Although this is the most recent listed in the draft, it is the only paper showing such low variations in the TSI. It could be argued that the main author on this chapter is a co- author on the same paper and thus favour this result over other papers and mechanisms. Another interesting result, if the Wang conclusion is correct would be, that the direct solar forcing already used in past climate models described in TAR have overestimated the solar forcing. Thus, with a lower solar forcing one would get a lower temperature trend unless one increases some other forcing. I believe this implication should be further discussed in the present report. Further, the Wang et al. paper neglects the influence of UV irradiance. There is a paper in press in Astron. Astrophys. by Krivova and Solanki which shows that over a solar cycle 60% of the change in total irradiance is due to changes in the UV. [Govt. of Norway (Reviewer's comment ID #: 2018-41)]	Rejected. See also response to 2-26. The text includes a detailed discussion and references that show why the original assumptions of the earlier irradiance estimates (cited in TAR) are apparently flawed. (SOD 2-56 lines 20-34). The Wang et al approach is one further piece of evidence, and its estimates of long term irradiance variability are consistent with the rejection of the earlier assumptions about the range of variability in sun-like star, the casues of cosmogenic isotope changes and the stability of the long term geoimagnetic indices. The Wang et al paper also estimates long-term UV and spectral irradiance variations (SDO, p. 56, line32). The Krivova and Solaknai estimate of UV vatiations contributing 60% of the

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					solar cycle does not agree with prior estimates that this contribution is 15%. The models used for the prior estimates reproduce cucrrent SORCE observations. Conclusion that the UV contribution is larger have likely neglected instrumental effects in the data. A problem is that alternate peer-reviewed studies that can be cited are not mentioned. It is true that the one study which represents the update in solar forcing estimate has a co-author who is a LA on chapter 2. But, as acknowledged in 2-26 and 2-901, they do not doubt the value of the Wang study nor the credentials of the LA. For information, no other review has disagreed with the new estimates or the underlying reason – through the zero-, first- and second-order drafts. The reduction of the direct irradiance change estimate from TAR to AR4 does not bear substantively on the explanation of the century-scale warming and attributions. This is because the solar forcing was much less than the greenhouse gas forcing even in TAR. Thus, considering the 20th century warming, solar forcing by TAR or AR4 estimate pales into a very small contribution compared to that by the LLGHGs.
2-894	A	55:1	58:8	This is a very good discussion but would be stronger if supplemented by something that summarizes the key advances that lead to your assessment of a smaller RF for solar forcing than previously. As it stands, many different lines of argument are presented and	Accepted (sort of). Two changes have been made. As per comment 2-907, a column has been added to Table 2.10

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				a table or other method to bring them together near the end of the section is needed. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-4)]	to better clarify how current understanding invalidates early. reconstructions Plus, a sentence has been added to the begining of the section. (also in reponee to comment 2-902 and 2-911).
2-895	A	55:10	55:10	The use of sunspot cycle length (SCL) for describing the solar forcing has been put in serious doubts by Benestad (2005): there are extreme variations in the SCL during late 18th and the 18th century, and these cannot be tied up with corresponding climate variability.I do not understand what is meant by 'instantaneous period (cycle length)' as this is a quantity that reflects an interval in time, but I do think that it is not valid to apply a low-pass filter to the SCL. Comparing a 3-point low-pass filtered version of SCL impliesa time scale of ~30 years,and it would be wrong to plot this against a 10-year-filtered temperature (a connection would imply an unphysical time-warp). [Rasmus E. Benestad (Reviewer's comment ID #: 18-2)]	Noted: text revised to remove "instantaneous period". The text does not support the use of cycle length – it simply notes historically what has been used.
2-896	A	55:20	55:20	Which 3 assumptions are you referring to here? [Joyce Penner (Reviewer's comment ID #: 197-26)]	Accpeted. Text modified.
2-897	A	55:34	55:34	This is a rather esoteric sentence for an IPCC report. [Joyce Penner (Reviewer's comment ID #: 197-27)]	Text simplified in line with comment
2-898	A	55:36	55:36	Is the reference 'Lean (2000)'? [Rasmus E. Benestad (Reviewer's comment ID #: 18-3)]	Wang et al (2005) reference moved forward in text.
2-899	A	55:38	55:40	Say somewhere that this is a model of the sun. [Nathan Gillett (Reviewer's comment ID #: 84-75)]	Accpeted, text modified.
2-900	A	56:0	58:	As in the TAR the discussion on the topic around galactic cosmic rays and clouds is fairly week and incomplete. In my opinion the GCR/cloud mechanisms is even more played down in this draft than in the previous TAR. Still, numerous papers have been published on this topic since the TAR publication. And all these mechanisms could well work in tandem. As in the TAR only the direct forcing is included in Figures 2.34 and 2.25. Even if we know that changes in the UV also plays a significant role, and maybe also GCR and clouds. At least these indirect forcing and their potential levels should be addressed. Such numbers has been published I think and now the indirect effects are mentioned, but not their potential forcings. These forcings should be indicated in the figures mentioned above. [Govt. of Norway (Reviewer's comment ID #: 2018-38)]	Rejected. See also resonse to 2-26. The AR4 material builds upon the TAR. The present discussion of the various components of solar forcing is consistent with current understanding of their relative importance and with the space limitations. The "numerous papers" published about GCR/clouds are empirical correlation studies. Correlative studies at best point to the plausibility of mechanism, which is indeed recognized in the text based on the literature. And, the debate that is

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					going on since the TAR is acknowledged in the text. However, even these correlation studies have been substantively challenged such that the degree of the effect remains ambiguous. Unlike for direct solar irradince, there are as yet no quantitiave estimates of the actual radiative forcing arising indirectly from GCR, since the purported mechanism/s have not been quantified. In the absence of quantitative connections, it is difficult to see how there can be a numerical representation on the bar chart. Re: effects on ozone, this is recognized to the extent that there is a 'bullet' in the ES – note ozone profile changes are less certain. Note that Comment 2-922 contradicts this comment. Note also that several forcings, about whom the knowledge base is very low, do not appear on the bar chart owing to the large uncertainty and gaps in knowledge.
2-901	A	56:0	58:	I would encourage IPCC to consider having only one solar physicists on the lead author team of such an important chapter. In particular since the conclusion of this section about solar forcing hangs on one single paper in which J. Lean is a co-author. I find that this paper, which certainly can be correct, is given too much weight. However, I will use the opportunity to stress that I have a lot of respect for the professional work of J. Lean. The inclusion of an expert on the cosmic ray cloud physics would improve this section. In fact a lot of the text and information in this section can be found in Lean et at. "Source contributions to new understaning of global change". [Govt. of Norway (Reviewer's comment ID #: 2018-43)]	See response to 2-26,2-893, 2-900. The main criticism is the work that the new estimate relies on, but it is equally important to note that the result of the work is not being invalidated, nor the co-author (and LA)'s credentials being challenged. No other review or comment has said that this estimate is incorrect. Note that the LOSU for the solar forcing

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					due to irradiance effects is Low, a measure of the uncertainty associated with this forcing. In our view, the estimate and the LOSU properly characterize our understanding of this forcing. For information, it is too late in the AR4 process to introduce additional scientists – these matters were decided by IPCC almost 3 years ago who chose LAs on the strengths of their respective expertise. See also 2-26 which mentions steps taken by the Chapter to bring in further comments on this section by experts, assess the collective scientific arguments and to help us enhance the text.
2-902	A	56:0	58:	The extended discussion on page 56-58 regarding climate effects of the galactic cosmic ray flux needs a clear statement of what the IPCC assessment is for this contribution. [Govt. of United States of America (Reviewer's comment ID #: 2023-105)]	Accepted. This has been attempted with a sentence at the very beginning of the Section 2.7.1
2-903	A	56:3		Define 'ephemeral regions' where first used. [Nathan Gillett (Reviewer's comment ID #: 84-76)]	Accpeted. Text modified.
2-904	A	56:5	56:17	There is significant overlap between this section in Chap. 2 and the equivalent section in Chap. 6 (6.6.3.1). I recommend that the discussion in Chap 6 be shortened and simply refer to the discussion in Chap. 2. [Gavin Schmidt (Reviewer's comment ID #: 227-2)]	Overlap issues have been addressed in revision
2-905	A	56:12		Delete 'in the climate system'. [Nathan Gillett (Reviewer's comment ID #: 84-77)]	? changed "climate" to "terrestrial"
2-906	A	56:20	56:24	These sentences divide the trend in solar irradiance into a 'secular increase', and a change associated with increased amplitude of the solar cycle. Isn't the 'secular increase' a trend in the mean solar irradiance? If so, then changes in the amplitude of the solar cycle would have no additional effect. Presumably the 'secular trend' is a trend in the brightness at solar minimum. This should be explained. [Nathan Gillett (Reviewer's comment ID #: 84-78)]	Noted, Accepted: Text revised to clarify that the trend refrers to long-term changes in the solar minima.
2-907	A	56:20	56:22	The 0.12% figure mentioned here is not the largest in Table 2.10. The Fligge& Solanki reconstruction has 0.3%. I presume the older larger values are no longer trusted (as	Accepted. Table 2.10 revised to include an additional such column.

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				mentioned earlier in section 2.7.1.2.1) but that is not made very clear here. Perhaps the table should include some measure of accuracy or what is now thought to be incorrect? [Gareth S. Jones (Reviewer's comment ID #: 121-9)]	
2-908	A	56:20		Section 2.7.1.2.2: There is a lot of information here to digest. It took me a little while to be confident that I understood it. May I suggest that a clearer distinction is made between the Maunder Minimum to present day minimum numbers and the 1750 to present day average numbers. [Gareth S. Jones (Reviewer's comment ID #: 121-8)]	Accepted. Text revised.
2-909	A	56:22	:24	It is not clear what is meant by "Accounting for the 11-year cycle": does the value of 0.8% apply to current cycle maxima or to current cycle mean? [Joanna Haigh (Reviewer's comment ID #: 95-2)]	Accepted. Text revised. Similar to above comment.
2-910	A	56:27	56:28	This is a very significant result. I trust it will withstand close scrutiny. [Michael Manton (Reviewer's comment ID #: 157-21)]	Noted.
2-911	A	56:38	58:8	Overall, what this section and its conclusion emphasize is the highly uncertain nature of the proposed mechanisms by which indirect solar effects might operate. What ought to be included in the conclusion is an acknowledgement of what is certain: that even if these particular mechanisms are not borne out, it seems clear that that some indirect solar effect on temperature must be at work, given the known strength of the historical correlation between solar activity and temperature. The AR4 should say what mechanisms are certain; and if others are mentioned as possible, this can be accepted. [Govt. of United States of America (Reviewer's comment ID #: 2023-106)]	Accepted. A sentence addressing this request has been added to the beginning of the solar section. Response to this request is consistent with request of comment 2-894.
2-912	A	56:39	56:39	Sorry to be parochial, but I think the paper by Harrison RG, Stephenson DB, 2005: Empirical evidence for a nonlinear effect of galactic cosmic rays on clouds. Proc Roy Soc A doi:10.1098/rspa.2005.1628 (or contact r.g.harrison@reading.ac.uk) deserves a mention here. [Keith Shine (Reviewer's comment ID #: 236-35)]	Accepted.
2-913	A	56:39	58:8	Note comment 10 above. Friis-Christensen appears to have abandoned the idea that this mechanism is significant for recent warming, but Marsh and Svensmark have revised the hypothesis, invoking rather implausible GCR effects on low clouds. N. Marsh, H. Svensmark, 2000. Low cloud properties influenced by solar activity. Phys. Rev. Lett. 85, 5004-5007. H. Svensmark, 1998. Phys. Rev. Lett. 22, 5027-5030. And especially:! P.E. Damon, P. Laut, 2004. Pattern of strange errors plagues solar activity and terrestrial climate data. Eos, 89, 370, 374 [Robert Kandel (Reviewer's comment ID #: 123-17)]	Noted. This comments counters requests for additional commentary on GCR (e.g., comment 2-26,2-900). The latest ISCCP dataset does not appear to support the correlation with low clouds any more. In any case, it is interesting that correlations continue to be made with low clouds, not all clouds. The correlations are considerably weaker when all (rather

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					than only low) clouds are considered, which raises interesting physical questions.
2-914	A	56:43	56:43	A '(' too many [Rasmus E. Benestad (Reviewer's comment ID #: 18-4)]	Accepted. Text revised.
2-915	A	57:0		Most of what is written on this page should be deleted, or at least moved to a relevant later chapter or chapters. 11-year cycle effects relate to climate fluctuations not to long-term climate change, apart from the (small?) long-term changes that result from changes in amplitude of the cycle. But aside from this, what is written here goes way beyond radiative forcing, talking about responses and atmospheric observations relating to the responses. This is not done for other forcings considered in this chapter. [Adrian Simmons (Reviewer's comment ID #: 242-31)]	Partially accpeted, partially rejected. The text about tropospheric responses has been moved, and reference is made Chap 9. The relevance of 11-year cycle effects is for elucidating mechanisms that may also be occurring on longer time scales. When considering indirect effects of solar variability through the stratosphere, it is diifcult to separate forcings and respsones since a stratospheric response can also be a tropospheric forcing, as per comment 2.252.
2-916	A	57:3	57:3	on" should be "in" or "of [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-51)]	Accepted. Text modified.
2-917	A	57:3		Insert 'at solar maximum' after '2-3%'. [Nathan Gillett (Reviewer's comment ID #: 84-79)]	Accepted. Text modified.
2-918	A	57:15	57:24	I think the link between solar variability and the annular modes in the troposphere is not very robust. For example Shindell et al. (2001) look for such an affect in the GISS model and find no annular mode response to solar forcing. Secondly, I'm not sure that this discussion belongs in the chapter on radiative forcing. [Nathan Gillett (Reviewer's comment ID #: 84-80)]	Accepted. Text moved to Chapt 9.
2-919	A	57:15	57:17	In this context the work by Egorova et al. (GRL, 2004) and Matthes et al. (JGR, 2006) are important as they make a connection to near-surface temprature, and should probably be cited. Conversely, I wonder why Lesley Gray's techical report is cited though it is not easily available (moreover it is wrongly cited with an incorrect order of authors). [Thomas Peter (Reviewer's comment ID #: 198-44)]	Noted. Lesley Gray' s report is cited because it provides a very detailed, comprehensive and timely review. This reduces the need for citing (many) specific references for which there is not sufficient space. Note this is an assessment.
2-920	A	57:16	57:24	Haigh, J.D. & H.K. Roscoe, "Solar effects on climate: the link between the stratosphere and troposphere", in press, Meteorologische Zeitschrift (2006), shows that a new index <solar-cycle x QBO> has significant correlation with NAM and SAM at a variety of	Noted.. But lack of space to include this reference.

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				altitudes. [Howard K. Roscoe (Reviewer's comment ID #: 219-11)]	
2-921	A	57:42	57:55	This discussion would be stronger if it noted that observations in limited regions (such as e.g. the US as cited) are less useful in probing the validity of such a mechanism than are global scale studies such as the ones cited on line 49. [Susan Solomon (NOAA) (Reviewer's comment ID #: 247-3)]	Noted. But, the US example is retained partly to address 2-912, and partly due to revision of other elements of the para.
2-922	A	57:42	58:8	There is no trend in the GCR (CLIMAX; Benestad, 2005 and references therein), at least for the lower-energy cosmic rays. I have had a dialogue with Nir Shaviv on RealClimate.org,and Shaviv reckons that the higher-energy cosmic rays may have decreased since the 1970. However, if GCR played a role for the recent global warming, then there should be a decrease in the low cloud cover, and in regions where there is only low clouds, as high clouds are believed to counteract the low clouds and furthermore stand in the way of light. Chapter 3.3-4. notes that any evidence about trends in cloudiness is inconclusive (Fig. 3.4.7 - however, these are for land-regions only) as is there no clear trend in the reflected short wave in Figure 3.4.8 (according to Svenmark, a global warming should imply a reduction negative trend in the SW measured by ERBS), but the balance of data suggests there has been an increase in the total cloud cover since 1950 - consistent with a decrease in DTR. Furthermore, an abledo mechanism should increase the dayside temperatures more than nightside temperatures, unless other mechanisms are involved that can explain how the nightside temperatures increase faster. It is also important to note the highest correlation between the variations in the GCR flux and cloud response is when clouds lag GCR by a few months (Kristjansson et al., 2002), but the purported mechanism suggests that the response should be almost instantaneous. [Rasmus E. Benestad (Reviewer's comment ID #: 18-5)]	Noted. There is much debate about the GCR/cloud association, which the text notes. Thanks, but we will be unable to take up discussions originating on websites and cite them here. But, the science points are useful. These run counter to the comment in 2-900.
2-923	A	57:46	57:47	There is a lot of "and" here. I suggest replacing "and alternative" by "as well as alternative" [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-52)]	Accepted. Text modified.
2-924	A	57:47	:48	Regarding the ISCCP cloud data set mentioned in section 2.7.1.3, the claim does not correspond to cloud cover after 1994. A comment that a possible calibration error has been found, should be added, and by correcting for this error there is a good correlation 8 years after 1994 (Mars Svenamrk, JGR 108(D6), 6, 2003. and Usoskin et al, GRL 31, L16109 (2004). Calibration errors are the lead authors main arguments for not using the Acrim TSI data. But for the ISCCP data possible calibration errors are neglected even if published papers have mentioned this. [Govt. of Norway (Reviewer's comment ID #: 2018-39)]	Rejected: The calibration error referred to does not exist. Calibrations change with time, and that is taken into account in the data. The Usoskin paper referred to requires a detrending of the data, for no apparent reason, to overcome the fact that the subsequent peak in GCR does not correspond to a peak in low cloud cover.
2-925	A	57:53	57:53	anit-phased" should be "anti-phased	Accepted. Text modified.

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				[Jón Egill Kristjánsson (Reviewer's comment ID #: 136-53)]	
2-926	A	58:5		The latter paper is sited on page 2-58, line 5, but the main result is not mentioned. Rather it is used to give the impression that this entire idea is very uncertain. That is a trend I see that for most of the references in this section about GCR. The main results are not mentioned. Instead they are in most cases used to debunk the mechanism. Several of the authors of key papers on this topic should get a chance to review the text in this section. This includes people like Solanki, Usoskin, Shaviv, Svensmark, Willson to mention a few. [Govt. of Norway (Reviewer's comment ID #: 2018-40)]	Noted. The "main results" in the various papers that this reviewer requests, are all, in one way or another, correlations between GCR (or their derivatieves) and some sort of climate indicator. The text has been modified to make this more clear. The text does not say that the idea is very uncertain – it is the degree of association and the quantitative aspects that are very uncertain. Also, the Usoskin et al (2004) reference is now included earlier in the text, while the specific text mentioned has been moved to Chapt 9. See also responses to 2-900
2-927	A	58:10	60:57	This section needs proofreading for grammar. [Nathan Gillett (Reviewer's comment ID #: 84-81)]	Done
2-928	A	58:10		Good section. Need to explain the incorporation of volcanic aerosols in simple climate models and models of intermediate complexity. [European Commission (Reviewer's comment ID #: 2008-17)]	Accepted in part. We discuss briefly the application of the volcanic aerosol data in climate models. The discussion is necessarily general owing to space considerations and attention to this subject given in other chapters. See also response to 2-929.
2-929	A	58:10		Section 2.7.2. The section is very informative, especially subsection 2.7.2.2 "Thermal, dynamic and chemistry perturbations forced by volcanic aerosols". However the section lacks a short paragraph, where the optimal usage of volcanic aerosols in simple climate models / and EMICs is described. Given that there are fairly complex dynamical processes (winter warming etc.), how do or could simple climate models and EMICs best incorporate volcanic forcing time series (e.g. scaling of Sato/Ammann time series by factor of 0.7?)? [Govt. of Germany (Reviewer's comment ID #: 2011-109)]	The physical principles are generally the same. If the simplified model can account for aerosol effects explicitly then they can be used to mimic the volcanic aerosol series. Othervise they can incorporate aerosol radiative forcing calculated using comprehensive climate models used in the IPCC AR4 simulations.
2-930	A	58:21	58:23	Should also reference Tett et al (2002)	The reference was added: Tett SFB,

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				[Gareth S. Jones (Reviewer's comment ID #: 121-10)]	GS Jones, PA Stott, DC Hill, JFB Mitchell, MR Allen, WJ Ingram, TC Johns, CE Johnson, A Jones, DL Roberts, DMH Sexton, MJ Woodage (2002), Estimation of natural and anthropogenic contributions to 20th Century temperature change, J. Geophys. Res., 107(D16), doi 10.1029/2000JD000028.
2-931	A	58:31		see comment #10 - also applies to "unfortunately" here [Danny Harvey (Reviewer's comment ID #: 101-14)]	Accepted.
2-932	A	58:32	58:32	has been" should be "have been [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-54)]	Done
2-933	A	58:33	58:33	"estimates of SO2" should be "estimate SO2" (2 in lower case) [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-55)]	Done
2-934	A	58:33		"SO2" 2 is lowerscript [Junying Sun (Reviewer's comment ID #: 261-26)]	Done
2-935	A	58:39	58:40	A gapfree aerosol record for the satellite age is provided by a new SPARC report: L. Thomason and Th. Peter (Eds.), Assessment of Stratospheric Aerosol Properties (ASAP), WCRP-124, WMO/TD- No. 1295, SPARC Report No. 4, February 2006. [Thomas Peter (Reviewer's comment ID #: 198-45)]	The report was published after FOD was prepared. Reference is included. However, the questions about acuracy of the filling procedures remain. The work on assimilation of stratospheric aerosols for satellite era period is on the way (for information, Larry Thomason, a lead author of the report is a collaborator on this project).
2-936	A	58:42	58:47	This is a very selctive and unexpected choice of references for ice core volcanic histories, and additionally it has little overlap with the references used for the same issue in chapter 6 (their listing is also poor). Good examples would be Palmer, A.S., V.I. Morgan, A.J. Curran, T.D. Van Ommen, and P.A. Mayewski, Antarctic volcanic flux ratios from Law Dome ice cores, Annals of Glaciology, 35, 329-332, 2002; Bigler, M., D. Wagenbach, H. Fischer, J. Kipfstuhl, H. Millar, S. Sommer, and B. Stauffer, Sulphate record from a northeast Greenland ice core over the last 1200 years based on continuous flow analysis, Annals of Glaciology, 35, 250-256, 2002; Castellano, E., S. Becagli, J. Jouzel, A. Migliori, M. Severi, J.P. Steffensen, R. Traversi, and R. Udisti, Volcanic eruption frequency over the last 45 ky as recorded in Epica-Dome C ice core (East Antarctica) and its relationship with climatic changes, Global and Planetary Change, 42 (1-4), 195-205,	Accepted, few references are included

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				2004. (The latter is perhaps over longer timescales than you wish to consider). [Eric Wolff (Reviewer's comment ID #: 292-1)]	
2-937	A	58:47	58:49	This statement is poorly phrased. For stratospheric aerosol, transport is not really the issue, and the deposition problem can be dealt with by using multiple cores (see eg Wolff, E.W., E. Cook, P.R.F. Barnes, and R. Mulvaney, Signal variability in replicate ice cores, Journal of Glaciology, 51 (174), 462-468, 2005.). A better statement would be: "However, the atmospheric loadings...ice records suffer from uncertainties due to imprecise knowledge of the latitudinal distribution of aerosol, and to depositional noise which can affect the signal for an individual eruption in a single core." [Eric Wolff (Reviewer's comment ID #: 292-2)]	The proposed rewording is fine. However, the problem with ice cores and models cannot necessarily be solved by using multiple cores. ALL models overestimate the rate of stratospheric transport of aerosols from equatorial reservoir to poles. A lot of material ends up getting deposited in midlatitudes. The deposition patterns in different ice cores are strongly correlated with precipitation, and averaging does not always allow us to get rid of noise. Correctly relating deposition rates with aerosol loading will need a lot more work.
2-938	A	59:11	59:12	This should state that Amman et al is based on model predictions, rather than measurements. [Howard K. Roscoe (Reviewer's comment ID #: 219-12)]	Accepted. Yes
2-939	A	59:21	59:21	Should also add another important difference between the series, the Ammann (2003) does not include Krakatoa eruption of 1883. [Gareth S. Jones (Reviewer's comment ID #: 121-11)]	Yes, but the dataset implemented in the NCAR simulations does.
2-940	A	59:25		An RF of -3Wm-2 is quoted for the Krakatau and Pinatubo eruptions. To what does this refer? The maximum effect? An average over a certain period? [Adrian Simmons (Reviewer's comment ID #: 242-32)]	Accepted. It is related to the maximum value
2-941	A	59:49	59:49	Sato et al" should be "the Sato et al [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-56)]	Done
2-942	A	59:50	59:50	Please clarify the location of references as the current text says "… use the larger (Ammann et al., 2003) optical depth value (Stenchikof et al., 2006)." So, both references provide optical depths values, but Ammann estimates a higher one? Please clarify. [Govt. of Germany (Reviewer's comment ID #: 2011-110)]	Accepted. Reworded to: "It is interesting to note (Stenchikov et al., 2006) that, in the Pinatubo case, the GISS models that use the Sato et al. (1993) data set yield an even greater solar reflection than the NCAR model which uses the larger (Ammann et al., 2003) optical depth value."

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2-943	A	59:51	59:52	"uncertainties to that in the basic …." is incomprehensible and has to be rephrased. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-57)]	Done
2-944	A	60:15	60:16	see comment #10 - also applies to "however" here [Danny Harvey (Reviewer's comment ID #: 101-15)]	Accepted. Revised.
2-945	A	60:22	60:26	This sounds quite speculative. Is this worth saying? [Nathan Gillett (Reviewer's comment ID #: 84-82)]	Accepted. Revised.
2-946	A	60:36		It would be best to add the Shindell et al 2004 citation already in this chapter's bibliography in with these citations. [Drew Shindell (Reviewer's comment ID #: 235-1)]	Done
2-947	A	61:1	65:40	Utility of RF: after the whole chapter was on RF this whole section comes a bit late. I would suggest to shorten it and put it into a box in order to separate it sufficiently from the rest and not to hinder the flow of arguments. [Thomas Peter (Reviewer's comment ID #: 198-46)]	Rejected. Placed here from FOD review comments
2-948	A	61:2	71:40	None of the cited RF computing methods solves the Schwarzchild radiative transfer equation analytically. Therefore, none of them is able to derive a sound greenhouse function and show how the globally averaged monochromatic optical depth depends on the changes of absorbent components. Without such a computation, the prediction of the changes of terrestrial optical depth remains a blind guess. New high res line-by-line computations on the ERBE (Earth Radiation Budget Experiment, 2004) data shows diminishing surface temperature sensitivities for positive planetary optical depths perturbations. The reason of this decreasing sensitivity could be the change of vertical temperature profile, which adjusts the Earth-atmosphere system to the most effective energy loss (maximum rapid cooling. The energy minimalization requirement leads to a stabilizing feedback, preventing the system form a runaway greenhouse effect. Therefore the RF ena dtemperature predictions of these pages seems to be over-estimated in the light of recent computer runs on measured up- and downward flux components. --- Nowadays the whole issue is in quick theoretical progress, and, in the radiative transfer commuinity, it is far from being settled. This progress is not reflected in the report. [MIKLOS ZAGONI (Reviewer's comment ID #: 300-4)]	References not given, we are unable to evaluate comment
2-949	A	61:6	61:8	The text says that radiative forcing can be used to estimate equilibrium tempearture response, but it cannot be used to estimate transient climate change. I disagree with this. Radiative forcing is related to equilibrium temperature resopnse by the climate sensitivity parameter, lambda, and to transient warming by the TCR. The TCR is better constrained by observations than climate sensitivity (see 9.6.1), therefore I think a strong argument could be made that forcing is a better predictor of transient climate change than it is of equilibrium climate change (this is more strongly dependent on feedbacks). [Nathan Gillett (Reviewer's comment ID #: 84-84)]	Partly accepted. Equilibrium s still the key RF definition. TCR dropped though and text reworded.

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2-950	A	61:6		relative' - should this be radiative? [Nathan Gillett (Reviewer's comment ID #: 84-83)]	No
2-951	A	61:26		See comment #25 regarding use of word "measured". "measured at the tropopause" is in any case redundant, as it is already part of the definition of the RF. [Adrian Simmons (Reviewer's comment ID #: 242-33)]	Accepted, text reworded
2-952	A	61:41	61:42	Boer and Yu (2003) is a better ref here. [Nathan Gillett (Reviewer's comment ID #: 84-85)]	accepted
2-953	A	62:14	62:16	Did these models look at the difference when the 2nd indirect effect is included? The study by Rotstayn and Penner would suggest that the difference will be large when the response of clouds to aerosols is included (Rotstayn, L.D, and J.E. Penner, 2001: Forcing, quasi-forcing and climate response, J. Climate, 14, 2960-2975.) [Joyce Penner (Reviewer's comment ID #: 197-28)]	Text reworded and reference added elsewhere for clarity
2-954	A	62:23		RF does not provide a "diagnostic for understanding climate response" but a prognostic for estimating climate response. [Joanna Haigh (Reviewer's comment ID #: 95-3)]	Not sure about this. I think it does both. Text reworded though for clarity
2-955	A	62:36	62:43	What does "aerosol-cloud interaction" or "cloud-aerosol interaction" mean here? Is the "albedo effect" included? (I suppose not because it is a part of the radiative forcing, but many would still regard it as a form of aerosol-cloud interaction) [Govt. of Finland (Reviewer's comment ID #: 2009-27)]	Accepted. Text expanded
2-956	A	62:42	62:44	"For the magnitude and range of realistic RFs discussed in this chapter, and excluding cloud-aerosol interaction effects, there is high confidence of a linear relationship between global mean RF and global mean surface temperature." First, the direct radiative forcing of individual anthropogenic aerosol components cannot be summed linearly to give the total aerosol direct forcing. This is shown in Table 7 of Jacobson, M. Z., Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001 Where the sum of the tropopause direct forcings of anthropogenic soil dust, S(V), BC, OM, NH4, and NH3 was +0.09 W/m2 whereas the direct forcing due to these same components treated together was -0.12 W/m2. The reason is that exclusion of one component (e.g., ammonium), has a nonlinear effect on water uptake, solid formation, mixing state, and absorption/scattering properties of a particle compared with the exclusion of all components. Second, please see the discussion about Efficacy below. [Mark Jacobson (Reviewer's comment ID #: 116-14)]	Seems more or less linear – rejected. Are we not speaking of nonlineaity in terms of very small forcings? There are residuals in the additivity problem and given small absolute values, perhaps it is not surprising that there is an apparent breakdown.
2-957	A	62:46	63:7	Section 2.85. Efficacy. The term efficacy should be defined more clearly and distinguished from another	Defintion clarifed. Sggested reference not added

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				parameter, the climate response per unit direct forcing of a substance relative to that of CO_2. Efficacy appears to be the surface temperature response per unit total radiative forcing of a substance relative to that of CO_2. If only direct forcing is examined, it appears to be the temperature response of direct forcing ignoring indirect effects on clouds in the temperature response. When indirect effects are included, the indirect radiative forcing appears to be added to the direct forcing in the denominator of the sensitivity parameter (lambda) and the additional temperature change is included in the numerator along with the temperature change due to direct effects. The IPCC report should clarify whether efficacy contains "indirect" forcing in the denominator of the lambda terms. If the above definition is correct, the problem with efficacy is that it hides the greater feedback of aerosols to climate relative to CO_2. Specifically, the efficacy parameter includes indirect forcing in the denominator and the resulting temperature change in the numerator of lambda for aerosols, so the climate response of aerosols relative to CO_2 is always suppressed. The suppression is not realistic since the indirect effect is really an averaged time-dependent climate response of the directly emitted aerosols. The net result is that efficacy results in aerosols having a climate effect per unit forcing (defined in terms of direct+indirect effects) similar to that of CO_2, which is misleading. Another parameter, which appears more realistic because it quantifies the greater response of aerosols relative to CO_2 and uses a consistent denominator (direct forcing), is the total surface temperature (climate) response per unit direct forcing (not total forcing) of a substance relative to that of CO_2. This parameter was used in Paragraph 63 of Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, J. Geophys. Res., 107, (D19), 4410, doi:10.1029/ 2001JD001376, 2002 Where it was found that the climate response per unit direct forcing of different substances relative to that of CO_2 were 1.4 K/W/m2 / 0.6 K/W/m2 = 2.3 for f.f. BC+OM 2.0 K/W/m2 / 0.6 K/W/m2 = 3.3 for SO_2 0.6 K/W/m2 / 0.6 K/W/m2 = 1.0 for CH_4 Note that, for CH_4, the function is unity, as expected, but for aerosols the greater feedbacks to the climate system, due to indirect, semidirect, and other feedback effects, are now clarified. The stronger climate response (in the opposite direction) of SO_2 versus BC in the results above arose because SO_2, when converted to particles, had a greater indirect effect than BC since when BC was first emitted, it was coated by lubricating oil and relatively hydrophobic. The greater climate response per unit direct forcing of BC relative to CO_2 is	

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				expected because BC takes part in many feedbacks that CO2 does not take part in (please see Section 3 of the paper above). Many other models will not currently obtain the same climate response per unit direct forcing because they do not contain all the feedback mechanisms and interactions between aerosols and clouds or treat size resolution and composition of aerosols and clouds as in the study above. [Mark Jacobson (Reviewer's comment ID #: 116-15)]	
2-958	A	62:46		SECTION 2.8.5 Efficacy: I would suggest that this section be expanded conceptually a little to add clarity by a) retitling to "Efficacy and Effective Radiative Forcing ", b) Defining both efficacy and effective forcing at the beginning following NRC 2005 more exactly eg "Efficacy" is defined as the ratio of the climate sensitivity parameter ?i for a given forcing agent to ? for a doubling of carbon dioxide (CO2) (E = ?i / ?CO2). The efficacy E is then used to define an effective forcing fe = f E." and b) Updating the table 4.2 page 88 of this report and including at the end of this section. I think this would add a lot of value to the assessment of this issue to a broader spectrum of users (IA modellers, EMIC drivers etc) [William Hare (Reviewer's comment ID #: 99-7)]	Partly accepted. Our own definiton retained, but moved to start Section. Extra table is not added
2-959	A	62:48	62:57	Please be precise in terms of which definition of "efficacy" you adopt in this report. The one option is, as stated in the first sentence, "Efficacy is defined as the ratio of the climate sensitivity parameter for a given forcing agent to the climate sensitivity parameter for CO2 changes". Thus "scaling the sensitivity" is one possible definition. The other one is, which seems to be adopted further below in the report by defining "EFFECTIVE RADIATIVE FORCING", i.e. to scale the radiative forcing. Note that only in equilibrium, these two definitions are the same, but the transient climate response will be different, if one chooses to scale the forcing rather than the sensitivity. This can be easily seen from the transient energy balance equation RF = Q + lambda*dT It, where Q is the ocean heat uptake and lambda the feedback parameter. It seems appropriate to adopt a "scaling forcing" efficacy definition, rather than a "scaling sensitivity" definition, although it should be double checked for consistency with - and across - all cited efficacy studies. [Govt. of Germany (Reviewer's comment ID #: 2011-111)]	Both are used in literature thefore both are stated here. Definition is moved to front and tightened
2-1317	B	63:47	63:47	Can you enlarge on why solar efficacy should be greater or less than greenhouse gas efficacy? This is an interesting question and the discussion here doesn't answer what I'm interested in knowing. [Stephen McIntyre (Reviewer's comment ID #: 309-7)]	Rejected – space limitations and expansion not justified

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2-960	A	63:51	63:53	This sentence gives the impression that the low efficacy result is an outlier and is thus easily ignored. But the result is one of the few (only?) for solar that is from a fully coupled model and the "unique" technique is grounded in physics which gives a very similar value to the more traditional calculation when applied to the slab-ocean model in Gregory 2004. [Gareth S. Jones (Reviewer's comment ID #: 121-12)]	Rejected. Gregorys own take is that it is an outlier
2-961	A	63:51	63:53	I am not sure I understand what the authors are attempting to say by stating that the slab-ocean model has an efficacy that is within the range of the other models. First one can't confirm or reject a model result just because it does or doesn't conform to other model results unless there is some independent evidence. Secondly how many different models were looked at? I am not sure as only the papers are referenced, but there may be less than the 9 studies used for all the plots, which does not add a great deal of confidence what a range actually is. See comment for Figure 2.23 [Gareth S. Jones (Reviewer's comment ID #: 121-13)]	Sentence now deleted
2-962	A	63:55	63:56	The statement about confidence made in this sentence is wrong. With so few model results one can't rule out the low solar efficacy result. At best the statement should say there is a "low confidence". See comment to figure 2.23 [Gareth S. Jones (Reviewer's comment ID #: 121-14)]	Accepted. Confidence changed to medium 5/10 chance
2-963	A	64:9		see comment #10 - also applies to "thus" here [Danny Harvey (Reviewer's comment ID #: 101-16)]	Accepted
2-964	A	64:19	64:21	This seems to be at odds with Fig 2.23 - one model has an efficacy >1 for tropospheric ozone. [Nathan Gillett (Reviewer's comment ID #: 84-86)]	Accepted, text reworded
2-965	A	64:23	64:38	Section 2.8.5.5. Scattering aerosol. The addition of the climate response per unit direct forcing of SO2 relative to that of CO2 (3.3, Comment 14 above) would be useful to mention here. [Mark Jacobson (Reviewer's comment ID #: 116-16)]	Rejected. Enough detail already supplied
2-966	A	64:38	64:38	The range should be 0.7 - 2.5 if you include the direct forcing results from Rotstayn and Penner (Rotstayn, L.D, and J.E. Penner, 2001: Forcing, quasi-forcing and climate response, J. Climate, 14, 2960-2975.) [Joyce Penner (Reviewer's comment ID #: 197-29)]	Rejected. We use cloud indirect effects from this report as our basis
2-967	A	64:40	65:8	Section 2.8.5.6. Absorbing aerosol. The addition of the climate response per unit direct forcing of f.f. BC+OM relative to that of CO2 (2.3 from Comment 14 above) would be useful to mention here. [Mark Jacobson (Reviewer's comment ID #: 116-17)]	Rejected. Enough detail already supplied
2-968	A	64:43	64:44	Would it be possible to include a short discussion of how it happens that the efficacy can be negative for certain types of aerosols, so that the reader need not try to understand this	partailly accepted. Due to Lack of space, we will cite a reference?

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				surprising result by searching through the cited papers. [Isaac Held (Reviewer's comment ID #: 105-14)]	
2-969	A	65:6	65:7	"…find efficacies both very much larger and very much smaller than 1.0 for biomass and fossil fuel carbon respectively. (Hansen et al. (2005) find similar efficacies for biomass and fossil fuel carbon.). The paper Jacobson, M. Z., The short-term cooling but long-term global warming due to biomass burning, J. Clim., 17 (15), 2909-2926, 2004 found the climate response of biomass burning particles to be in the opposite direction to that of fossil-fuel soot particles prior to the papers of either Penner et al. (2006) or Hansen et al. (2005). The paper states (p. 2922), second paragraph, "Although biomass and fossil-fuel burning emit roughly equivalent levels of BC globally, the modeled short-term climate response of biomass burning (cooling of -0.35 K in year 10) was opposite from that of fossil-fuel (ff) BC+OM emissions calculated in Jacobson (2002a) (warming of +0.35 K in year 5). Although biomass-burning BC suppresses cooling in biomass particles, such particles contain much more OM, K, Na, Ca, Mg, NH4, Cl, SO4, and NO3 than do fossil-fuel soot particles. In addition, the OM: BC in biomass burning particles (8:1) is much larger than in fossil-fuel soot particles (0.5:1 to 3:1)…." [Mark Jacobson (Reviewer's comment ID #: 116-18)]	Rejected. Not sure paper pertains to efficacy
2-970	A	65:23	65:29	... and rest of section 2.8.5. "Efficacy" - Efficacies are discussed in reference to the GCM studies - please include some judgment as to the applicability of "efficacies" for simple climate models and maybe EMICs. For example, it would be very valuable, if this chapter could offer a judgment on whether more appropriate temperature and ocean heat uptake projections might be achieved by applying either a scaling efficacy parameter to the forcing (or to the sensitivity) or something else. [Govt. of Germany (Reviewer's comment ID #: 2011-112)]	Rejected.research perscriptive and outside scope. No clear answer from, research
2-971	A	65:23	65:26	Note that the TAR did not have conclusions regarding efficacy. If you can stick to this conclusion (and I don't believe it holds if you include 2nd indirect effects, as in Rotstayn and Penner (2001) you would need to increase the range to 40%. (Rotstayn, L.D, and J.E. Penner, 2001: Forcing, quasi-forcing and climate response, J. Climate, 14, 2960-2975.)) then it is an important new finding, not a confirmation. [Joyce Penner (Reviewer's comment ID #: 197-30)]	Partly accepted. !st sentence reworded in line with comment. Caveats frame the 25% range, so increasing it is rejected
2-972	A	65:25	65:25	suggest changing "RF is a predictor of" to "RF is proportional to" [Isaac Held (Reviewer's comment ID #: 105-15)]	accepted
2-973	A	65:26		Confidence levels in RF are cited for all RF mechanisms except aerosols and stratospheric ozone changes. Where do we stand in that front? These are important and need to be	Accepted. Sentence added

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				addressed. [Govt. of United States of America (Reviewer's comment ID #: 2023-107)]	
2-974	A	65:27	65:29	actual climate efficacies could be different from those quoted in section 2.8.5'. So the statements that 'we can have high confidence that efficacies for scattering aerosol would be in the 0.7-1.1 range' and similar apply only to models? This needs to be made clear in section 2.8.5.5. More usefully, some attempt should be made to account for the uncertainties due to the use of models here, and fold them into the quoted uncertainty ranges. [Nathan Gillett (Reviewer's comment ID #: 84-87)]	Reejcted. This is made clear at start and end of secton
2-975	A	65:33	65:33	Again, does "aerosol-cloud interaction" include the "albedo effect"? [Govt. of Finland (Reviewer's comment ID #: 2009-28)]	Accepted -text clarified
2-976	A	65:34	65:37	Note that the Rotstayn and Penner results also show that the aerosol cloud albedo effect has a significant effect on the response outside the range you quote above. (Rotstayn, L.D, and J.E. Penner, 2001: Forcing, quasi-forcing and climate response, J. Climate, 14, 2960-2975.) [Joyce Penner (Reviewer's comment ID #: 197-31)]	Accepted – sentence added
2-977	A	65:37	65:38	Please rephrase to clarify the meaning of this sentence. [European Commission (Reviewer's comment ID #: 2008-18)]	Sentence dropped
2-978	A	65:37	65:38	The sentence "As well as any aerosol-cloud interactions… temperature response" is hard to understand, if at all. Please rephrase. [Govt. of Germany (Reviewer's comment ID #: 2011-113)]	Sentence dropped
2-979	A	65:38	65:39	But your figure summarizing these effects does not accound for the cloud interaction terms if the 2nd indirect effect is included. [Joyce Penner (Reviewer's comment ID #: 197-32)]	Correct. As these indirect effects are difficult to qauntify the figure does not do this. Text clairfied
2-980	A	65:42	69:42	The IPCC should be commended for its attempt to provide some guidance on comparing the impact of various emissions, and using economic valuation to guide decisions. However, the lack of sector specific information limits utility. We recommend providing such information. [Govt. of United States of America (Reviewer's comment ID #: 2023-108)]	Thank—you. Unfortunately there are not the publications to provide sector-specific information.
2-981	A	65:52	65:54	I dispute that RF is a measure of equilibrium climate change. I thought it was a measure of the perturbation to the tropopause radiation budget associated with an instantaneous change in forcing. It is just as much a measure of transient climate change as it is of equilibrium climate change. [Nathan Gillett (Reviewer's comment ID #: 84-88)]	Taken into account. "comparitive" added
2-982	A	66:3		Surely cosmic rays are not an RF. They have not been covered earlier in the chapter. [Nathan Gillett (Reviewer's comment ID #: 84-89)]	Accepted, text deleted

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2-983	A	66:12	66:44	I think the authors really need to rethink using the lexicon for scientific understanding that is used for radiative forcing. The problem that persists is that there does not seem to be any account taken of the potential importance of the term to the issue at hand in deciding upon the term used to describe it. This continues to lead to criticisms of scientific understanding that confuse the public. Looking at Figure SPM-2, for example, the uncertainty ranges for stratospheric water vapour and for contrail cirrus are a tenth of a W per square meter or less, yet the level of understanding is "Low" whereas the same level of understanding is given for direct aerosol effects when the uncertainty range spans about 0.8 W per square meter. There are other similar contradictions (though I am glad that the level of understanding on solar has been increased). What is really needed here is an indication about whether the uncertainty is likely to be significant or not--for aerosols and perhaps land surface it is; for other species, the uncertainties are not really that important--or maybe they are, but it would really help to change that column to something indicating likely relative importance in calculating the overall radiative forcing and the response. At the very least, I do not think the brief referral to this column in the figure SPM-2 caption is adequate because the figure will frequently be shown without any explanation of what is meant--a new column/description needs to be created. I also found it particularly interesting that the figure for Question 2.1, Figure 2, did not have this column--so why is it included in the SPM and TS? [Michael MacCracken (Reviewer's comment ID #: 152-257)]	The scientific understanding lexicon has been considerably "beefed up" since TAR, Table 2.11 and the revised text, along with introductions in the TS clearly explains where we are coming from. Futher edits for clarity will be made. However, column retained. Specifically adding this column allows the use of more ojective error bars
2-984	A	66:22	66:23	In what sense do "observations verify the existence of an RF", given that the RF is defined to be the change in a somehat conceptual quantity from its value in 1750. In any case, for any radiatively active component of the atmosphere, the "existence" of an RF is not in doubt: it is the magnitude and sign of the RF that have to be determined. [Adrian Simmons (Reviewer's comment ID #: 242-34)]	Accepted. Text reworded to clarify meaning
2-985	A	66:50	66:50	Sorry if I am being slow, but I couldn't see the efficacies. [Keith Shine (Reviewer's comment ID #: 236-36)]	I couldn't either –the incorrect figure was placed in the final version due to TSU error, they will be reinstated
2-986	A	67:17	67:25	This comes back to my comment on the executive summary. The sentence on line 21 contradicts that on line 17. It would be useful to give the probabilities for a positive RF and for a RF larger than 0.x Wm-2 (x being whatever chapter 9 needs to attribute confidently observed warming to human activities). Is the probability of the net RF to be larger than 0 Wm-2 less than 90%? It does not seem to be the case from figure 2.24, but if it is the case, then I am afraid there is no way to change a "likely" into a "very likely" by tweaking the language. [Olivier Boucher (Reviewer's comment ID #: 27-32)]	Acceted, text reworded
2-987	A	67:17	67:21	Is it likely or very likely that net radiative forcing has been positive since 1750?	Accepted, text reworded, it is very

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				[European Commission (Reviewer's comment ID #: 2008-19)]	likely
2-988	A	67:17	67:21	Please be precise on whether the net RF is LIKELY or VERY LIKELY positive since 1750. Whereas line 17 states VERY LIKELY in terms of warming (which requires at least a net positive RF), the statement in line 21 says that "However, the net RF for all anthropogenic drivers taken together is LIKELY to be positive". Please be consistent. [Govt. of Germany (Reviewer's comment ID #: 2011-114)]	Accepted, text reworded, it is very likely
2-989	A	67:17	67:21	The first sentence here states that it is 'very likely' that humans have exerted a net warming influence on the climate, while the last one states that it is only 'likely' that the net anthropogenic forcing is positive. Surely there is an inconsistency here? Both statements are saying the same thing, but with different probability levels attached. [Nathan Gillett (Reviewer's comment ID #: 84-90)]	Accepted, text reworded, it is very likely
2-990	A	67:17	67:25	(see also Fig. 2.24) Summing up all rad. forcings in a probabilistic way is great improvement compared to the TAR and should be kept. However, the summary bullet taken from that, namely that the net forcing is likely greater than zero, is somewhat meaningless. The important question is whether the forcing is large enogh to be consistent with the observed warming, i.e. can potentially explain the warming or not. If the forcing is say 0.5 W/m2 then although it might had a warming effect over the last century, it is inconsistent with the observed warming. There have been a number of studies since the TAR (e.g. Forest Science 2002, Knutti Nature 2002, see Anderson Science 2003 for a summary) which have shown that the total forcing must be larger than 0.8 W/m2 (probably even more, depending on the method used) to be a consistent explanation for the observed warming. I strongly believe that this matters more than whether the forcing is greater than zero. This should be discussed, and possibly in Fig. 2.24b it should be indicated which part of the PDF is consistent with anthropogenic forcing being able to explain the observed warming. It should also be carried into the executive summary and SPM. I realize that this has some attribution aspect but still I think it needs to be discussed. As it stands now, there is an implicit conclusion that because the forcing is positive, we are confindent that it causes the current warming, which is incorrect. [Reto Knutti (Reviewer's comment ID #: 133-1)]	Rejected. Chapter 9 will compare top down and bottom up appraoches for RF evaluation, we already state this in the intro. SPM/TS will consider comment on how attribution is effected
2-991	A	67:17	:18	The statement, "These summations imply that since 1750, humans have very likely exerted a net warming influence on climate." should be immediately followed by: "An important question is whether the magnitude of anthropogenic RF is large enough to provide a plausible explanation for the observed, industrial-era increase in global-mean surface temperature. Based on a survey of inverse climate-model calculations (Refs 1-6), a minimum threshold of +0.8 W/m2 has been suggested (Ref 7) in order for this causal connection to be legitimate. This threshold is indicated as a vertical line in the lower panel of Figure 2.24. In terms of the well-known RFs associated with greenhouse gases, we see	Rejected. Chapter 9 will compare top down and bottom up appraoches for RF evaluation, we already state this in the intro. SPM/TS will consider comment on how attribution is effected

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				that humans are virtually certain to have exerted a warming influence that exceeds this threshold. When the more uncertain RFs associated with albedo changes are included, the probability distribution expands greatly. Yet even according to this expanded probability distribution, it is likely that the total RF from human activity exceeds the suggested threshold of +0.8 W/m2." [Govt. of United States of America (Reviewer's comment ID #: 2023-109)]	
2-992	A	67:19	67:20	Is the net RF from RF with low and very low LOSU likely OR very likely to be negative? [Olivier Boucher (Reviewer's comment ID #: 27-35)]	Text clarified, and 3rd pdf added to diagram
2-993	A	67:19	67:19	Revise the magnitude of the total error (Is it 0.4 instead of 0.3?) [Govt. of Spain (Reviewer's comment ID #: 2019-19)]	Accepted, error will be reassessed
2-994	A	67:25		The cloud lifetime effect will not "become" anything: it exists but we don't know how large it is. I suggest replacing "(if that becomes realistic) could reduce central estimate" by "would reduce the central estimate" [Joanna Haigh (Reviewer's comment ID #: 95-4)]	Accepted –good idea
2-995	A	67:28	68:6	I have a problem with attributing RF to historical emissions. I will repeat my comment #3 here. I am particularly concerned about figure 2.25 which gives the RF in 2004 due to emissions and changes since 1750. A better way to explain this may be to stick to a version of Figure 2.24, and add to the figure to show what percentage each emission contributes to the change in concentration of each GHG. Another advantage in doing it this way is that one can then include the effects from OH trend on CH4 (p. 20, line 18), and on dynamics feedback on ozone (p. 21, line 20). Also, does the NOx emission include aircraft NOx or not? [Malcolm Ko (Reviewer's comment ID #: 135-9)]	Rejected. Cf. Reply to comment 2-266 We do try to replace changes in radiative active components to primary emissions. The OH trend effect on CH4 is a good example. It is affected by emissions (e.g. NOx, CO and VOCs) and thus part of the CH4 trend is attributed to these emissions. Also, Figure 2.24 is widely used and needs to be kept as simple as possible. Fig. 2.25 includes the same net RF as 2.24, but with the additional information relating the RF to the primary emissions.
2-996	A	67:28		Section 2.9.3: The uncertainties associated with the RF of NOX are not sufficiently discussed in this section. In particular the values quoted in table 2.13 and pictured in figure 2.25 show that the combined effect on CH4 and O3 is positive. Whereas, for instance Shindell et al. 2005 suggest it is negative. Section 2.10.3.4 concludes that it is not yet sensible to quote an overall GWP100 for NOx. I would similarly argue that is not yet sensible to quote a preindustrial-present day RF for NOx (it is not quite the same	Taken into account. Fig. 2.25 deliberately does not show net RF of NOx emissions (as has to be done in GWP calculations). We agree that the uncertainty is quite large wrt.

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				calculation, but similar principles hold). I have issue with the way the ozone and methane forcings are apportioned in table 2.13 (row 8, columns 2 and 9) using coefficients from page 269 of the TAR. These coefficients were calculated using Y2100-Y2000 changes and (for NOx) cannot be applied to Y2000-Y1850 changes since the chemistry is so non-linear, and the chemical environment was so different in the pre-industrial. I suggest having no entries for NOx in table 2.13 or figure 2.25 with a note to explain why. If it is necessary to include NOx values, the Shindell et al. (2005) ones should be quoted since no other study has separated out the effect of a 100% anthropogenic NOx reduction. It is wrong to extrapolate back from other studies by assuming linearity. I suggest adding text to page 67, line 57 after "10% or less." saying:-" The total global mean forcing from NOx emissions is very uncertain as it is the combination of effects on methane, ozone and nitrate aerosols. Many studies have considered the effects of NOx on ozone and methane through perturbations to the present day atmosphere (e.g. Fuglestvedt et al. 1999, Wild et al. 2001, Berntsen et al., 2005; Berntsen et al. 2006); however due to the non-linearities in the NOx chemistry (e.g. Wang and Jacob 1998) it is not possible to extrapolate back from these to determine the NOx contribution to the preindustrial to present day forcing. One study (Shindell et al. 2005) has simulated the impact of preindustrial to present day NOx changes and calculated a contribution to the methane forcing of -0.17 Wm-2 and to the ozone forcing of +0.06 Wm-2. This gives a total of -0.11 Wm-2 +/- 0.06 Wm-2. When combined with the contribution to nitrate aerosol forcing, the emissions-based forcing from NOx is likely to be significantly negative." [William Collins (Reviewer's comment ID #: 45-10)]	The net, but fig. 2.25 only show the individual effects on CH4 and ozone. The intent is to show that NOx has this dual effect, not giving the exact numbers for the net. Thus NOx effects are kept in the figure. To avoid using the TAR linear response function, we have replaced the numbers with those of Shindell et al. 2005.
2-997	A	67:28		Hansen et al (2005) calculated the contributions of individual aerosols to the indirect effect. His findings should be reported here. [European Commission (Reviewer's comment ID #: 2008-20)]	Partly Rejected. As stated by Hansen et al. (2005, paragraph 90) 'However, because of the complexity of aerosols, with internal and external mixtures of various compositions, our poor knowledge of aerosol source distributions, and the crude representations of aerosols in climate models, it is not possible today to do a good job of such an apportionment of the indirect effect.' A note of this is included in the text and through an extention of Table 2.13, with ref to Hansen et al.

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					The apportionment done by Hansen et al. (their fig. 14) is only for what they define as the 'Cloud Cover Effect' which includes the cloude lifetime effects (Albrecht, 1989), and is thus not consistent with the IPCC defined cloude albedo effect which is shown in Figure 2.25.
2-998	A	67:28		Section 2.9.3 "Global Mean Radiative Forcing by Emission Precursor". Please include a short discussion of the results in Hansen et al (2005) "Efficacies of climate forcings" and specifically his (uncertain) results about the individual aerosols' contributions to the indirect (cloud-albedo) effect (see his figure 14). His result (flagged as uncertain in his study) that e.g. Organic Carbon might have a similarily strong effect in terms of its indirect forcing compared to SOx is very policy significant, as these non-SOx aerosols (precursor) emissions are projected to follow different emission pathways in the future compared to SOx. Thus, please include a paragraph in this section that specifically deals with the question about the different aerosol's possible contributions to the indirect effects and whether (or not) there can be made any quantitative judgments at this stage. [Govt. of Germany (Reviewer's comment ID #: 2011-115)]	Rejected Cf. Reply to comment 2-997
2-999	A	67:42	67:42	I was worried that the CFC/N2O split in the ozone depletion referred back to a very old pre-ozone hole paper (Isaksen et al 1986) - isnt there something more up-to-date. Also, the absence of halons seems strange to me. [Keith Shine (Reviewer's comment ID #: 236-37)]	Accepted. New references to Nevison et al., JGR, 1999 and WMO 2002 replace the Isaksen et al. Ref.
2-1000	A	67:45	67:45	The nitrate aerosol forcing seems inconsistent between Tables 2.12 and 2.13 [Keith Shine (Reviewer's comment ID #: 236-38)]	Accepted, Changed in Table 2.13 to -0.1 W/m2.
2-1001	A	67:56	67:57	Replace:- "the effect of non-linear chemistry on RF is" by:- "the non-linear effect induced by treating the precursors separately is". It is important to clarify this, because the RF response to NOx emissions starting from the preindustrial atmosphere will be non-linear in the NOx emission rate to greater than 10% (see for example the O3 response to 50% and 100% NOx removal in table 1 of Shindell et al. 2005). This has implications for table 2.13 and figure 2.25. [William Collins (Reviewer's comment ID #: 45-6)]	Accepted.
2-1002	A	68:27	68:27	"Chapter 10". Should this be "chapter 9"? Most of the 20th Century simulations work is described there. [Gareth S. Jones (Reviewer's comment ID #: 121-15)]	Accepted. Reference to Chapter 9 made later in sub-section.
2-1003	A	68:27	68:27	There is a sentence "see also Chapter 10" for Figure 2.26, I could not find the corresponding description in Chapter 10. So how about add the following sentence in	See response to 2-1002

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				Chapter 10, and modify the sentence "see also Chapter 10.2: Projected Changes in Radiative Forcing)" [Yoko Tsushima (Reviewer's comment ID #: 269-4)]	
2-1004	A	68:36	68:39	The text here states that GHG forcing is smaller than the indirect effect of aerosol - this therefore implies that the net anthropogenic forcing is negative, which is at odds with the statement on pg 67, ln 17, that humans have very likely exerted a net warming influence on climate. [Nathan Gillett (Reviewer's comment ID #: 84-91)]	Accepted, paragraph is modified.
2-1005	A	68:36	68:36	Chapter 10. Should this be "chapter 9"? Most of the 20th Century simulations work is described there. [Gareth S. Jones (Reviewer's comment ID #: 121-16)]	Accepted
2-1006	A	68:42	68:44	The text states that the indirect effect of aerosol is comparable in magnitude to the GHG forcing, again in contrast to the assertion that net anthropogenic forcing is very likely positive (pg 67, ln 17). [Nathan Gillett (Reviewer's comment ID #: 84-92)]	Accepted, sent. will be deleted.
2-1007	A	68:50	68:50	See earlier comment on "surface forcing". Needs to be defined and explained. [European Commission (Reviewer's comment ID #: 2008-21)]	Accepted. Surface forcing is introduced in section 2.2. This section describes the instantaneous change..
2-1008	A	68:50	68:50	Please include a reference to a definition and a non-expert description of the "surface forcing" concept. An introduction to the concept of "surface forcing" would fit into current section 2.2. [Govt. of Germany (Reviewer's comment ID #: 2011-116)]	Accepted. See response to 2-1007.
2-1009	A	68:53	68:53	Chapter 10. Should this be "chapter 9"? Most of the 20th Century simulations work is described there. [Gareth S. Jones (Reviewer's comment ID #: 121-17)]	Rejected. The reference here is to chapter 10.
2-1010	A	68:56	68:56	"not well to poorly constrained" is incomprehensible [Govt. of Finland (Reviewer's comment ID #: 2009-29)]	Accepted. Sent. will be revised.
2-1011	A	68:56		What does this mean? [Nathan Gillett (Reviewer's comment ID #: 84-93)]	Accepted, will be revised.
2-1012	A	69:13	69:15	This statement implies that the stratospheric adjustment is not an issue for RF due to increase in CO2. Is this true? CO2 influences the stratospheric radiative heating rates substantially. [Govt. of Finland (Reviewer's comment ID #: 2009-30)]	Noted. Instantaneous estimate presented here for simplicity. Only this was available from the models at the time of preparing this chapter. For the points made in this sub-section, this is sufficient.
2-1013	A	69:38	69:39	A difference in the amount of energy absorbed by the troposphere from the low to high	Accepted, text will be revised.

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				latitudes" is a bad expression because the amount of energy absorbed is different at low and high latitudes even without anthropogenic influences. It should be something like "a difference in radiative forcing of tropospheric absorption [or heating rates] between low and high latitudes [Govt. of Finland (Reviewer's comment ID #: 2009-31)]	
2-1014	A	69:41	69:41	The comma (,) should be a period (.) [Govt. of Finland (Reviewer's comment ID #: 2009-32)]	Accepted
2-1015	A	69:44		SECTION 2.10 GWPs and Other Metrics for Comparing Different Emissions COMMENT: Much of the beginning of this section introduces the issue in a manner that takes a position in an ongoing debate academically and politically and also interprets decisions made in the UNFCCC and Kyoto context in ways that are not universally agreed. Care needs to be taken to reword these areas so as to be non prescriptive and policy neutral and do not attempt and implicit effort to review WG3 literature! Literature cited in further comments below on individual sections of text which contradicts or challenges to some significant degree the description given in the current text includes: Aaheim, A., and A. Schjolden (2004). "An approach to utilise climate change impacts studies in national assessments." Global Environmental Change Part A 14(2): 147-160. Aaheim, H. A. (1999). "Climate policy with multiple sources and sinks of greenhouse gases." Environmental & Resource Economics 14(3): 413-429. Aaheim, H. A., J. S. Fuglestvedt, and O. Godal (2004). Costs Savings of a Flexible Multi-Gas Climate Policy. Oslo, Norway, CICERO: http://www.cicero.uio.no/media/2658.pdf Rypdal, K., T. Berntsen, J. S. Fuglestvedt, K. Aunan, A. Torvanger, F. Stordal, J. M. Pacyna et al. (2005). "Tropospheric ozone and aerosols in climate agreements: scientific and political challenges." Environmental Science & Policy 8(1): 29-43. Sygna, L., J. S. Fuglestvedt, and H. A. Aaheim (2002). "The adequacy of GWPs as indicators of damage costs incurred by global warming." Mitigation and Adaptation Strategies for Global Change 7(1): 45-62(18). Torvanger, A. (2004). Would including more source species enhance the cost-effectiveness of climate policy? Oslo, Norway, CICERO: http://www.cicero.uio.no/media/2660.pdf [William Hare (Reviewer's comment ID #: 99-8)]	Accepted, careful rewording has been done. Cf. specific comments below.
2-1016	A	69:44		ABSTRACTS OF ARTICLES CITED Aaheim, A. and A. Schjolden (2004). "An approach to utilise climate change impacts studies in national assessments." Global Environmental Change Part A 14(2): 147-160. This paper proposes methods to assess the socioeconomic impacts of climate change within the framework of national accounting and macroeconomic models. The methods	Noted.

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				are illustrated with examples. The framework of national accounting serves several important purposes in the assessment of the national impacts of climate change. First, an accounting system requires that assumptions and output from independent sector studies of impacts be standardised and made comparable. Second, it serves as a checkpoint for the availability and quality of information about impacts of climate change. Third, it provides a starting point for more extensive macroeconomic analysis of impacts. Aaheim, H. A. (1999). "Climate policy with multiple sources and sinks of greenhouse gases." Environmental & Resource Economics 14(3): 413-429. This paper studies how inclusion of many sources, sinks and reservoirs - a comprehensive approach - affects climate policy, compared with a control merely of CO2. Two questions of particular importance arise in such an analysis. One is how to aggregate the emissions of different climate gases, and the other is how to include all relevant measures in the analysis. To aggregate gases properly, an intertemporal analysis should be carried out. To assure that all relevant measures are included, we suggest that certain measures to reduce emissions of greenhouse gases are specified explicitly and evaluated together with indirect measures, such as carbon charges. A numerical analysis based on an optimal control model indicates that direct measures may play an important role in the design of climate policy, especially for the control of the emissions of greenhouse gases other than CO2. Similar to other studies of the time-path for abatement efforts, the bulk of abatement should be taken by the end of the planning period. This result is significantly strengthened if gases with short life-times in the atmosphere, such as methane, are subject to control. Aaheim, H. A., J. S. Fuglestvedt, et al. (2004). Costs Savings of a Flexible Multi-Gas Climate Policy. Working Paper 2004:03. Oslo, Norway, CICERO. Current climate policies are based on the use of the Global Warming Potential (GWP) index to compare various greenhouse gases. Yet, from an economic point of view, more efficient methods exist. The purpose of this paper is to examine the potential cost savings from applying an efficient and more flexible metric as compared to using GWPs, given some long-term goal for stabilization of the climate. We also calculate the costs when only emissions of carbon dioxide (CO2) are targeted. As compared to the least cost multi-gas flexible case, we estimate that the mitigation costs are increased by about 2% by using GWPs, which amounts to about 16-106 Billion US $ per year depending on the stabilization goal. If only CO2 emissions are targeted, costs increase by about 11%. Given our assumptions we conclude from this that most cost savings that stem from including non-CO2 greenhouse gases in climate policy may be realized when applying GWPs, even though these gas tradeoffs are rather different from the efficient ones. Sygna, L., J. S. Fuglestvedt, et al. (2002). "The adequacy of GWPs as indicators of damage costs incurred by global warming." Mitigation and Adaptation Strategies for	

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				Global Change 7(1): 45-62(18). <P>This article looks at the ability of Global Warming Potentials (GWPs) to work as indicators of equivalence for temperature development and damage costs. We look at two abatement scenarios that are equivalent when using 100-year GWPs: one scenario reduces short-lived gases, mainly methane (CH_4); the other scenario reduces carbon dioxide (CO_2). Despite their equivalence in terms of CO_2 equivalents, the scenarios do not result in equal rates or levels of temperature change. The disparities continue as we move further down the chain of causality toward damage costs, measured either in terms of rate of climate change or level of climate change. Compared to the CH_4 mitigation scenario, the CO_2mitigation scenario gives present value costs 1.3 and 1.5 times higher for level- and rate-dependent damage costs, respectively, assuming a discount rate of 3. We also test the GWPs for other time horizons and the conclusions remain the same; using GWP as an index to reflect equivalent climate effects and damage costs from emissions is questionable.</P> Torvanger, A. (2004). Would including more source species enhance the cost-effectiveness of climate policy? Policy Note 2004:02. Oslo, Norway, CICERO. Incorporating ozone precursors and particle emissions in future climate policy agreements could improve the level of cost-effectiveness, but would also add complexity and complications to negotiation, reporting and implementation. To assess the cost saving potential, a case study of Norway is carried out. Only NMVOC and NOx are included, since data for the other species are not available. It turns out that the potential for reducing emissions of these gases is limited, and in the range of 4 to12 % of the potential of the six gases included in the Kyoto Protocol. One must be careful when trying to generalize the results from Norway to other countries. Rypdal, K., T. Berntsen, et al. (2005). "Tropospheric ozone and aerosols in climate agreements: scientific and political challenges." Environmental Science & Policy 8(1): 29-43. In addition to the six greenhouse gases included in the Kyoto Protocol, the tropospheric ozone precursors CO NMVOC and NOx and the aerosols/aerosol precursors black carbon, organic carbon and SO2 also play significant roles in climate change. The aim of this paper is to review some of the main scientific and political challenges associated with incorporating tropospheric ozone and aerosol precursors into climate agreements, and to discuss how these challenges have a bearing on the design of future climate agreements. We argue that the optimal policy design for a particular substance depends on a combination of scientific and political concerns. We look particularly at regional climate effects, negative forcing, metrics (measuring climate effects against other gases on a common scale). political attractiveness. and verification and compliance. We systematically review the existing knowledge on these issues. explore their impact on	

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				policy design. and conclude that, with current scientific knowledge. CO and NMVOC could conceivably be included in a global climate agreement. either in a basket,with the long-lived greenhouse gases or in a separate basket. while NO_x and aerosols might be regulated more appropriately through regional agreements with links to a global agreement. However, the complexity and fairness implications of including tropospheric ozone precursors and aerosols might negatively affect the political feasibility of a future agreement. (C) 2004 Elsevier Ltd. All rights reserved. [William Hare (Reviewer's comment ID #: 99-11)]	
2-1017	A	69:48	69:49	There are two issues here: the use of the term "trade-off" and the reference to aerosols as a necessary part of a framework.1) The use of "trade-off" is complicated: what is one trading off and over what timeframe (climate damages, mitigation costs etc) and these can be quite different depending on the metrics used and maybe even incompatible (Sygna, L., J. S. Fuglestvedt, and H. A. Aaheim (2002). "The adequacy of GWPs as indicators of damage costs incurred by global warming." Mitigation and Adaptation Strategies for Global Change 7(1): 45-62(18).) I would suggest changing "the trade-off between" to "comparing" 2) It is by no means clear and certainly not agreed that "Multi-component abatement strategies" need aerosols and their are different views about this (see eg Rypdal, K., T. Berntsen, J. S. Fuglestvedt, K. Aunan, A. Torvanger, F. Stordal, J. M. Pacyna et al. (2005). "Tropospheric ozone and aerosols in climate agreements: scientific and political challenges." Environmental Science & Policy 8(1): 29-43. and Torvanger, A. (2004). Would including more source species enhance the cost-effectiveness of climate policy? Oslo, Norway, CICERO: http://www.cicero.uio.no/media/2660.pdf). I would therefore suggest rewording sentence so that it reads "It is generally agreed that multi-component abatement strategies to limit anthropogenic climate change need a framework and numerical values for comparing emissions of different greenhouse gases. The efficacy of inclusion of short lived forcing agents in such strategies remains controversial" [William Hare (Reviewer's comment ID #: 99-9)]	(1) Rejected. The purpose of the emissions metrics discussed in this section is to provide a basis (i.e. aggregate climate impacts of a given emission to numerical values (could be regional and/or temporal dependent)). That means that chosing a given metric will provide a basis for trade-offs. (2) Accepted. At this stage in the Section (first sentencein the Introduction) there is no need to be specific, so the whole parentesis has been deleted.
2-1018	A	69:50	69:54	This sentence contains several elements that are policy prescriptive and/or not fully correct: 1) "necessary tool to operationalize comprehensive and cost-effective policies" 2) reference to Article 3 to justify 1); 3) reference to " decentralised manner " is ambigous and could be misinterpreted and 4) prescribing that "allowing for substitution between different climate agents" is an essential part of operationalization of cost effective policies (this may not be the case see Rypdal, K., T. Berntsen, J. S. Fuglestvedt, K. Aunan, A. Torvanger, F. Stordal, J. M. Pacyna et al. (2005). "Tropospheric ozone and aerosols in climate agreements: scientific and political challenges." Environmental Science & Policy 8(1): 29-43.) Suggest rewording of this sentence to something like: The Global Warming Potential (GWP) or other emission metrics are accepted as	Accepted. Much of this text has been deleted or rreworded.

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				providing importants tools to operationalize comprehensive and cost-effective, multi-gas policies for the long lived greenhouse gases." . [William Hare (Reviewer's comment ID #: 99-10)]	
2-1019	A	69:53	69:53	'specified target' has a particular policy interpretation. It would be more policy neutral to use expression 'specific emissions constraint'. Term constraint leaves open whether driver is 'volume' or 'price'. [Govt. of Australia (Reviewer's comment ID #: 2001-167)]	Accepted.
2-1020	A	69:54	69:57	This sentence contains statements which are dubious or oversimplified. It is not at all clear that the metric formulation depends on the factors indicated. It is certainly the case and agreed in the literature that the choice of metrics or the parameters underlying them (time horizon) affects the outcome climatically in significant ways. This sentence needs to be deleted I think and consequently the beginning of the next sentence the words "In both cases" would need to be deleted. [William Hare (Reviewer's comment ID #: 99-12)]	Taken into account. The sentence has been clarified and backed up by including references to the literature discussing this distinction. O'Neill, Fuglestvedt, Tol Economics, Natural Science, and the Costs of Global Warming Potentials" Journal Article by Brian C. O'Neill Climatic Change 58 (2003): 251-260.
2-1021	A	69:54	70:2	The sentence stating "metric formulation depends" on whether we're interested in the UNFCCC objective or reducing climate change impacts is not clear; what is the distinction? [Govt. of United States of America (Reviewer's comment ID #: 2023-110)]	Taken into account – cf. reply to comment 2-1020.
2-1022	A	69:55	69:55	delete the reference to "cost-benefit analysis", as this is a highly contentious issue that goes beyond science, which should be the only subject of WG1 (the entire material in paranthesis should be deleted) [Danny Harvey (Reviewer's comment ID #: 101-18)]	Accepted.
2-1023	A	69:55		nowhere does the UNFCCC declare its goal to be "avoiding dangerous climatic change". Rather, Article 2 declares its goal to be to "prevent dangerous antrhopogenic interference in the climate system". The distrinction between the two is not trivial, and has major policy implications, as explained in a forthcoming paper of mine ("Dangerous Anthropogenic Interference, Dangerous Climatic Change, and Harmful Climatic Change: Non-Trivial Distinctions with Significant Policy Implications", accepted for publication in Climatic Change) [Danny Harvey (Reviewer's comment ID #: 101-17)]	Taken into account. Reference to UNFCCC goal removed from text.
2-1024	A	69:57	69:57	Kyoto Protocol does not include a long-term target: (1) delete text in brackets; (2) replace 'target' by 'constraint'	Accepted

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				[Govt. of Australia (Reviewer's comment ID #: 2001-168)]	
2-1025	A	70:2	70:10	These sentences engage in one side of economic debate in the field are in appropriate in this chapter and should be deleted. Speficially 1)"both formulations also involve input from economics" is a claim and not a fact 2) SOME "Economists have argued" in favour of cost benefit approach applied here but this is by no means the agreed view in the field 3) "Shorter term targets such as rate limits are also discussed in the literature with different implications for the choice parameters for metrics etc. So please delete "In addition, both formulations also involve input from economics. Economists have argued that, ideally, the metric should be the outcome of an analysis that minimizes the discounted present value of damages and mitigation costs (e.g., Manne and Richels, 2001). If a climate forcing reduction trajectory is formulated to achieve a long-term target the proper trade-off between gases is then their relative contribution to that trajectory, that is, the ratio of the shadow prices. The shadow price of gas g is the reduced cost of meeting the desired policy if we were allowed to emit one extra unit of gas i at time t. Otherwise, if a long-term target is not set, the proper trade-off is the relative contribution of various gases to the impacts, that is, the ratio of the marginal damage costs." 463 2-463 13 [William Hare (Reviewer's comment ID #: 99-168)]	Taken into account. The text has been significantly shortened. We have attempted only to refer to economics to remind the readers that emission metrics is an issue that has also been studied by economists. The whole discussion of these alternative metrics are referrred to the WGIII report.
2-1026	A	70:2	70:32	DELETE THE ENTIRE MATERIAL BEGINNING WITH "IN ADDITION" as all of this is highly contentious, has all sorts of implicit ethical and moral judgements which you have not even begun to address, and goes way beyond the core science, which is the only thing the WG1 should deal with. [Danny Harvey (Reviewer's comment ID #: 101-19)]	Taken into account – cf. comment 2-1025
2-1027	A	70:4	70:5	This appears to be more fitting for the Working Group III report, where such statements can be made with all their ramifications. Here it works confusing and it does not do justice to the rich literature on this topic. [European Commission (Reviewer's comment ID #: 2008-22)]	Accepted – cf. comment 2-1026
2-1028	A	70:4	70:5	The current sentence "Economists have argued that, ideally, the metric should be the outcome of an analysis that minimizes the discounted present value of damages and mitigation costs (e.g. Manne and Richels, 2001)." Please delete that sentence as it is a) not an appropriate reflection of the wide and diverging views in the economic literature and b) not central to the GWP discussion in the WG1 report. In regard to a) there is a large debate both on discounting as well as the appropriateness of the traditional cost-benefit analysis to the climate change problem (e.g. distributional issues, etc..). [Govt. of Germany (Reviewer's comment ID #: 2011-117)]	Accepted – cf. comment 2-1026
2-1029	A	70:4		Define "discounted present value". Reference in Glossary if used elsewhere. Define as used in WG III.	The term "discounted present value" has been removed from the discussion.

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				[Govt. of United States of America (Reviewer's comment ID #: 2023-111)]	
2-1030	A	70:9	:10	Define "marginal damage costs". Reference in Glossary if used elsewhere. Define as used in WG III. [Govt. of United States of America (Reviewer's comment ID #: 2023-112)]	The term "marginal damage cost" has been removed from the discussion
2-1031	A	70:26	70:28	The paper by den Elzen et al. (2005) should be mentioned. (den Elzen M, Fuglestvedt J, Hohne N, et al. Analysing countries' contribution to climate change: scientific and policy-related choices ENVIRONMENTAL SCIENCE & POLICY 8 (6): 614-636 2005) [Joyce Penner (Reviewer's comment ID #: 197-33)]	Accepted.
2-1032	A	70:33	75:19	GWP and radiative efficiencies are defined and discussed in the text on a per mass basis, however radiative efficiencies in Table 2.14 are in Wm-2ppm-1. I suggest to at least add a remark pointing this out in the text. [Gian-Kasper Plattner (Reviewer's comment ID #: 200-1)]	Rejected. Explanation becomes a bit tutorial.
2-1033	A	70:38		Define "time horizon". Reference in Glossary if used elsewhere. Define as used in WG III. [Govt. of United States of America (Reviewer's comment ID #: 2023-113)]	Rejected. Time horizons are defined through expicitly giving the equation giving the difinition of teh GWP. In Section 2.10.2 when Table 2.14 is referenced the normal time horizons of 20, 100 and 500 years are given.
2-1034	A	70:39		Remove "decay in". (C is the abundance not a rate of change). [Joanna Haigh (Reviewer's comment ID #: 95-5)]	Accepted.
2-1035	A	70:42	70:50	most of this material can be deleted, given the deletion of lines 2-32, and the rest modified. [Danny Harvey (Reviewer's comment ID #: 101-20)]	Rejected. The text is needed as lines 2-32 is not completely deleted, but made significantly shorter.
2-1036	A	71:2	71:22	Is this much detail on the economic implications of different metrics needed? [Nathan Gillett (Reviewer's comment ID #: 84-94)]	Yes, we do think so. Metrics are policy tools that are used directly. Thus, the statement on line 21, that 'GWPs remians the recommended metric' needs a solid foundation.
2-1037	A	71:2		You state "The adequacy of the GWP concept has been widely debated since its introduction", which was in 1991 or 1992, but you only cite papers from 2000 onward. An early critique, close to the time of its introduction, is: Harvey, L.D.D. 1993, "A guide to global warming potentials (GWPs)", Energy Policy 21: 24-33. [Danny Harvey (Reviewer's comment ID #: 101-21)]	Accepted.
2-1038	A	71:10		Define GTP. [Nathan Gillett (Reviewer's comment ID #: 84-95)]	Accepted.
2-1039	A	71:14	:16	We agree with this statement that a lack of temporal equivalence does not invalidate the	Noted. This is as we see it already

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				GWP concept or provide any guidance to replace it, and suggest it be elevated to the summary of this chapter as a key point. [Govt. of United States of America (Reviewer's comment ID #: 2023-114)]	taken into the ES through the qualifying statement about GWPs being a appropriate metric.
2-1040	A	71:15		what source are you quoting? The phrase does not appear in the UNFCCC, as explained in comment #17 [Danny Harvey (Reviewer's comment ID #: 101-22)]	Accpted. Changed to offical UNFCCC terminology.
2-1041	A	71:17	71:17	O'Neill citation is missing from list. [Thomas Peter (Reviewer's comment ID #: 198-47)]	Accepted.
2-1042	A	71:18	71:22	Again, this appears to be more fitting for Working Group III, as well as for Chapter 18 of Working Group II. Suggest to delete. [European Commission (Reviewer's comment ID #: 2008-23)]	Accepted. Text shortened and rephrased.
2-1043	A	71:18	71:22	Please rephrase or delete the section "The cost difference between a multi-gas strategy …. .and a cost-optimal strategy (accounting for damage and mitigation costs)" given that the here stated "optimal" reference level implies an utilitarian welfare optimization paradigm . The cost-benefit optimization is only one of many possible approaches and has been challenged numerous times in regard to its applicability to the climate change problem. (cost-benefit asymmetry; market costs, non-market damages; assumes monetarization of all damages;).. For example, the UNFCCC did not choose a cost-benefit analysis approach, but rather a target-based approach to prevent dangerous anthropogenic interference with the climate system. Anyway, the here stated reasoning for GWPs should not and is not the reason for why many actors agreed on them under the Kyoto Protocol.. Thus, please shorten this and other sections to avoid arguing with economic theories within the WG 1 report. [Govt. of Germany (Reviewer's comment ID #: 2011-118)]	Accepted – cf. Comment 2-1042.
2-1044	A	71:18	71:22	These sentences engage in one side of economic debate in the field are inappropriate and take place in WGIII and hence need to be deleted. [William Hare (Reviewer's comment ID #: 99-14)]	Accepted – cf. Comment 2-1042.
2-1045	A	71:20	71:20	'shortcomings' is one word, not two [ian Enting (Reviewer's comment ID #: 63-7)]	Accepted.
2-1046	A	71:24	71:27	Their are several issues here which warrant either deletion of the sentence or a larger discussion here of the issues raised 1) sentence is correct to a point, provided that the reference to climate change includes "global" but at the regional level it is not correct 2) The reference to the "total potential of mitigating climate change " is a kind of gobbledy gook statement due to the very different implications of different choices of eg time horizon for calculating the GWPs and the shortl lifetime of the aerosols compared to the time horizons normally discussed. "On a global level the mean metric values can be used to give an indication of the total potential of mitigating climate change by including a	Taken into account – paragraph rephrased and made shorter.

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				certain forcing agent in climate policy (cf. Figure 2.28)." [William Hare (Reviewer's comment ID #: 99-15)]	
2-1047	A	71:24		The GWP concept was never intended to be applied to aerosols, and I don't know of any serious suggestion that it should be. If there are such suggestions, they should be cited, then you should explain why the GWP concept should not be applied to aerosols (namely, aerosol forcing is not the negative of GHG forcing as far as impacts are concerned, because of the very different responses of precipitation to aerosols and to GHGs, as has been established in a number of recent papers including one of my own). I would delete this entire paragraph. [Danny Harvey (Reviewer's comment ID #: 101-23)]	Taken into account – paragraph rephrased and made shorter.
2-1048	A	71:24	:40	This paragraph on the possibility of developing GWPs for short-lived species needs to be worded much more carefully to note fundamental differences between short-lived and long lived species in calculating GWP. Also, the concept of GWP as derived for radiative forcing is too simplistic to capture the climate effects of absorbing aerosols, including black carbon. Comparing greenhouse gas effect with absorbing aerosol requires the use of climate models that predict the actual impact on surface temperature. [Govt. of United States of America (Reviewer's comment ID #: 2023-115)]	Taken into account – paragraph rephrased and made shorter.
2-1049	A	71:26	71:28	This sentence is problematic in large part because what is discussed by the cited reference is not exactly congruent with its implications regarding "effective mitigations strategies" and the implied role of GWP metrics in this context (See abstract below). I would suggest rewording to something like "There are substantial cobenefits in relation to mitigation actions on greenhouse gases and air pollutants (Hansen and Sato (2004)) however the efficacy of the inclusion of short lived forcing agents into international agreements is not clear (Rypdal et al. (2005))" "Rypdal, K., T. Berntsen, et al. (2005). "Tropospheric ozone and aerosols in climate agreements: scientific and political challenges." Environmental Science & Policy 8(1): 29-43. In addition to the six greenhouse gases included in the Kyoto Protocol, the tropospheric ozone precursors CO NMVOC and NOx and the aerosols/aerosol precursors black carbon, organic carbon and SO2 also play significant roles in climate change. The aim of this paper is to review some of the main scientific and political challenges associated with incorporating tropospheric ozone and aerosol precursors into climate agreements, and to discuss how these challenges have a bearing on the design of future climate agreements. We argue that the optimal policy design for a particular substance depends on a combination of scientific and political concerns. We look particularly at regional climate effects, negative forcing, metrics (measuring climate effects against other gases on a common scale). political attractiveness. and verification and compliance. We systematically review the existing knowledge on these issues. explore their impact on policy design. and conclude that, with	Taken into account – paragraph rephrased and made shorter.

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				current scientific knowledge. CO and NMVOC could conceivably be included in a global climate agreement. either in a basket,with the long-lived greenhouse gases or in a separate basket. while NO, and aerosols might be regulated more appropriately through regional agreements with links to a global agreement. However, the complexity and fairness implications of including tropospheric ozone precursors and aerosols might negatively affect the political feasibility of a future agreement. (C) 2004 Elsevier Ltd. All rights reserved. [William Hare (Reviewer's comment ID #: 99-16)]	
2-1050	A	71:29	71:31	There is debate as whether or not GWPs should be used for short-lived species. This statement appears to assume that they can and should be used, without reference or further explanation. [European Commission (Reviewer's comment ID #: 2008-24)]	Taken into account – paragraph rephrased and made shorter.
2-1051	A	71:29	71:31	IMPORTANT: The applicability of GWPs to short-lived or cooling species is not justifiable, both from a scientific and policy perspective. Thus, please do not include sentences like this one that states ".... metric values for short-lived compounds vary significantly by region and time so that for operationalization on a decentralized level, robust regionally varying GWPs must be established and agreed upon". This statement assumes a) the desirability and b) the feasibility of GWPs for short-lived species. Rather, before jumping to conclusions that GWPs for short-lived species are feasible, please provide a solid analysis of the time and space-variability of the impacts of different short-lived compound emissions from different regions at different times. [Govt. of Germany (Reviewer's comment ID #: 2011-119)]	Accepted. Text carefully reworded
2-1052	A	71:29	71:31	Delete this sentence as it is policy prescriptive (eg presupposing the necessity and desirability of this) and the science does not seem to be clear yet (where are the papers and work to back this up or has this been made up in this chapter? Delete: "However, the metric values for short-lived compounds vary significantly by region and time so that for operationalization on a decentralized level, robust regionally varying GWPs must be established and agreed upon." [William Hare (Reviewer's comment ID #: 99-17)]	Accepted. Sentence deleted
2-1053	A	71:33	71:36	it is not correct that the GWP gives equal weight to the short term climate fluctuations from short-lived gases as to the long-term climate fluctuations from long lived gases. The weighting depends on the chosen time horizon, and is certainly not equal. The longer the time horizon, the less weight that will be given to the short-lived gases. [Danny Harvey (Reviewer's comment ID #: 101-24)]	Accepted, text deleted.
2-1054	A	71:37		the quoted phrase comes from Article 2 of the UNFCCC, but the source is not given [Danny Harvey (Reviewer's comment ID #: 101-25)]	Accepted, text deleted.

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2-1055	A	71:49	71:49	Add "(Bern2.5CC)" after "updated version of the Bern carbon-cycle model"; The name Bern2.5CC is used in chapters 6, 8, and 10 of IPCC-AR4 WG1. [Gian-Kasper Plattner (Reviewer's comment ID #: 200-2)]	Accepted.
2-1056	A	71:49	71:49	References to the "updated version of the Bern carbon-cycle model" are missing. Add (i) Plattner, G.-K., F. Joos, T. F. Stocker and O. Marchal, Feedbacks mechanisms and sensitivities of ocean carbon uptake under global warming, Tellus, 53B, 564-592, 2001 and (ii) Joos, F., I. C. Prentice, S. Sitch, R. Meyer, G. Hooss, G.-K. Plattner, S. Gerber and K. Hasselmann, Global warming feedbacks on terrestrial carbon uptake under the Intergovernmental Panel on Climate Change (IPCC) emission scenarios, Global Biogeochemical Cycles, 15(4), 891-907, 2001. [Gian-Kasper Plattner (Reviewer's comment ID #: 200-3)]	Rejected Extra references not needed – we already have two
2-1057	A	72:1		SECTION: 2.10.3 Indirect GWPs The presentation of calculation of a "global mean" GWP for shortlived forcing agents and those with cooling effects as though they can be uses as part of the well known basket approach appears to be highly problematic as it contains implicit judgements about the relevance of regional effects that are quite dominant and could involves substantial adverse regional consequences is applied in the kind of system implied here. The efficacy of the use of these within a basket approach in relation to say global warming targets is low or plain bad, as can be seen with simple calculations. Another issues is that the effects of emission changes of these forcing agents isis quite different from the well mixed greenhouse gases where all parties are affected more or less equally at all times by increases or decreases in emissions. Finally where is the science behind all these issues to be reviewed or is this made up here? I would strongly suggest a very different discussion here about this issue to deal with the questions raised abobve and strong consideration should be given to not including these GWP estimates at this stage. [William Hare (Reviewer's comment ID #: 99-18)]	Partly Rejected. This relates also to the last paragraph of Section 2.10.1. This paragraph has been modified to much less positive about GWPs for shortlived species. The introduction to Section 2.10.3 clearly states several caveats on the limitations of using the indirect GWPs of short-lived species in policy making. New findings since the TAR (cf. Section 2.8), have increased our confidence in linearity of the forcing response relationship
2-1058	A	72:15		Related to the previous comment, it is questionable to use short-lived species in calculations of global or regionally average GWP. Given the political implications of such calculations, the authors should reconsider these statements. [European Commission (Reviewer's comment ID #: 2008-25)]	Partly rejected. With the rephrasing of the last paragraph of Section 2.10.1, and the caveats in the first paragraph of Section 2.10.3, we believe this this provides a balanced presentation. In our opinion not mentioning short lived

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					GWPs at all is also policy prescriptive and will then fall short of not advancing the scientific state of affairs..
2-1059	A	72:15		IMPORTANT: Given its strongly regionally different impacts of the short-lived compounds emissions, it is not scientifically appropriate for the IPCC to give central GWP estimates that average across multiple regions, and different times. Unlike global mean temperatures and other global mean variables, the GWPs are a politically (sensitive) concept that implies that emissions from different gases can be compared on the same metric, no matter at what time of the day, time of the year, or in which nation they are emitted in. This is justified for long-lived gases, of which the impact on the climate system does not depend on its source location and timing. This fundamental premise is not given for short-lived emissions from CO, NMVOCs, NOx or other aerosol precursors. Thus, stating global average GWPs for highly heterogenous effects depending on the region and timing of the emission goes one step to far for a scientific report, as it implies a politically (sensitive) decision, namely that you actually can - for the purpose of an "decentralized"/global emission control system, assume those short-lived compound emissions to have a comparable effect, no matter when and where they are emitted. This assumptions flaws are obvious when assuming a an emission control system that would lump together regionally cooling aerosol species with globally warming long-lived GHGs. Even if the the theoretical net sum of "GWP-weighted" aerosol and GHG emissions were zero, the GHG's climate system impacts are not offset by regional aerosol cooling at all, as the forcing mechanisms and their climate impacts are just not comparable. Please revise the section 2.10.3 accordingly so that no globally and time-averaged GWPs are stated anywhere (e.g. CO, NVMOCs, NOx, aerosols). This implies as well that figure 2.28 has to be adapted in order to solely show GWPs of LLGHGs. [Govt. of Germany (Reviewer's comment ID #: 2011-120)]	Partly rejected. The text states: 'The indirect GWP will in many cases depend on the location and time of the emissions.' And 'Thus the usefulness of the global mean GWPs to inform policy decisions can be limited.' Could be severely limited. In addition to the changes made to the last paragraph of Section 2.10.1 this should provide a balanced view. Figure 2.28 will be removed and replaced by alternative in section 2.9.
2-1060	A	72:29	72:32	Unfortunately the straightforward position that CO2 produced by CH4 and CO emissions was included in the national inventories for CO2 has become more confused with the approval of the 2006 National Greenhouse gas Inventory Guidelines earlier this year. Several inventory experts have advised that the new guidelines will not include the CO2 that appears as an oxidation product of reduced species. I don't think that you should provide any detailed consideration of this issue which could easily be wrong until the new guidelines are fully understood. But you may wish to add a sentence to say something like "This issue may need to be reconsidered as inventory guidelines are revised." [Martin Manning (Reviewer's comment ID #: 155-1)]	Accepted. Text reworded
2-1061	A	72:39		You need a reference for the adjustment time of 10.8 years the one given in the Table	Take into account. Ch. 7 will include

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				footnote (c) appears wrong. I can find no mention of methane adjustment times in section 7.4.1, and a search of the PDF file reveals that the value of 10.8 years does not occur anywhere in the chapter. This also seems to be the case for the first draft of Ch07 as well. If the value comes from a literature reference, please cite the paper directly from your chapter. If it is meant to come from an assessment in Ch07 can you please double check this and provide a more specific cross-chapter reference. [Martin Manning (Reviewer's comment ID #: 155-2)]	this now.
2-1062	A	72:44	72:45	Maybe it can't be helped but this sounds a bit too convenient and a bit of a fudge. I can imagine a policymaker assuming that your approach was to fudge the adjustments to the ozone and stratospheric water effects in order to retain the value of 23. [Martin Manning (Reviewer's comment ID #: 155-3)]	Noted. It can't be helped
2-1063	A	73:14	73:23	Despite what the text said about not giving a GWP number for aircraft NOx. Several values are given in Table 2.15. If you do keep the numbers in Table 2-15, would some one please check them? If I take the Wild et al paper integrated forcing of 5.4 mW m-2 yr for 0.5 Tg(N) emission, and the AGWP for CO2 of .09 mW m-2 yr for 1 Tg(CO2), then the GWP for N emitted by aircraft as NO or NO2 should be around 110. Finally, the chapter should give a brief discussion of the uncertainties. [Malcolm Ko (Reviewer's comment ID #: 135-10)]	Taken into account. The text does not say that a GWP number for aircraft is not given, but that a central estimate is not given. We will double check the Wild et al. number.
2-1064	A	73:16	73:16	Its should be "the effect due to formation ..." [Govt. of Finland (Reviewer's comment ID #: 2009-33)]	Text no longer there
2-1065	A	73:27	73:28	Section 2.10.3.5. If your citation rules permit it, you should mention that these values are evaluated in the current ozone assessment (WMO, 2007). This document should be "in press" in the fall (2006). [John S. Daniel (Reviewer's comment ID #: 54-4)]	Rejected. We cannot cite this, as this is not formally available.
2-1066	A	73:41	74:38	The approach taken here to provide new and unpublished material on the GWPxemissions for short-lived gases and aerosols in this IPCC report is not appropriate, irrespective of the language regarding uncertainties that has been added since the FOD. Past IPCC reports have had careful discussion on the reasons why this could not be done so a departure from that would need careful justification and would have to be based on published work to substantiate it. The fundamental problem is that this discussion is doing 'science on the hoof' within the report because the approach taken and the numbers given are not based on a body of published literature addressing the problems. Unless the authors can provide an adequate list of literature references that substantiate the provision of GWPs for short lived gases and aerosols, and their uncertainties, this should be removed. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-16)]	Accepted. See also reply to comment 2-1067. However, we don't agree that we are doing 'Science on the hoof' as all all numbers are taken from published material (AEROCOM publications for the aerosols). We do accept that there are issuees concerning aerosols.
2-1067	A	73:41		SECTION: 2.10.4 Aerosols and aerosol precursors (As above): The presentation of	Accepted.

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				calculation of a "global mean" GWP for shortlived forcing agents and those with cooling effects as though they can be uses as part of the well known basket approach appears to be highly problematic as it contains implicit judgements about the relevance of regional effects that are quite dominant and could involves substantial adverse regional consequences is applied in the kind of system implied here. The efficacy of the use of these within a basket approach in relation to say global warming targets is low or plain bad, as can be seen with simple calculations. Another issues is that the effects of emission changes of these forcing agents isis quite different from the well mixed greenhouse gases where all parties are affected more or less equally at all times by increases or decreases in emissions. Finally where is the science behind all these issues to be reviewed or is this made up here? I would strongly suggest a very different discussion here about this issue to deal with the questions raised abobve and strong consideration should be given to not including these GWP estimates at this stage. [William Hare (Reviewer's comment ID #: 99-19)]	We acknowledge the politically sensitive nature of providing GWPs for short-lived (and cooling) species., and the fact that they may be misused to privide the basis for regional reduction in emissions. However, we still believe that there is useful information provided by figure 2.28. This information has now been presented as integrated RF over a time horizon of a one year pulse of current emissions (and carefully not stating what the corresponding global mean AGWP values are). The information has been removed from Section 2.10 (Metrics) and put in Section 2.9 (Synthesis)
2-1068	A	74:1	74:2	GWPs for BC and OC. The 100-year climate response per unit mass emission of f.f. BC+OM relative to that of CO2-C was estimated as 90-190 in Jacobson, M.Z., Correction to "Control of fossil-fuel particulate black carbon and organic matter," J. Geophys. Res., 110, D14105, doi:10.1029/2005JD005888, 2005. This parameter is analogous to a global warming potential. An application of the numbers is given at http://www.stanford.edu/group/efmh/fossil/fossil.html. [Mark Jacobson (Reviewer's comment ID #: 116-19)]	Taken into account.
2-1069	A	74:12	74:13	limited knowledge of atmospheric residence time of particles might also be a source of uncertainty [Govt. of Hungary (Reviewer's comment ID #: 2012-28)]	Noted. The statement mention removal of particles (which then determines the residence time).
2-1070	A	74:16	74:18	This illustrates the previous comment. This statement takes liberties in its interpretation of the Kyoto Protocol, as the KP does not include short-lived emissions. Therefore, a statement as to the consistency of a method with the KP is misleading and inappropriate. Please also delete and correct Figure 2.28. [European Commission (Reviewer's comment ID #: 2008-26)]	Accepted – cf. Reply to comments 2-1066 and 2-1067.
2-1071	A	74:16	74:18	IMPORTANT: The current text states "A simple method to compare future climate	Accepted – cf. Reply to comments 2-

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				impacts of current emissions is to multiply current emissions of all climate agents with their GWP 100 values to obtain equivalent CO2 emissions. This is consistent with the Kyoto Protocol through its adoption of GWPs with 100 years time horizon." Please revise that sentence as its current form is a non-justifiable political judgment as to what is and what is not consistent with the Kyoto Protocol. The Kyoto Protocol does - for good reason - not include the short-lived emissions, thus, IPCC cannot claim that comparing those short-lived emissions with LLGHGs is "consistent" with the Kyoto Protocol. For the same reason, the bottom half of Figure 2.28 is misleading as short-lived and long-lived emissions are compared on a - polically very sensitive - scale, GWP weighted emissions. The GWP scale is ultimately meant to lump emissions together. If short-lived gases are in the basket, this would be scientifically similarily inappropriate, as if the indirect negative stratospheric forcing were lumped together with the positive direct forcing by halocarbons, which e.g. Velders et al. (2005) refrains from doing, because these "are two distinct climate forcing mechanisms that do not simply offset one another". [Govt. of Germany (Reviewer's comment ID #: 2011-121)]	1066 and 2-1067.
2-1072	A	74:16	74:18	This sentence is too simplistic. The method might be simple but it does not uniquely and adequately "ompare future climate impacts of current emissions". It should no refer to "all climate agents" (where are the scenario calculations that back this assertion up?) "A simple method to compare future climate impacts of current emissions is to multiply current emissions of all climate agents with their GWP100 values to obtain equivalent CO2 emissions. 470 2-470 20 [William Hare (Reviewer's comment ID #: 99-121)]	Section removed.
2-1073	A	74:16	74:18	This sentence is wrong as it is NOT "consistent with the Kyoto Protocol": The KP does not count shortlived forcing agents and did not quite deliberately. [William Hare (Reviewer's comment ID #: 99-21)]	Section removed.
2-1074	A	74:23	74:26	I found this hard to understand. [Nathan Gillett (Reviewer's comment ID #: 84-96)]	Section removed.
2-1075	A	74:33	74:36	This is incomplete. Please reconsider in light of the above comments. [European Commission (Reviewer's comment ID #: 2008-27)]	Section removed.
2-1076	A	74:33	74:36	IMPORTANT: It is important to list the caveats of short-lived species GWPs, and please be comprehensive in this respect. Thus, not only the "location of the emission source", but as well the "timing of the emission source" is important (line 34) as well as "GWP variation by source timing" not only "region". Please revise sentences accordingly. [Govt. of Germany (Reviewer's comment ID #: 2011-122)]	Section removed.
2-1077	A	74:38		figure 28. "GtC yr-1" (not GtCO2) is a more usual unit of CO2 emissions. [Chris Jones (Reviewer's comment ID #: 120-15)]	Taken into account. Figure revised and units has changed.
2-1078	A	76:0	110:	references part: add following references that are cited in comments 1-9:	Some of these are added

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				[Hongbin Yu (Reviewer's comment ID #: 299-10)]	
2-1079	A	76:0	110:	Kaufman, Y.J., et al., 2005c: A critical examination of the residual cloud contamination and diurnal sampling impacts on MODIS estimates of aerosol over ocean, IEEE Trans. Geosci. Rem. Sens., 43(12), 2886-2897. [Hongbin Yu (Reviewer's comment ID #: 299-11)]	Noted
2-1080	A	76:0	110:	Martonichik, J.V., et al., 1998: Determination of land and ocean reflective, radiative, and biophysical properties using multiangle imaging, IEEE Trans. Geosci. Rem. Sens., 36, 1266-1281. [Hongbin Yu (Reviewer's comment ID #: 299-12)]	noted
2-1081	A	76:0	110:	Moody, E.G., M.D., King, S. Platnick, C.B. Schaaf, and F. Gao, 2005: Spatially complete global spectral surface albedos: value-added datasets derived from Terra MODIS land products, IEEE Trans. Geosci. Remote Sens., 43, 144-158. [Hongbin Yu (Reviewer's comment ID #: 299-13)]	noted
2-1082	A	76:0	110:	Zhou, M., H. Yu, R.E. Dickinson, O. Dubovik, and B.N. Holben, 2005: A normalized description of the direct effect of key aerosol types on oslar radiation as estimated from AERONET aerosols and MODIS albedos, J. Geophys. Res., 110, D19202, doi:10.1029/2005JD005909. [Hongbin Yu (Reviewer's comment ID #: 299-14)]	noted
2-1083	A	76:0	110:	Yu, H., R.E. Dickinson, M. Chin, Y.J. Kaufman, B.N. Holben, I.V. Geogdzhayev, and M.I. Mishchenko, 2003: Annual cucle of global distributions of aerosol optical depth from integration of MODIS retrievals and GOCART model simulations, J. Geophys. Res., 108(D3), 4128, doi:10.1029/2002JD002717. [Hongbin Yu (Reviewer's comment ID #: 299-15)]	noted
2-1084	A	76:0	110:	Yu, H., R.E. Dickinson, M. Chin, Y.J. Kaufman, M. Zhou, L. Zhou, Y. Tian, O. Dubovik, and B.N. Holben, 2004: The direct radiative effect of aerosols as determined from a combination of MODIS retrievals and GOCART simulations, J. Geophys. Res., 109, D03206, doi:10.1029/2003JD003914. [Hongbin Yu (Reviewer's comment ID #: 299-16)]	noted
2-1085	A	76:1		The reference list is not up to date. Some papers cited in the text are missing, and some papers included in the reference list are not cited in the text. [Govt. of Finland (Reviewer's comment ID #: 2009-34)]	Will be proof read
2-1086	A	76:1		References von Hoyningen-Huene, W., M. Freitag, and J. B. Burrows: Retrieval of aerosol optical thickness over land surfaces from top-of-atmosphere radiance. J. Geophys. Res., 108(2003), D9 4260, doi:10.1029/2001JD002018, 2003. Kokhanovsky, A.A., von Hoyningen-Huene, W., Bovensmann, H., Burrows, J.P.: The	Some added

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				determination of the atmospheric optical thickness over Western Europe using SeaWiFS imagery. IEEE Transactions on Geoscience and Remote Sensing 42 (2004) 4, 824 – 832 Lee, K.H., Kim, Y.J., von Hoyningen-Huene, W.: Estimation of regional aerosol optical thickness from satellite observations during the 2001 ACE-Asia IOP. JGR 109 D19S16, doi:10.1029/2003JD004126,2004. Lee, K.H. Kim, J.E., Kim, Y.J., Kim J, von Hoyningen-Huene, W.: Impact of smoke aerosol from Russian Forest Fires on atmospheric environment over Korea during May 2003. Atmospheric Environment, 39 (2005) 85-99. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-3)]	
2-1087	A	76:5	76:7	The Abel paper has appeared in ACP, update. [Thomas Peter (Reviewer's comment ID #: 198-35)]	updated
2-1088	A	77:36	77:36	Remove double comma. [Olivier Boucher (Reviewer's comment ID #: 27-37)]	accepted
2-1089	A	78:56		Papers cited here to be added to the references for Chapter 2. Jeong, M.J , Z. Li, D.A. Chu, and S-T. Tsay, 2005: Quality and Compatibility Analyses of Global Aerosol Products Derived from the Advanced Very High Resolution Radiometers and the Moderate Imaging Spectroradiometer, J. Geophy. Res., 110, D10S09, doi:10.1029/2004JD004648 Li, Z., and L. Kou, 1998: Atmospheric direct radiative forcing by smoke aerosols determined from satellite and surface measurements, Tellus (B), 50, 543-554. Li, Z., A. Trishchenko, 2001: Quantifying the uncertainties in determining SW cloud radiative forcing and cloud absorption due to variability in atmospheric condition, J. Atmos. Sci., 58, 376-389. Taubman, B.A., L. Marufu, B. Vant-Hull, C. Piety, B. Doddridge, R. Dickerson, Z. Li, 2004: Smoke Over Haze: Aircraft Observations of Chemical and Optical Properties and the Effects on Heating Rates and Stability. J. Geophy. Res., 109, D02206, doi: 10.1029/2003JD003898. Vant-Hull, B., Z. Li, B.F. Taubman, R. Levy, L. Marufu, F.L. Chang, B.D. Doddridge, R.D. Dickerson, 2005: Smoke over Haze: Comparative Analysis of Satellite, Surface Radiometer and Airborne In-situ Measurements of Aerosol Optical Properties and Radiative Forcing over the Eastern US, J. Geophy. Res., D10S21, doi:10.1029/2004JD004518. [Zhanqing Li (Reviewer's comment ID #: 147-8)]	Some of these added
2-1090	A	80:56	80:56	T. Tsushima" -> "Y. Tsushima [Yoko Tsushima (Reviewer's comment ID #: 269-1)]	accepted
2-1091	A	82:34		Please add: Diehl, K., and S. Wurzler, 2004: A freezing module for heterogeneous drop	No reason given - rejected

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				freezing in immersion mode. J. Atmos. Sci., 61, No 15, 2063-2073 [Sabine Wurzler (Reviewer's comment ID #: 296-3)]	
2-1092	A	83:30	83:31	Omit "Eichel et al." It is not cited in the text. [Sabine Wurzler (Reviewer's comment ID #: 296-2)]	accepted
2-1093	A	84:39	84:39	delta did not print well in my version. [Olivier Boucher (Reviewer's comment ID #: 27-40)]	noted
2-1094	A	85:18	85:19	Gedney et al. was published in 2006. I suspect the volume number is incorrect. [Olivier Boucher (Reviewer's comment ID #: 27-38)]	Reference corrected
2-1095	A	85:53	:54	Correct reference: Gray, L J, J D Haigh and R G Harrison 2005 Review of the influences of solar changes on the Earth's climate. Hadley Centre Technical Note 62, Met Office, Exeter [Joanna Haigh (Reviewer's comment ID #: 95-6)]	Ref corrected
2-1096	A	87:22	87:22	remove the period before Haywood. [Olivier Boucher (Reviewer's comment ID #: 27-41)]	Accepted
2-1097	A	91:3	91:5	The font size is wrong for this reference. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-58)]	Font corrected
2-1098	A	91:15	91:18	Separate "Kernthaler et al." from "Keppler et al." [Govt. of Finland (Reviewer's comment ID #: 2009-35)]	seperated
2-1099	A	91:16	91:16	There should be a new line starting with "Kernthaler" [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-59)]	accept
2-1100	A	91:17	91:17	An error. "Kernthaler" should move to the next line [VINCENT GRAY (Reviewer's comment ID #: 88-273)]	accept
2-1101	A	91:28	91:34	The author list contains several errors: a) Horowitz is misspelled; b) A. Jones is not a co-author of this paper; c) A. Kirkevåg is missing; d) J. E. Kristjansson is missing; e) Krol is misspelled; f) D. Roberts is not a co-author. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-60)]	corrected
2-1102	A	91:34	91:34	The volume should be 6, and the page numbers are 1815-1834. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-61)]	accept
2-1103	A	92:6	92:7	The following reference, cited on page 121, is missing: Kristjansson, J. E., 2002: Studies of the aerosol indirect effect from sulfate and black carbon aerosols. J. Geophys. Res., 107, 4246, 10.1029/2001JD000887. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-62)]	accept
2-1104	A	92:8	92:9	The following reference, cited on page 121, is missing: Kristjansson, J. E., T. Iversen, A. Kirkevåg, Ø. Seland, and J. Debernard, 2005: Response of the climate system to aerosol direct and indirect forcing - the role of cloud feedbacks. J. Geophys. Res., 110, D24206, doi:10.1029/2005JD006299.	accept

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				[Jón Egill Kristjánsson (Reviewer's comment ID #: 136-63)]	
2-1105	A	92:44	92:45	The author's name is mis-spelled: it should be "MacFarling Meure". The font is also too small. [Keith Lassey (Reviewer's comment ID #: 140-8)]	accept
2-1106	A	95:41	95:41	Spelling correction: MacFarling Meure [Govt. of Australia (Reviewer's comment ID #: 2001-169)]	accept
2-1107	A	95:41	95:42	Macfraling Meure phd is available on line, (from cmar.csiro.au) and URL should be given (as well as 'in press' GRL paper if this meets cutoff). Because of the extensive citing of PhD, (and the fact that it is available) it should remain as a reference since short GRL paper is not adequate substitute. [ian Enting (Reviewer's comment ID #: 63-8)]	accept
2-1108	A	95:41	95:42	The abbreviation of the journal name "Environmental Sciences" is presumably "Environ. Sci.". [Keith Lassey (Reviewer's comment ID #: 140-9)]	accept
2-1109	A	95:41	95:42	The thesis title is wrong. It should be "The variation of atmospheric carbon dioxide, methane and nitrous oxide during the Holocene from ice core analysis" (eg, see http://www.lib.unimelb.edu.au/collections/earth/thesesmr.html) [Keith Lassey (Reviewer's comment ID #: 140-10)]	accept
2-1110	A	95:43	95:43	Insert: MacFarling Meure, C., Etheridge, D., Trudinger, C., Steele, P., Langenfelds, R., van Ommen, T., Smith, A. and Elkins, J. The Law Dome CO2, CH4 and N2O Ice Core Records Extended to 2000 years BP. Geophysical Research Letters, in press, 2006. [Govt. of Australia (Reviewer's comment ID #: 2001-170)]	accept
2-1111	A	96:1	96:3	The font size is wrong for this reference. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-64)]	accept
2-1112	A	96:4	96:4	This line is blank. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-65)]	accept
2-1113	A	101:21	101:21	Breon should read Bréon. Météorologie. [Olivier Boucher (Reviewer's comment ID #: 27-36)]	accept
2-1114	A	102:23	102:24	The following reference, cited on page 121, is missing: "Rasch, P. J., and J. E. Kristjánsson, 1998: A comparison of the CCM3 model climate using diagnosed and predicted condensate parameterizations. J. Climate, 11, 1587-1614." [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-75)]	accept
2-1115	A	104:1		Add "Sakai, T., Nagai, T., Nakazato, M., and Matsumura, T., 2004. Raman lidar measurement of water vapor and ice clouds associated with Asian dust layer over Tsukuba, Japan. Geophys. Res. Lett. 31, L06128, doi:10.1029/2003GL019332." in the list.	Rejected –no reason given

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				[Masao Mikami (Reviewer's comment ID #: 177-4)]	
2-1116	A	104:7		Add "Sassen, K., 2005. Dusty ice clouds over Alaska, Nature, 434, 456." in the list. [Masao Mikami (Reviewer's comment ID #: 177-5)]	accept
2-1117	A	105:12		There should be a middle initial A. in the name G. Schmidt here. [Drew Shindell (Reviewer's comment ID #: 235-2)]	accept
2-1118	A	106:26	106:28	Stier, P. , J. Feichter, S. Kinne, S. Kloster, E. Vignati, J. Wilson, L. Ganzeveld, I. Tegen, M. Werner, Y. Balkanski, M. Schulz, O. Boucher, A. Minikin, and A. Petzold, 2005: The aerosol-climate model ECHAM5-HAM. Atmos. Chem. Phys., 5, 1125-1156. [Marco A. Giorgetta (Reviewer's comment ID #: 85-1)]	accept
2-1119	A	107:1	107:1	Kristjanssion" should be "Kristjansson [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-66)]	accept
2-1120	A	107:54	108:2	The author list contains several errors: a) The order of Guibert and Kinne should be reversed; b) Y. Balkanski is missing in front of S. Bauer; c) R. Easter is missing; d) Horowitz is misspelled; e) P. Huang is missing; f) S. Kloster is missing; g) A. Kirkevåg is missing; h) J. E. Kristjansson is missing; i) Tie is misspelled. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-67)]	accept
2-1121	A	108:2	108:2	The title of the paper is wrong. The correct title is: "Analysis and quantification of the diversities of aerosol life cycles within AeroCom". [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-68)]	accept
2-1122	A	108:2	108:2	The volume should be 6, and the page numbers are 1777-1813. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-69)]	accept
2-1123	A	108:35		Insert "Uchiyama, A., Yamazaki, A., Togawa, H., and Asano, J., 2005. Characteristics of Aeolian dust observed by sky-radiometer in the ADEC Intensive Observation Period 1(IOP1). J. Met. Soc. Japan. 83A, 291-305" in the list. [Masao Mikami (Reviewer's comment ID #: 177-2)]	Accepted
2-1124	A	110:3	110:6	Omit [Sabine Wurzler (Reviewer's comment ID #: 296-4)]	accepted
2-1125	A	110:13	110:14	Is the Yu (2002) reference used at all in this chapter? Remove if not. [Gareth S. Jones (Reviewer's comment ID #: 121-124)]	accepted
2-1126	A	111:1	111:1	Replace "climate change" by change of climate". The term "climate change" is defined differently by the FCCC and IPCC and its use is confusing 413 2-413 277 [VINCENT GRAY (Reviewer's comment ID #: 88-124)]	Rejected. Not confusing in this context
2-1127	A	111:4	111:13	[From David Wratt - David Fahey is an LA for this Chapter]. (Formatting) This section should be italicised [David Wratt & David Fahey (Reviewer's comment ID #: 67-97)]	Accepted.
2-1128	A	111:4	111:4	Replace "climate change" by change of climate". The term "climate change" is defined	Rejected. Not confusing in this

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				differently by the FCCC and IPCC and its use is confusing 410 2-410 274 [VINCENT GRAY (Reviewer's comment ID #: 88-97)]	context.
2-1129	A	111:5	111:5	Insert after "atmosphere" ,"and changes to the climate of cities and other human occupation from buildings and energy production" [VINCENT GRAY (Reviewer's comment ID #: 88-278)]	Rejected. Not needed in this context.
2-1130	A	111:5	111:5	Insert after "known", "greenhouse gas" [VINCENT GRAY (Reviewer's comment ID #: 88-279)]	Rejected. The contribution is largest of all terms, not just of GHGs.
2-1131	A	111:7	111:7	Delete "change" The term "climate change" is defined differently by the FCCC and IPCC and its use is confusing, so avoid it [VINCENT GRAY (Reviewer's comment ID #: 88-275)]	Rejected. Not confusing in this context.
2-1132	A	111:10	111:10	Replace "climate change" by "change of climate". The term "climate change" is defined differently by the FCCC and IPCC and its use is confusing [VINCENT GRAY (Reviewer's comment ID #: 88-276)]	Rejected. Not confusing in this context.
2-1133	A	111:10	111:10	Insert before "is" "within urban arfeas" [VINCENT GRAY (Reviewer's comment ID #: 88-280)]	Rejected. Not a necessary distinction in this context.
2-1134	A	111:11	111:11	Insert aftwr "contrtibution",", largely confined to urban areas,"417 2-417 281 [VINCENT GRAY (Reviewer's comment ID #: 88-280)]	Rejected. Not correct in this context.
2-1135	A	111:12	111:12	Insert after "discuss" "some of" [VINCENT GRAY (Reviewer's comment ID #: 88-282)]	Accepted.
2-1136	A	111:12	111:12	Delete "change". The term "climate change" is defined differently by the FCCC and IPCC and its use is confusing [VINCENT GRAY (Reviewer's comment ID #: 88-283)]	Rejected. Not confusing in this context.
2-1137	A	111:23	111:23	Insert the following sentence at the end of this line: Carbon dioxide is also released in natural processes. [Wilmer Anderson (Reviewer's comment ID #: 5-28)]	Accepted.
2-1138	A	111:25	111:26	The statement that "currently methane abundances are decreasing in the atmosphere" is not consistent with section 2.3.2. The growth rate of methane abundance has decreased, and the concentration may even have stabilized, but it is not really decreasing. [Govt. of Finland (Reviewer's comment ID #: 2009-36)]	Accepted.
2-1139	A	111:31	111:31	Insert the following sentence at the end of this line: These refigeration agents are no longer in use so that their concentrations will decrease in time. [Wilmer Anderson (Reviewer's comment ID #: 5-29)]	Accepted.
2-1140	A	111:40		[From David Wratt - David Fahey is an LA for this Chapter]. Add the word "the" after "reduced", ie … accumulation of halocarbons in the atmosphere has reduced THE stratospheric ozone layer … [David Wratt & David Fahey (Reviewer's comment ID #: 67-95)]	Accepted. Corrected by deleting the word 'layer' instead.

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2-1141	A	111:45	111:45	Insert after "activities" "are often assumed to have (but withoit much evidence)" [VINCENT GRAY (Reviewer's comment ID #: 88-284)]	Rejected. In this context, 'assumed' understates the confidence in this assertion.
2-1142	A	111:52	53:11	Please add state of mixture and morphology/shape. [Caroline Leck (Reviewer's comment ID #: 144-23)]	Rejected. Suggested additional detail is not needed in this context.
2-1143	A	111:52		[From David Wratt - David Fahey is an LA for this Chapter]. Suggest slight wording change here to help inform non-technical readers: AerosolS ARE SMALL PARTICLES WHICH are present in the atmosphere with widely varying size … [David Wratt & David Fahey (Reviewer's comment ID #: 67-96)]	Accepted.
2-1144	A	111:55	111:55	Insert the following phrase: …organic compounds, and black carbon (black carbon is the soot that results from burning organic materials). … [Wilmer Anderson (Reviewer's comment ID #: 5-30)]	Accepted. Added the word 'soot'.
2-1145	A	111:57	111:57	Add at end "Ordinary clouds are a particularly important form of aerosol" [VINCENT GRAY (Reviewer's comment ID #: 88-285)]	Rejected. Introducing clouds here confuses the discussion.
2-1146	A	112:4	112:4	Delete "change". The term "climate change" is defined differently by the FCCC and IPCC and its use is confusing. So avoid it [VINCENT GRAY (Reviewer's comment ID #: 88-286)]	Rejected. Not confusing in this context.
2-1147	A	112:6	112:6	Delete "change". The term "climate change" is defined differently by the FCCC and IPCC and its use is confusing. So avoid it [VINCENT GRAY (Reviewer's comment ID #: 88-287)]	Rejected. Not confusing in this context.
2-1148	A	112:9	112:9	Delete "change". The term "climate change" is defined differently by the FCCC and IPCC and its use is confusing. So avoid it [VINCENT GRAY (Reviewer's comment ID #: 88-288)]	Rejected. Not confusing in this context.
2-1149	A	112:21	112:21	Delete "change". The term "climate change" is defined differently by the FCCC and IPCC and its use is confusing. So avoid it [VINCENT GRAY (Reviewer's comment ID #: 88-289)]	Rejected. Not confusing in this context.
2-1150	A	112:22		[From David Wratt - David Fahey is an LA for this Chapter]. Suggest some rewording as follows: The contribution to radiative forcing from SOME of the agents influenced DIRECTLY by human activities are shown …. (Reasons for suggestions: "Some" rather than "each" since figure doesn't include aerosols; addition of "directly" since figure doesn't include water vapour. [David Wratt & David Fahey (Reviewer's comment ID #: 67-98)]	Accepted.
2-1151	A	112:37	112:37	Add at end "Human activities also alter the temperature in and near cities from building and energy production, causing an upwards bias to global surface temperature" [VINCENT GRAY (Reviewer's comment ID #: 88-290)]	Rejected. This FAQ does not address consequences of climate forcing from human activities.
2-1152	A	112:41	112:44	I suggest to delete this paragraph about contrails, since contrail direct RF (0.01 W/m2) is	Rejected. Although your point is well

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				about two orders of magnitude smaller than other anthropogenic forcings (such as CO2). [Michael Danilin (Reviewer's comment ID #: 55-26)]	taken, contrails are included here in order to maintain consistency with the primary radiative forcing figure (Fig. 2.24).
2-1153	A	112:43	112:44	FAQ 2.1 states that "global aircriaft operations are estimated to have increased Earth's cloudiness, which causes a small positive radiative forcing." But, Ch. 2 (Ex. Sum., page 6, lines 14-17) state: "The global effect of aviation aerosol on background *cloudiness remains unknown* and a best estimate remains unavailable for the RF of total cloudiness changes (contrails, induced cirrus cloudiness, and aerosol effects) caused by subsonic aircraft operations, these other effects have a very low level of scientific understanding." And, Ch. 2, page 51 (lines 23-27) states: "The global RF values for contrail and induced cloudiness are assumed to vary linearly with global fuel use if aircraft flight regions remain unchanged. The best estimate for the RF of persistent linear contrails for aircraft operations in 2000 is 0.010 W m–2 (Table 2.9; Sausen et al., 2005). The value is based on independent estimates derived from Myhre and Stordal (2001) (0.015 W m–2) and Marquart et al. (2003) (0.006 W m–2). The two values also serve to set the uncertainty range of a factor of 2." The statement in the FAQ seems inconsistent with Ch. 2 Ex. Sum. and page 51. [WG1 TSU (Reviewer's comment ID #: 285-3)]	Accepted. As stated, the text in the Exec. Summ, main text, and FAQ 2.1 are consistent and correct. However, confusion easily arises over the fact that persistent contrails are only one part of aviation induced cloudiness, and the only part that can be quantified. The FAQ text has been simplified to discuss only persistent contrails, which are included in Fig. 2-24.
2-1154	A	112:47	112:47	Insert after "changes" "ocean circulation changes (El Niñ0 and La Niña)," [VINCENT GRAY (Reviewer's comment ID #: 88-291)]	Rejected. This FAQ does not include potential consequences of climate change, only the forcing terms associated with human activities.
2-1155	A	112:52	112:53	[From David Wratt - David Fahey is an LA for this Chapter]. I'm not sure it is wise to say "The atmosphere is currently free of Volcanic aerosol" - This is true at present, but might not be true by the time the AR4 is published and printed. Safer to say something like: In 2006 the stratosphere was free of volcanic aerosol because ...". 119 2-119 99 [David Wratt & David Fahey (Reviewer's comment ID #: 67-291)]	Accepted, but revision handled differently.
2-1156	A	113:0		Table 2.1. For CFC-11 the change in RF since 1998 should be negative. For methyl chloroform and CF4 the changes in concentrations and RF since 1998 should be included, since data are readily available. It would be preferable to define "Other Kyoto gases" as HFCs+PFCs+SF6 [Govt. of Belgium (Reviewer's comment ID #: 2003-8)]	Accpeted
2-1157	A	113:3	113:3	ALL the confidence figures in Table 2.1 MUST BE DOUBLED to reflect 95% confidence levels [VINCENT GRAY (Reviewer's comment ID #: 88-292)]	Uncertanities are 5-95%
2-1158	A	113:7	113:7	Replace "standard deviation" by "two standard deviations (to show 95% confidence	Uncertanities are 5-95%

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				levels)" [VINCENT GRAY (Reviewer's comment ID #: 88-293)]	
2-1159	A	113:7	113:7	Insert "two" after "including" and put "s" on "standard deviation 430 2-430 294 [VINCENT GRAY (Reviewer's comment ID #: 88-293)]	Uncertanities are 5-95%
2-1160	A	113:8	113:8	Plural of "uncertainties" [VINCENT GRAY (Reviewer's comment ID #: 88-295)]	corrected
2-1161	A	114:0		Table 2.2: It would be helpful define symbols like tau, Å and DRE in the table caption. [Govt. of Finland (Reviewer's comment ID #: 2009-37)]	Accepted. Now included.
2-1162	A	114:0		Table 2.2, references for MODIS: "Kaufman et al. (2005)" should be "Kaufman et al. (2005a)". Also add "Spinhirne et al. (2005)" for GLAS reference. [Hongbin Yu (Reviewer's comment ID #: 299-8)]	Accepted.
2-1163	A	116:0		Table 2.3, caption: "Yu et al. (2005)" should be "Yu et al., 2006" [Hongbin Yu (Reviewer's comment ID #: 299-9)]	Accepted.
2-1164	A	116:2	116:2	Yu et al., 2006 instead of 2005 [Claudia Marcolli (Reviewer's comment ID #: 158-22)]	Accepted.
2-1165	A	117:0	117:0	Table 2.4, data for Model No. I, GATORG from Jacobson (2001), are incorrect. Below are the correct numbers: LOAD (mg SO4 m-2): 3.06 RF (W/m2) (tropopause) -0.32 NRFM (W/g) -105 The load is determined from the paper as 2.23 Tg SO42- (total anth+nat tropospheric SO4) x 1.96 mg/m2/Tg x 0.7 anth SO4 / (anth+nat SO4) = 3.06 mg-SO4 m-2. [Mark Jacobson (Reviewer's comment ID #: 116-20)]	Accepted. Corrected.
2-1166	A	117:0		Table 2.4: tau_aer in the caption, t_aer in the first row of the table [Govt. of Finland (Reviewer's comment ID #: 2009-38)]	Accepted. Ammended.
2-1167	A	118:0	118:0	Table 2.5, the LOAD of POM (mgPOM m-2) for GATORG from Jacobson (2001) should be 1.70 [Mark Jacobson (Reviewer's comment ID #: 116-21)]	Rejected. This is at odds with the numbers that are given in your next comment. We will use the numbers in your next comment as they seem consistent with the 2001 Jacobson nature paper
2-1168	A	118:0	118:0	Table 2.5, direct forcing results for BC from the paper Jacobson, M. Z., Strong radiative heating due to the mixing state of black carbon in atmospheric aerosols, Nature, 409, 695-697, 2001	Accepted.

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				are missing. Below are the results Load POM Load BC RFBC RFFFBC (mg/m2) (mg/m2) (W/m2) (W/m2) 2.55 0.39 0.55 0.27 recommended (multiple distributions) 2.55 0.39 0.62 0.31 not physical (well-mixed internal mixture) 2.55 0.39 0.31 0.15 not likely (pure external mixture) [Mark Jacobson (Reviewer's comment ID #: 116-22)]	
2-1169	A	119:0	119:0	Table 2.6, the following information for model E, GATOR (Jacobson, 2001) is missing: LOAD Cloud Cover Surface Forcing Atmospheric Forcing (mg/m2) (%) all sky all sky (W/m2) (W/m2) *6.4 61.9 -2.5 2.38 *The load number shown excludes soil dust, a portion of which was considered anthropogenic in the paper. [Mark Jacobson (Reviewer's comment ID #: 116-23)]	Accepted.
2-1170	A	120:0	122:	Table 2.7 is difficult to read. It might be beneficial to add horizontal lines between the different papers [Claudia Marcolli (Reviewer's comment ID #: 158-23)]	Accpeted – table redrawn
2-1171	A	123:0		Table 2.8: Ramankuttty and Foley (1999): Is the CO2 RF really negative (-0.40 Wm-2)? [Govt. of Finland (Reviewer's comment ID #: 2009-39)]	ACCEPT. This will be corrected.
2-1172	A	123:1	123:1	"land cover change" is conventionally used for natural processes, "land use change" for anthropogenic effects. [Thomas Peter (Reviewer's comment ID #: 198-17)]	NOTED. Use of these terms will be examined.
2-1173	A	124:1	124:10	The extrapolation to 2004 from 2000 assumes a 13% increase in fuel use by aviation. This is not supported by the data. Using the calculated inventory data from FAA publication FAA-EE-005-02 (http://www.faa.gov/about/office_org/headquarters_offices/aep/models/sage/media/FAA-EE-2005-02__SAGE-Inventory_Report-Text.pdf), global fuel use increased from 2000 to 2004 by a total of 4% with declines in 2001-03 relative to 2000. Similar data in the US Department of Energy International Energy Annual shows a decline in jet fuel consumption from 2001-2 relative to 2000. Data is not available from that source for 2003-2004. (see http://www.eia.doe.gov/pub/international/iea2003/table35.xls for 2002 data, http://tonto.eia.doe.gov/FTPROOT/international/021901.pdf for 2001 data (look at jet fuel in Table 3.5), and http://tonto.eia.doe.gov/FTPROOT/international/021900.pdf for 2000 jet fuel data (look at Table 3.5).	Accepted. 2004 estimates removed.

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				[Steven Baughcum (Reviewer's comment ID #: 16-5)]	
2-1174	A	124:2	124:3	Table 2.9: I suggest to drop the last column of this Table showing results for 2004 and related footnote (e) because the assumption of the 13% increase of aviation fuel burn compared with that in 2000 is wrong! The September 11, 2001 event and SARS strongly reduced aviation growth from 2000 to 2004. While I don't know exact values of the aviation fuel burn in 2004, the latest available inventory for 2002 shows that aviation fuel consumption in 2002 was smaller than in 2000. Hence, it is extremely unlikely that fuel burn grew more than 14% from 2002 to 2004. I do not see how this last column of Table 2.9 adds any value to the discussion in Chapter 2 and strongly advice to drop it and the related footnote (e) avoiding to be criticized later for the wrong 13% growth assumption in this valuable IPCC document. [Michael Danilin (Reviewer's comment ID #: 55-28)]	Accepted. 2004 estimates removed from Table 2.9.
2-1175	A	125:3		The second column of table would be easier to read if each sentence started with a capital letter and ended with a full stop. [Joanna Haigh (Reviewer's comment ID #: 95-7)]	Accepted
2-1176	A	125:7	:8	The reference to the solar cycle is somewhat confusing. I suggest replacing ", and increases the net radiative forcing in the table by an additional 0.09 W m-2.." by ". Thus during contemporary cycle maxima the forcing is 0.09 W m-2 larger than the values shown in the Table." [Joanna Haigh (Reviewer's comment ID #: 95-8)]	Accepted
2-1177	A	126:0		Table 2.11, Stratospheric water vapour from other: asserting theat the consensus here is so low undermines the 25 authors and 75 refrees of the SPARC report on this subject. [Howard K. Roscoe (Reviewer's comment ID #: 219-13)]	Rejcted. This table refers to attribution of trends to humans, not the trends themselves
2-1178	A	126:2	127:4	I suggest to drop column "Consensus" from Table 2.11 because it adds no value to this Table and just duplicates LOSU results, since consensus 1,2,3 correspond to LOSU high, medium, and low/v.low, respectively. 93 2-93 24 [Michael Danilin (Reviewer's comment ID #: 55-13)]	Rejected. Posistion is made clear that LOSU is mupliplier of E and C.
2-1179	A	127:0	127:0	Another effect to consider is the effect of black carbon aerosol inclusions within cloud drops and ice crystals on cloud absorption: Jacobson, M.Z., Effects of absorption by soot inclusions within clouds and precipitation on global climate, J. Phys. Chem., in press, 2006, www.stanford.edu/group/efmh/jacobson/soot_incl_clouds.htm. [Mark Jacobson (Reviewer's comment ID #: 116-24)]	Not discussed as a forcing within main body, so not considered
2-1180	A	128:0	128:0	Table 2.12. The recommended value of the direct forcing of anthropogenic aerosol particles of -0.5 W/m2 appears arbitrary. The average among all models is close to -0.2 W/m2. The justification for the larger negative recommendation is that "observation"	Rejected. The expected direct forcing is likely more negative than the current mean model estimate as

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				studies provide larger forcings. However, it is not possible for observations to provide direct forcings since they do not sample the same location and time twice. Differences in irradiances or optical depths due to aerosols at different times or locations from observations are often due to differences in meteorology (e.g., RH) rather than differences in loading, so it is difficult even to determine whether an atmosphere is clean versus polluted based on optical depth. [Mark Jacobson (Reviewer's comment ID #: 116-25)]	explained in the corresponding text. Most models miss anthropogenic nitrate and dust. Observation based estimates on the other hand can be viewed as rather independent methods to retrieve RF and should be taken into account for given a best estimate of RF. There is currently no conclusive study in the literature explaining the differences between observation based and model based RF estimates. Fine mode optical depth has been shown to be correlated with "polluted air" and there is no reason to believe that meteorology driven factors alone can explain the global distribution of fine mode aerosol optical depth as observed by satellites and used for estimating anthropogenic RF based on these.
2-1181	A	128:0	128:0	Table 2.12. The recommended value for the direct forcing of fossil-fuel black carbon (+0.2) is skewed by a preponderance of models that do not treat the internal mixing of black carbon and have a variety of other simplifications. The most detailed calculation of the direct forcing of BC, which accounted for the evolution of its mixing state among 16 size distributions, was not even included in the table of results (please see comment to Table 2.5, above). A direct forcing of fossil-fuel BC of +0.25 W/m2 would seem to be more consistent with this result and that of other detailed studies (e.g., Liao and Seinfeld, 2005). [Mark Jacobson (Reviewer's comment ID #: 116-26)]	Rejected. A fair amount of models have investigated internal mixtures. At the same time major differences among BC RF estimates arise from assumptions on the vertical distribution of absorbing aerosols and clouds. Recent work also demonstrated reduced BC life time when BC was transfered to soluble internally mixed particles. Covariation of lifetime effects, cloud-BC interactions and optical model assumptions had not been explored in a systematic manner at the time of preparing this assessment to conclude that certain model results underestimate the BC RF. A large uncertainty is resulting

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					and documented in the report, suggesting that a BC FF estimate of 0.2 describes current knowledge with sufficient accuracy. The recommended BC RF from the missing reference Jacobson 2001 resembled the one given in Jacobson, M.Z., 2001: Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols. J. Geophys. Res., 106, 1551-1568, and was originally thus omitted. However the reference is now changed for the table following the reviewers suggestion in comment 1168.
2-1182	A	128:0	128:0	Table 2.12. The recommended value for the forcing black carbon due to its effect on snow albedo (+0.1 W/m2) should be closer to +0.06 W/m2 due to an error in the estimate of the direct forcing due to this effect on P. 2-49, lines 21-23 of the present IPCC report. [Mark Jacobson (Reviewer's comment ID #: 116-27)]	Reejcted. No evidence for 0.06 in paper. We disgree with comment on P49. In fact all reference to Jacobson's work in BC section will be dropped. RF estimate stands based on Hansen work giving upper bound..
2-1183	A	128:0		Table 2.12. This table is titled "Global mean radiative forcing (1750-2004)". Presumably the columns labeled SAR and TAR refer however to changes between 1750 and some date prior to 2004. There is a slight discrepancy between the RFs for N2O given in Tables 2.1 and 2.12 [Govt. of Belgium (Reviewer's comment ID #: 2003-9)]	N2O forcing in tables corrected and Table title chaged
2-1184	A	128:0		Table 2.12; the range of values cited for aviation induced cirrus does not appear to be consistent with Table 2.9. This needs to be reconciled. [Govt. of United States of America (Reviewer's comment ID #: 2023-116)]	Table corrected
2-1185	A	128:1	128:1	All uncertainties in Table 2-12 MUST BE doubled to reflect 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-296)]	90% confidence limits now used
2-1186	A	129:14	129:14	Table 2.13 note (e): "2.3.7" should be "2.3.6" [William Collins (Reviewer's comment ID #: 45-7)]	accepted
2-1187	A	129:15	129:15	Table 2.1.3 note (e): "Ramaswamy et al." should be "Prather et al." (page 269 is chapter 4, not chapter 6 of IPCC TAR). [William Collins (Reviewer's comment ID #: 45-8)]	accepted

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2-1188	A	129:16	129:16	Table 2.1.3 note (f): The value for nitrate aerosol forcing given in Table 2.12 is -0.10, whereas here it is -0.15. Is this wrong, or does it include an indirect effect? If so, this should be explained. [William Collins (Reviewer's comment ID #: 45-9)]	Tables clarified
2-1189	A	129:19	129:19	Should not foonote "i" refer to Table 2.1, rather than Table 2.12? (There is no separate value for HFCs+PFCs+SF6 in Table 2.12) [Govt. of Belgium (Reviewer's comment ID #: 2003-10)]	Accepted - yes
2-1190	A	130:0	130:	Table 2.14. It is not correct to call CCl4, CH3Br and CH2BrCl "Halons" (better: tetrachloromethane, bromomethane and bromochloromethane) [Govt. of Belgium (Reviewer's comment ID #: 2003-12)]	accepted
2-1191	A	130:0		Table 2.14: Should SAR(100) be TAR(100)? [Govt. of Finland (Reviewer's comment ID #: 2009-40)]	accepted
2-1192	A	130:0		Table 2.14 Since compounds such as perfluoroalkanes and hydrofluoropolyethers are included in this table, the list of compounds should also include perfluoropolyethers. Data on a representative compound from this class of materials is provided in Young, C.J., M.D. Hurley, T.J. Wallington, and S.A. Mabury, 2006: Atmospheric Lifetime and Global Warming Potential of a Perfluoropolyether, Environmental Science and Technology, 40, 2242-2246. This reference provides a lower limit for the lifetime of CF3OCF(CF3)CF2OCF2OCF3 at 800 years and a radiative forcing value of 0.65 W m-2 ppbv-1. [John Owens (Reviewer's comment ID #: 194-11)]	accepted
2-1193	A	130:1	130:2	It is stated that "for ozone depleting substances and their replacements data are taken from IPCC/TEAP (2005) unless otherwise indicated". However, Table 2.14 contains data on many such replacement compounds (mainly HFEs and PFCs) which were not included in IPCC/TEAP (2005), yet the origin of the data is not indicated. Clearly, some comes from the TAR. With successive IPCC and WMO reports each referring back to previous ones, it is becoming increasingly difficult to trace exactly the basis for the lifetime calculations. Some questions in this respect, which appear to be unanswered in the text: Which OH rate constants were used? Was a "correction" made for stratospheric destruction, ocean uptake, or tropospheric photolysis? Was the Prather-Spivakowsky methyl chloroform referencing technique used? If so, for what mean temperature? [Govt. of Belgium (Reviewer's comment ID #: 2003-11)]	Refeecing clarified
2-1194	A	130:3	132:11	Table 2.14. For the current scientific ozone assessment that is in preparation, the global lifetime of CH3Cl will most likely be revised to 1.0 year, due to the discovery of additional sinks. The work of Tokarczyk et al. (2003) (2 papers) suggests a partial lifetime due to ocean removal of 4.1 years. [John S. Daniel (Reviewer's comment ID #: 54-5)]	Updated and reference added

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2-1195	A	130:3	132:11	Table 2.14. For the current scientific ozone assessment that is in preparation, the global lifetime of SF5CF3 is likely to be revised to 650-950 years, reflecting the recent results discussed in Takahashi et al (2002) [John S. Daniel (Reviewer's comment ID #: 54-6)]	Accpeted – table updated
2-1196	A	130:3	132:11	Table 2.14. For several compounds, lifetimes are taken from the TAR, while these were revised in the more recent ozone assessment (WMO, 2003). The more recent revisions should be used. These include HFE-125, -134, -143a, -245cb2, -245fa2, -254cb2, -347mcc3, and -356pcf3. [John S. Daniel (Reviewer's comment ID #: 54-7)]	accepted
2-1197	A	130:3	132:11	Table 2.14. In calculating GWPs for the current ozone assessment that is in preparation, there were several GWPs for which I calculated different values. These include N2O (all GWPs), HFC-125 (20-yr GWP), CF4 (all GWPs), and C10F18 (500-yr GWP - I calculated 9440 rather than 9500). The authors can feel free to contact me if they desire to compare these specific values. [John S. Daniel (Reviewer's comment ID #: 54-8)]	Noted – will clarify
2-1198	A	130:3	132:11	Table 2.14. There is a new paper (in press) by Robson et al. [2006] that suggests that the past NF3 radiative efficiency calculations are based on cross sections whose strong regions were saturated. This paper recommends an increase in the efficiency from 0.13 to 0.21 Wm-2ppb-1. I believe the authors now have a copy of this paper. [John S. Daniel (Reviewer's comment ID #: 54-9)]	Accetped – reference added
2-1199	A	131:0	131:	Table 2.14. For NF3, a recently published paper gives a much greater radiative efficiency (0.21 W/m2.ppb) and GWPs (17200 for a 100-yr ITH). See Robson et al, GRL, 33, L10817, doi:10.1029/2006GL026210. For C5F12, the name in the first column should be PFC-4-1-12. Methylene chloride and methyl chloride should not be called "Freons" (Freon is a registered trade mark and does not apply to CH3Cl and CH2Cl2) [Govt. of Belgium (Reviewer's comment ID #: 2003-13)]	Accepted
2-1200	A	131:0	131:	Table 2.14. For HFE-254cb2 the lifetime indicated (and hence the GWPs) are clearly too low by about an order of magnitude. This conclusion is based on NASA-JPL Publication 02-25 "Chemical Kinetics and Photochemical Data for Use in Atmospheric Studies" Evaluation No. 14, which recommends an OH reaction rate constant of 2.2 E(-14) cm3/molecule.s at 298 K, from Tokuhashi et al (2000) and rejects the earlier value of Heathfield et al (1998) as being much too high. Note that the WMO assessment (2002) gives a lifetime of 2.6 yr for this compound, which is reasonable [Govt. of Belgium (Reviewer's comment ID #: 2003-14)]	Accepted
2-1201	A	131:6		Table 2.14 The chemical category is listed as "perfluorocarbons" however some of these materials are not carbon based such as SF6 and NF3. A more accurate description would be "fully fluorinated compounds" or "perfluorinated compounds."	Accepted –text changed

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				[John Owens (Reviewer's comment ID #: 194-1)]	
2-1202	A	131:15		Table 2.14 The halocarbon designation for C5F12 would be PFC-4-1-12. [John Owens (Reviewer's comment ID #: 194-2)]	accepted
2-1203	A	131:32		The material identified as HFE-449s1 actually contains both n and i isomers of the perfluorobutyl group. Therefore, it is more accurate to represent the chemical formula as C4F9OCH3 [John Owens (Reviewer's comment ID #: 194-3)]	accepted
2-1204	A	131:32		Table 2.14 The lifetime of HFE-449s1 should be corrected to 3.8 years. Wallington et al. (J. Phys. Chem. A, 101, 8264-8274, (1997)) reported that the n- and i- isomers of HFE-449s1 (HFE-7100) were expected to have similar reactivity with OH radicals based upon the observation that there was no difference in isomer reactivity toward Cl and F radicals. The n-C4F9OCH3 was reported in Wallington et al. (1997) to have an OH reaction rate constant of 1.2x10(-14) cm3 molecule-1 s-1 leading to a lifetime of approximately 5 years. These data were re-quoted in WMO Scientific Assessment of Ozone Depletion: 1998, Global Ozone Research and Monitoring Project - Report No. 44, as 5.0 years and attributed to the HFE-7100. The value calculated to two significant figures is 4.7 years. Later measurements by M. Molina and co-workers at MIT on the pure i-C4F9OCH3 demonstrated that the i-C4F9OCH3 is more reactive with OH than the normal isomer. The material identified as HFE-7100 (HFE-449s1) is an approximate 60/40 mixture by weight of the iso and normal isomers. More recent measurements by Oyaro and Nielsen (Asian Chemistry Letters, 7, Nos 2&3, (2003) 119-122) on this commercial mixture of isomers found a kOH value of 1.48x10(-14) cm3 molecule-1 s-1 indicating a lifetime of 3.8 years. [John Owens (Reviewer's comment ID #: 194-5)]	Accetped and table updated
2-1205	A	131:32		More recent measurements by Oyaro and Nielsen (Asian Chemistry Letters, 7, Nos 2&3, (2003) 119-122) on this commercial mixture of isomers found a kOH value of 1.48x10(-14) cm3 molecule-1 s-1 indicating a lifetime of 3.8 years. [John Owens (Reviewer's comment ID #: 194-6)]	Reference added and table updated
2-1206	A	131:32		Table 2.14 The radiative forcing values for HFE-449s1 (HFE-7100) and HFE-569sf2 (HFE-7200) appear to be reversed. The RF values from the original references for HFE-449s1 (Wallington, T. J., et al, J. Phys. Chem. A. 1997, 101, 8264.) and for HFE-569sf2 (Christensen L. K., et al, J. Phys. Chem. A 1998, 102, 4839.) are 0.37 W/m2 and 0.39 W/m2, respectively. In WMO Scientific Assessment of Ozone Depletion: 1998, Global Ozone Research and Monitoring Project - Report No. 44 these values were multiplied by 0.8 to account for the short lifetime of these compounds. This should result in radiative forcing values of 0.30 W/m2 and 0.31 W/m2 for HFE-469s1 and HFE-569sf2, respectively.	Accepted and corrected

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				[John Owens (Reviewer's comment ID #: 194-7)]	
2-1207	A	131:33		Table 2.14 The material identified as HFE-569sf2 actually contains both n and i isomers of the perfluorobutyl group. Therefore, it is more accurate to represent the chemical formula as C4F9OC2H5. [John Owens (Reviewer's comment ID #: 194-4)]	accepted
2-1208	A	131:34		Table 2.14 The designation "H-Galden 1040x" is based upon a commercial name. The halocarbon designation would be HFE-43-10pccc124. [John Owens (Reviewer's comment ID #: 194-8)]	accepted
2-1209	A	131:35		Table 2.14 The designation "HG-10" is based upon a commercial name. The halocarbon designation would be HFE-236ca12. [John Owens (Reviewer's comment ID #: 194-9)]	accepted
2-1210	A	131:36		Table 2.14 The designation "HG-01" is based upon a commercial name. The halocarbon designation would be HFE-338pcc13. [John Owens (Reviewer's comment ID #: 194-10)]	accepted
2-1211	A	132:0		Table footnote (c) – I can find no mention of methane adjustment times in section 7.4.1, and a search of the PDF file reveals that the value of 10.8 years does not occur anywhere in the chapter. This also seems to be the case for the first draft of Ch07 as well. If the value comes from a literature reference, please cite the paper directly from your chapter. If it is meant to come from an assessment in Ch07 can you please double check this and provide a more specific cross-chapter reference. [Martin Manning (Reviewer's comment ID #: 155-4)]	More specific cross reference to chapter 7 added
2-1212	A	133:0		Table 2.15: Rows 13 and 14: The division signs should be "-" signs. [William Collins (Reviewer's comment ID #: 45-13)]	accepted
2-1213	A	133:0		Table 2.15: Rows 11 and 12: There were a couple of errors in the Derwent et al. 2001 paper. The values have been reworked by David Stevenson (Stevenson et al. 2004). The reworked values should be used here (if they aren't already) with a comment to that effect. [William Collins (Reviewer's comment ID #: 45-14)]	accepted
2-1214	A	133:0		Table 2.15: Publications years needed for Derwent et al., Berntsen et al., Wild et al., Stevenson et al. [William Collins (Reviewer's comment ID #: 45-15)]	References added
2-1215	A	133:0		Table 2.15: There is no footnote (b) [William Collins (Reviewer's comment ID #: 45-16)]	Footnote added
2-1216	A	133:47		The sentence that begins on this line reads a little strangely in view of what precedes it. I suggest it begins "Human activities influence water vapour directly through methane emissions, …". [Adrian Simmons (Reviewer's comment ID #: 242-35)]	Text reworded

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2-1217	A	134:31	134:32	The final sentence of the paragraph should be omitted, unless discussion of the indirect radiaitve forcing from water vapour is included also. [Adrian Simmons (Reviewer's comment ID #: 242-36)]	Paragraph rewitten. Sentence no longer exits
2-1218	A	135:0		explain what is meant by dashed and solid lines [Claudia Marcolli (Reviewer's comment ID #: 158-24)]	Explaination added
2-1219	A	136:0		Figure 2. Figures should be understandable on their own. More detailed description of figure needed in caption. [Dylan Millet (Reviewer's comment ID #: 178-11)]	Caption expanded
2-1220	A	136:1	136:9	Figure 2.2 This is my only serious criticism. The figure and caption, as they stand, are unintelligible. I looked up the original figure in Hansen et al. 2006 and see that there are words and parts of the figure missing and the caption is changed. I would suggest getting permission to reproduce the original figure and the original caption, which is reasonably understandable. Note that in the main text the figure is presented with no explanation at all. So another option would be for the author's to use a paragraph on page 2-9 where the figure is referenced to describe what it is all about. However, I would strongly urge using the original figure. [Patrick Hamill (Reviewer's comment ID #: 97-8)]	Caption expanded. The Hansen figure is not correct for this chapter, so need to draw our own figure
2-1221	A	137:5	137:5	The top graph shows plainly that carbon dioxide concntrations are increasing in a linear fashion, and that there is no evidence of a recent increase in rate. [VINCENT GRAY (Reviewer's comment ID #: 88-260)]	Rejected. Both Northern and Southern Hemisphere continuous analyser data show increasing rates.
2-1222	A	137:5	137:5	The second diagram shows that emissions show considerable variability in their rate of increase, but no indication that there is a current change in rate. [VINCENT GRAY (Reviewer's comment ID #: 88-261)]	Accepted
2-1223	A	137:6		Figure#2.3"for Mauna Loa" to "from Mauna Loa" [Junying Sun (Reviewer's comment ID #: 261-29)]	Accepted
2-1224	A	137:9	137:9	"pink" should be "violet", I think. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-70)]	Accepted
2-1225	A	137:10		Figure#2.3 permeg(parts per million), but usually ppm is short for parts per million [Junying Sun (Reviewer's comment ID #: 261-30)]	Accepted ...deleted ppm
2-1226	A	137:12	137:12	Insert the word "in" before "GtC" [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-71)]	Accepted
2-1227	A	137:12	137:13	Sentence "Data … (Marland et al., 2005)" needs a verb -- eg "Data are from …" [Keith Lassey (Reviewer's comment ID #: 140-11)]	Accepted
2-1228	A	137:13	137:13	emissions" should be "emission [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-72)]	Accepted
2-1229	A	137:14	137:14	enegy" should be "energy	Accepted

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				[Jón Egill Kristjánsson (Reviewer's comment ID #: 136-73)]	
2-1230	A	137:14	137:16	The plot of the 13C/12C ratio is actually upside down, with the lighest at the top. This is ok in principle, but the caption should report that inversion, especially when the refence in the text (page 2-11, lines 35-37) talks about "the 13C/12C ratio … presented in Fig. 2.3 decreasing in line with fossil fuel emissions ...". At first glance in Fig. 2.3 13C/12C appears to be increasing rather than decreasing until one notices the inverted scale. Suggest noting in the caption that the scale is inverted "to improve clarity". [Keith Lassey (Reviewer's comment ID #: 140-12)]	Accepted
2-1231	A	137:14	137:14	Enegy --> Energy [Thomas Peter (Reviewer's comment ID #: 198-14)]	Accepted
2-1232	A	138:5	138:5	This diagram needs the addition of uncertainty ranges (to two standard deviations) [VINCENT GRAY (Reviewer's comment ID #: 88-262)]	Noted ..this figure has been removed
2-1233	A	138:5		Figure#2.4 "Timeseries" to "Time series" [Junying Sun (Reviewer's comment ID #: 261-31)]	Noted ..this figure has been removed
2-1234	A	138:6	138:6	Are these derived from global means concentrations (if so, how)? Is CH4 indirect forcing included? [Govt. of Australia (Reviewer's comment ID #: 2001-171)]	Noted ..this figure has been removed
2-1235	A	138:7		The author's name is mis-spelled: it should be "MacFarling Meure". [Keith Lassey (Reviewer's comment ID #: 140-13)]	Noted ..this figure has been removed
2-1236	A	139:4	139:6	The caption says (twice) that the graph shows the trend in global methane abundance, but it actually shows the instantaneous concentration. [Nathan Gillett (Reviewer's comment ID #: 84-97)]	Noted...the plot has been changed to include AGAGE data and the caption has been reworded
2-1237	A	139:4	139:4	This diagram shows clearly that atmospheric methane concentrations have been constant since 1999, and are likely to fall if the trends from 1980 continue [VINCENT GRAY (Reviewer's comment ID #: 88-263)]	Noted... No peer reviewed scientific literature assessed suggests that the methane mixing ratio will fall.
2-1238	A	139:10	139:10	Double all these figurtes to show two standartd deviations (95% confidence limits) [VINCENT GRAY (Reviewer's comment ID #: 88-264)]	Noted 5-95% confidence used
2-1239	A	140:2		Figure#2.6 In the legend "CMDL(flask, insitu)" to CMSDL(flask, in-situ)" [Junying Sun (Reviewer's comment ID #: 261-32)]	Noted. Actually now NOAA/GMD and so updated.
2-1240	A	140:5		Fiugre#2.6 "northern" to "Northern" [Junying Sun (Reviewer's comment ID #: 261-33)]	Accepted.
2-1241	A	140:8		Figure#2.6 "CMDL" to "ESRL" [Junying Sun (Reviewer's comment ID #: 261-34)]	Noted. Changed to NOAA/GMD.
2-1242	A	142:0	143:	indicate the zero line in Figures 2.8 and 2.9 A and B [Claudia Marcolli (Reviewer's comment ID #: 158-25)]	Noted. We will try but it will make some of the panels even more cluttered.

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2-1243	A	142:0		Figure 2.8: Please add zero lines [Govt. of Finland (Reviewer's comment ID #: 2009-41)]	See reply 2-1242.
2-1244	A	143:0		Figure 2.9: Please add zero lines [Govt. of Finland (Reviewer's comment ID #: 2009-42)]	See reply 2-1242
2-1245	A	143:0		The caption and key say that the blue lines show 'oceanic re-emissions'. Are these total emissions including oceanic re-emissions? Or are they really just the oceanic re-emissions as the caption says? If it's the latter, why are they so well correlated with the corrected emissions? [Nathan Gillett (Reviewer's comment ID #: 84-98)]	Noted. Obviously we show OH concentrations not emissions here. We add "using" after "and" on line 143.12.
2-1246	A	143:0		Figure 9. CHCl3 incorrectly used in figure caption; should be CH3CCl3. [Dylan Millet (Reviewer's comment ID #: 178-12)]	Accepted-see 2-1248.
2-1247	A	143:5		The graphs don't show trends. [Nathan Gillett (Reviewer's comment ID #: 84-99)]	Accepted. Change "trends" to "variations".
2-1248	A	143:11	143:11	Add missing C in CH3CCl3 [Ronald Prinn (Reviewer's comment ID #: 202-7)]	Accepted on lines 143.8 and 143.11.
2-1249	A	146:0		Figure 2.12: "Increased cloud height" (Pincus and Baker, 1994) should be "increased cloud thickness" [Govt. of Finland (Reviewer's comment ID #: 2009-43)]	Rejected. Geometric thickness can be confused with optical thickness.
2-1250	A	146:0		CDNC or CDCN? And what does it stand for? [Claudia Marcolli (Reviewer's comment ID #: 158-26)]	Accepted.
2-1251	A	146:0		Resolution of graphic makes text hard to read [Govt. of United States of America (Reviewer's comment ID #: 2023-117)]	Noted. The highest resolution possible will be used in the final printed version.
2-1252	A	146:2	146:2	Twomey (1974) not in reference list. [Olivier Boucher (Reviewer's comment ID #: 27-39)]	Accepted.
2-1253	A	147:2	147:4	Figure 2.13, representing the MODIS 550-nm aerosol optical thickness average for a) January/February/March 2001 and b) August/September/October 2001. Fig. 2.12a is possibly missing data as the squares all appear white, although the legend indicates data values for only red, orange and black squares. [Govt. of Japan (Reviewer's comment ID #: 2014-36)]	Noted. The caption is ammended.
2-1254	A	147:6		Define t_aer. [Nathan Gillett (Reviewer's comment ID #: 84-100)]	Rejected. Aerosol optical depth is a standard measurement of the optical efficiency multiplied by the column loading. This is explained in the text.
2-1255	A	149:0	149:	Figure 2.15 – why no results from Hadley? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-5)]	Noted. There ARE results from the Hadley Centre – the study by Bellouin

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					in Nature is the first row of the table. The Hadley Centre UM did not participate in the AEROCOM project and there has not been a significant publication on the direct radiative effect from ALL aerosol types. This is being rectified as we speak (Jim Haywood).
2-1256	A	150:0		Use the standard AR4 names for the models. Also the caption says that the labels are at the top, but they are in fact at the left. [Nathan Gillett (Reviewer's comment ID #: 84-101)]	accept
2-1257	A	150:0		Figure 2.16, bottom line of figure: "Kristjansson et al., 2002" should be "Kristjansson, 2002" [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-74)]	accept
2-1258	A	150:0		figure cation 2.16: I could not find any lables at the top of the boxes. And: what are the red lines? Average and standard deviation? [Claudia Marcolli (Reviewer's comment ID #: 158-27)]	Figure explained
2-1259	A	153:0		Figure 2.19: Zero lines could be helpful [Govt. of Finland (Reviewer's comment ID #: 2009-44)]	added
2-1260	A	154:0		Figure 2.20: Please add zero line [Govt. of Finland (Reviewer's comment ID #: 2009-45)]	added
2-1261	A	154:0		I found the key confusing on this figure. The text '30 Oct 2003 - 17 Oct 2003' is written in black, but refers to the coloured lines. [Nathan Gillett (Reviewer's comment ID #: 84-102)]	Colour added to key
2-1262	A	154:0		Figure 2.20: The legend "Cycle (Model) Max-Min" is incorrect as some lines are measurements, and its colour black is confusing as it does not refer to the black line. Delete it. [Howard K. Roscoe (Reviewer's comment ID #: 219-14)]	Colour added to key
2-1263	A	157:0		Figure 2.23 Looking at Joshi et al 2003 (clim dyn) the three models should have efficacies of (roughly) 0.8, 0.93 and 1. If this is correct then the lowest value for g is too low in the plot. [Gareth S. Jones (Reviewer's comment ID #: 121-20)]	Accepted, plot corrected
2-1264	A	157:6	157:12	Only the papers which produce the results are referenced, no mention is made of what models were used (apart from the Gregory 2004 models in the main text). What models were used really should be noted here or elsewhere in a table. This is important because it has bearing on the significance of ranges of models. For instance for the Solar efficacy	Partly accepted. Caption now states that independent models are only used -one model per line

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				there are 9 symbols. But briefly looking at the papers suggest that f, k and g (one of) are variates of the same model (IGCM) (I could be wrong!) and the two h's is the same atmosphere model coupled to different oceans. So really there could be much less than 9 "independent" results here. [Gareth S. Jones (Reviewer's comment ID #: 121-19)]	
2-1265	A	157:10	157:10	There are two Joshi et al 2003 in references, which one is this supposed to be? I suspect it is Joshi et al clim.dyn. 2003. [Gareth S. Jones (Reviewer's comment ID #: 121-18)]	Rejected. Reviewer is incorrect, there is only one Joshi et al. There is one Joshi and SHine
2-1266	A	158:0		Figure 2.24: "The level of scientific understanding" for solar radiative forcing is "medium" here while it is "low" in Table 2.11. Which is the official truth? [Govt. of Finland (Reviewer's comment ID #: 2009-46)]	Accepted. Changed to low
2-1267	A	158:0		Upper panel: What exactly does 'Timescale' mean here? Is it the timescale of variations in the forcing, or the timescale for the forcing agent to disappear from the atmosphere if emissions ceased? There seems to be some inconsistencies here. The indirect cloud albedo affect is given a timescale of hours-days, but it is influenced by emissions of aerosols which have varied on decadal timescales. Similarly ozone is given a timescale of weeks to months, but it is influenced by CFCs which have lifetimes of decades in the stratosphere. By contrast land surface albedo is given a timescale of 10-100 years, even though it is influenced by vegetation leaf cover, snow cover etc, which vary on timescales of days-months. Indeed if the indirect aerosol forcing is deemd to vary as clouds form and dissipate, then this will also affect the forcing due to land surface albedo on a similar timescale (the forcing is different for clear and cloudy skies). I suggest either removing this column, or more clearly defining what timescale means here - I think the removal definition might be easiest. [Nathan Gillett (Reviewer's comment ID #: 84-103)]	Scale will be clarified based on time forcing agent takes dissapear if emissions ceased
2-1268	A	158:0		Figure 2.24, lower part, is misleading in that it suggests that there is a reasonable possibility that the total anthropogenic radiative forcing is less the 0.8 W /m2 or even negative. This may be true based on an analysis of the uncertainty in each forcing term, as computed by a bottom-up analysis based (as much as possible) on first principles. However, such low forcings are not at all plausible in light of the observed warming over the last century and the inability of natural mechanism to explain that warming. This is made very clear in Chapter 9 of the WG1 AR4 report. Thus, you need to clearly explain the points that I make here, and cross-reference Chapter 9 in the discussion of this figure and in the caption. [Danny Harvey (Reviewer's comment ID #: 101-26)]	Accepted, cross reference to chapter 9 made in text
2-1269	A	158:0		Figure 2.24 There should be a mention of what timeperiods the RF are calculated over. [Gareth S. Jones (Reviewer's comment ID #: 121-21)]	Accepted all 1750-2005

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2-1270	A	158:0		Figure 2.24 The colours seperating the plots in the lower graph are not distinct enough. The orange is not very different to the red [Gareth S. Jones (Reviewer's comment ID #: 121-22)]	accepted
2-1271	A	158:0		In Figure 2.24 : Is total RF large enough to explain the observed warming? See supplemental material file Govt_USA_AR4ERSOD_Ch10_sup.png [Govt. of United States of America (Reviewer's comment ID #: 2023-130)]	Cross reference to chapter 9 will be made
2-1272	A	158:0		In Figure 2.24. The 2.24 caption does not state that the uncertainties are one sigma, whereas the other two renditions do note this. [Govt. of United States of America (Reviewer's comment ID #: 2023-131)]	Text on uncertainty range will be clarified
2-1273	A	158:1	158:3	It is notable (surprising?) that the level of scientific understanding for pre-satellite-era solar forcing which is based on proxies and models has jumped from "Very Low" in the TAR, to "Medium" in the AR4 figure. This should either be explained and highlighted here, or corrected including in this Figure which appears 3 times. In addition, this contradicts Chapter 2, page 6, lines 27-28! [Haroon Kheshgi (Reviewer's comment ID #: 125-31)]	Changed to "low". Accepted
2-1274	A	158:3	:31	We suggest a revision to Figure 2.24, bottom panel, and associated text. This figure shows the probability distribution of total anthropogenic radiative forcing (RF). A rough mock-up of the proposed revision to the lower panel is included below. [Govt. of United States of America (Reviewer's comment ID #: 2023-118)]	Rejected. However, text with cross reference to chapter 9 will address this. We do not want make attribution statements – which this extra line is
2-1275	A	158:3	:31	We strongly support the ideas of discussing total anthropogenic RF and of presenting this as a statistical probability distribution, as is done in the current version of Figure 2.24, bottom panel. This figure includes a vertical line at a total forcing value of zero (W/m2) and the text (page 2-67, lines 17-18) uses this vertical line to conclude that total anthropogenic RF since 1750 is "very likely" to have been positive. This conclusion is deemed important enough to mention in the Chapter 2 Executive Summary (page 2-6, line 19) as well as in the Summary for Policy Makers (page SPM-5, line 1) and in the Technical Summary (page TS-12, lines 19-24). [Govt. of United States of America (Reviewer's comment ID #: 2023-119)]	No reply required
2-1276	A	158:3	:31	However, the zero line is not the only, and probably not the most important, threshold value to show and discuss. We recommend that a vertical line be added to the lower panel of Figure 2.24 at an RF value of +0.8 W/m2. This is the critical value of total anthropogenic RF that emerges from six "inverse" (or "top-down") climate-model calculations (Refs 1-6, below) as summarized by Ref. 7 (below). Values of total anthropogenic RF that are less positive than +0.8 W/m2 are inconsistent with every one of these "inverse" studies. Inconsistency, in this sense, means that it would be problematic to posit known RF's as the explanation of the observed, industrial-era increase in surface temperature (about +0.6 K). (Such an inconsistency would imply that the observed	Reference to chapter 9 made to top-down appraoch discussion, but figure will not adopt this approach

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				warming was caused by currently unknown forcings or that natural variability or climate sensitivity is higher than what climate models currently allow). To show and discuss only the threshold at zero W/m2 fails to consider this issue of causal connection and fails to acknowledge and integrate important scientific progress since the TAR. [Govt. of United States of America (Reviewer's comment ID #: 2023-120)]	
2-1277	A	158:3	:31	Along with the change to the lower panel of Figure 2.24, the relevant statements in the Chapter 2 text and Executive Summary should be modified to include consideration of whether total anthropogenic RF is a plausible explanation of the observed global-mean temperature increase, as detailed above. [Govt. of United States of America (Reviewer's comment ID #: 2023-121)]	This is outside scope of our chapter. Chaapter 9 can consider this
2-1278	A	158:3	:31	1. Wigley, T. M. L., and S. C. B. Raper, Interpretation of high projections for global-mean warming, Science, 293, 451-454, 2001. [Govt. of United States of America (Reviewer's comment ID #: 2023-122)]	noted
2-1279	A	158:3	:31	2. Harvey, L. D. D., and R. K. Kaufmann, Simultaneously constraining climate sensitivity and aerosol radiative forcing, J. Climate, 15, 2837-2861, 2002. [Govt. of United States of America (Reviewer's comment ID #: 2023-123)]	noted
2-1280	A	158:3	:31	3. Gregory, J. M., R. J. Stouffer, S. C. B. Raper, P. A. Stott, and N. A. Rayner, An observationally based estimate of climate sensitivity, J. Climate, 15, 3117-3121, 2002. [Govt. of United States of America (Reviewer's comment ID #: 2023-124)]	noted
2-1281	A	158:3	:31	4. Andronova, N. G., and M. E. Schlesinger, Objective estimation of the probability density function for climate sensitivity, J. Geophys. Res., 106, 22605-22611, 2001. [Govt. of United States of America (Reviewer's comment ID #: 2023-125)]	noted
2-1282	A	158:3	:31	5. Knutti, R., T. F. Stocker, F. Joos, and G.-K. Plattner, Constraints on radiative forcing and future climate change from observations and climate model ensembles, Nature, 416, 719-723, 2002. [Govt. of United States of America (Reviewer's comment ID #: 2023-126)]	noted
2-1283	A	158:3	:31	6a. Forest, C. E., P. H. Stone, A. P. Sokolov, M. R. Allen, and M. D. Webster, Quantifying uncertainties in climate system properties with the use of recent climate observations, Science, 295, 113-117, 2002. [Govt. of United States of America (Reviewer's comment ID #: 2023-127)]	noted
2-1284	A	158:3	:31	6b. Forest, C. E., P. H. Stone, A. P. Sokolov (2006), Estimated PDFs of climate system properties including natural and anthropogenic forcings, Geophys. Res. Lett., 33, L01705, doi:10.1029/2005GL023977. [Govt. of United States of America (Reviewer's comment ID #: 2023-128)]	noted
2-1285	A	158:3	:31	7. Anderson, T. L., R. J. Charlson, S. E. Schwartz, R. Knutti, O. Boucher, H. Rodhe, and J. Heintzenberg, Climate forcing by aerosols - A hazy picture, Science, 300, 1103-1104,	noted

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				2003. [Govt. of United States of America (Reviewer's comment ID #: 2023-129)]	
2-1286	A	158:5	158:5	This diagram omits water vapour and ordinary clouds. It is no excuse to say they are "feedbacks". They are important components of the radiative forcing budget and it is dishonest to leave them out. [VINCENT GRAY (Reviewer's comment ID #: 88-265)]	Rejected. The case for "leaving them out" is clearly outlined in the text – they are not RFs by any definition
2-1287	A	158:5	158:5	All the "error bars" must be doubled, to show two standard deviations and 95% confidence levels [VINCENT GRAY (Reviewer's comment ID #: 88-266)]	Uncertainty limits altered to 90%
2-1288	A	158:5	158:5	The total net radiative forcing could obviously be zero or negative, particularly if the correct confidence levels were inserted [VINCENT GRAY (Reviewer's comment ID #: 88-267)]	Noted. Text clarified
2-1289	A	158:22	158:22	It should be stated whether indirect forcing by CH4 included? [Govt. of Australia (Reviewer's comment ID #: 2001-172)]	Accepted. This will be done in text
2-1290	A	158:27		insert "that" after "assuming" [Danny Harvey (Reviewer's comment ID #: 101-27)]	accepted
2-1291	A	159:0		Figure 2.25: The error bars need to be included in this figure since in some cases they will be very significant. It is not sufficient to refer to footnotes to table 2.13 because, given the popularity of IPCC figures, this picture is likely to be reproduced many times in talks and other presentations without due consideration being given to the uncertainties. [William Collins (Reviewer's comment ID #: 45-11)]	Rejected. We tried thefigure with error bars and it made the figure too complicated to understand. Errors apear in table, so repition not needed
2-1292	A	159:0		Figure 2.25. The estimate of direct forcing from nitrate aerosol is well-done and very defensible, but I don't agree with the decision on the figure to place nitrate under short-lived gases instead of under aerosols. Even though NOx is a precursor to nitrate aerosols, there are three reasons to put nitrate under aerosols. First, nitrate aerosol belongs with the other aerosols because observational constraints are primarily on the total aerosol, not the total minus nitrate. This is explained cogently on 2-5 line 11, "The RF of separate aerosol species is less certain that the combined RF." [Daniel Murphy (Reviewer's comment ID #: 183-23)]	Accepted. Nitrate bar will be moved
2-1293	A	159:0		Figure 2.25 continued. Second, in many regions with anthropogenic emissions, there is much more nitric acid than ammonia in the gas phase. In those regions formation of nitrate aerosol is limited by ammonia. The modeling studies cited to support the nitrate RF did not only change NOx. For example, Liao and Seinfeld (2005) estimated that from pre-industrial to present times NOx increased from 8.6 to 40 Tg N yr-1 but they also estimated that ammonia increased from 18.7 to 57.6 Tg N yr-1. So in that study one cannot attribute changes in ammonium nitrate solely to changes in NOx. Third, nitrate and sulfate undergo chemical reactions so that in a neutral (as opposed to acidic) aerosol	Accepted. Nitrate will be moved

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				decreases in sulfate are directly linked to increases in nitrate. They should therefore be in the same aerosol category. To reiterate, I am not disagreeing with the nitrate estimate, only where it is put on the figure. Another way of saying this entire comment is that in the chapter summary [p. 2-5 line 14] nitrate is listed as an aerosol forcing but on Figure 2.25 nitrate is shown under short-lived gases. That is confusing. [Daniel Murphy (Reviewer's comment ID #: 183-24)]	
2-1294	A	160:0	160:	Fig 2.26 The RF for LLGHGs looks like it is straight lines interpolated between points, rather than the smoothly varying curve that the GHG changes show, reflected in figure 2.4 [Govt. of Australia (Reviewer's comment ID #: 2001-173)]	Accepted. Will improve figure.
2-1295	A	160:0		Figure 2.26 "tropopause" in the lower panel should be "troposphere" [William Collins (Reviewer's comment ID #: 45-12)]	Accepted.
2-1296	A	160:0		Figure 2.26: lower part: Ozone (tropopause) should be ozone (troposphere)? [Govt. of Finland (Reviewer's comment ID #: 2009-47)]	See 2-1295
2-1297	A	160:0		Upper panel: Why is the direct aerosol forcing almost zero? [Nathan Gillett (Reviewer's comment ID #: 84-104)]	Rejected. Could be so in some models. Because of positive and negative contributions, it can end up as a small value.
2-1298	A	160:5	160:5	Again, you have left out water vapour and clouds. It is no excuse that the models are not able to handle them except as "feedbacks" [VINCENT GRAY (Reviewer's comment ID #: 88-268)]	Rejected.
2-1299	A	160:6	161:9	Model name is referred differently in Figure 2.26 and 2.27. Group name is included in Figure 2.27, but is not included in Figure 2.26. If we follow the style in Figure 2.27, name should be "MIROC" -> "CCSR/NIES/FRCGC MIROC". [Yoko Tsushima (Reviewer's comment ID #: 269-2)]	Accepted. Will revise.
2-1300	A	160:10	160:11	Should this be chapter 9 not "chapter 10"? Chap 9 covers most of the 20thC simulations work, whilst chapter 10 deals with projections. [Gareth S. Jones (Reviewer's comment ID #: 121-23)]	Accepted. Yes, but dropping this sent.
2-1301	A	161:0		Figure 2.27: 2nd line of caption: "antrhropogenic" should be "anthropogenic" [Govt. Of Finland (Reviewer's comment ID #: 2009-48)]	Accepted.
2-1302	A	161:9	161:9	CCSR-MIROC model -> CCSR/NIES/FRCGC MIROC model [Yoko Tsushima (Reviewer's comment ID #: 269-3)]	Accepted. Will revise.
2-1303	A	161:10		The caption of Fig 2.27 states that what is shown is "almost similar" to the RF. But RF is defined as the change since 1750, not 1860. [Adrian Simmons (Reviewer's comment ID #: 242-37)]	Noted. These illustrative representations are instantaneous forcings which suffice for the points made in the sub-section. The reference to RF was confusing, and that is

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					dropped.
2-1304	A	162:5	162:6	Delete from "The figure gives an indication" on line 5 to "current emissions" on line 6. This statement is untrue. There has to be some indication of how emissions influence atmospheric concentrations before you can claim that variations in emissions influence climate. [VINCENT GRAY (Reviewer's comment ID #: 88-269)]	Figure no longer exists
2-1305	A	162:5		IMPORTANT: Figure 2.28. Please delete bottom half of figure 2.28 as GWPs are a political concept that normally implies that it is meaningful to built a weighted sum of different emissions, independent of its source region and timing. Given that the forcing implications of short-lived cooling agents and long-lived warming agents do not simply offset one another, it seems scientifically misleading to present both aerosols and LLGHGs on this same GWP scale. Furthermore, it is inappropriate to present central GWP estimates for these short-lived species, given that their radiative effects are strongly dependent on the timing and region of the source emissions. Thus, please delete the bottom half of figure 2.28 for these two reasons and possibly expand the top half for the different gases. [Govt. of Germany (Reviewer's comment ID #: 2011-123)]	Figure is deleted. Different rpresentation now made in Section 2.9. GWP reference dropped
2-1306	A	163:1	163:7	[From David Wratt - David Fahey is an LA for this Chapter]. The middle panel (methane and hydrocarbons) could be confusing to readers. I sugget this could be improved by removing the bracketed words (RF only) from above this panel, and adding the following words to the legend: …In the middle figure, the black curve is for methane, and the red curve is for halocarbons and shows radiative forcing only. [David Wratt & David Fahey (Reviewer's comment ID #: 67-100)]	Accepted.
2-1307	A	163:3	163:3	Add label ‘‘Year" to the horizontal axis in this Figure and replace the header in the middle panel as follows ‘‘Methane (black line) and Halocarbons (red line)" [Michael Danilin (Reviewer's comment ID #: 55-25)]	Accepted.
2-1308	A	163:5	153:5	The diagram for methane should show that it has stabilised since 1999 [VINCENT GRAY (Reviewer's comment ID #: 88-270)]	Accepted.
2-1309	A	163:5	163:5	The figure caption should read: Atmospheric concentrations of important greenhouse gases … [Wilmer Anderson (Reviewer's comment ID #: 5-31)]	Accepted.
2-1310	A	163:5	163:5	What is the red line in the methane curve? Do the RF scales take into account the non linearity of RF with concentration? [Govt. of Australia (Reviewer's comment ID #: 2001-174)]	Accepted. Red curve to be omitted.
2-1311	A	164:2	164:2	I suggest to replot this Figure by dropping results for contrail cirrus, which are tiny compared with other anthropogenic perturbations. Also, the quoted value of 0.01 W/m2 corresponds to the linear contrail RF and does not include their indirect effects. Hence,	Rejected. Although your point is well taken, contrails are included here in order to maintain consistency with the

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				contril results shown in this plot generate more questions than give answers and could confuse readers. [Michael Danilin (Reviewer's comment ID #: 55-27)]	primary radiative forcing figure (Fig. 2.24).
2-1312	A	164:5	164:5	Again water vapour abd clouds are omitted. It is no excuse to cite the inadequacies of model treatent [VINCENT GRAY (Reviewer's comment ID #: 88-271)]	Rejected. Water vapor direct effect and cloud indirect effect are included.
2-1313	A	164:5	164:5	Again all the uncertainty bars should be doubled to rerpresent two standarddeviations and 95% accuracy [VINCENT GRAY (Reviewer's comment ID #: 88-272)]	Accepted.