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| 7-1 | 7 | 0 | | | | Propose to add references to AMAP report 2012 on effects of BC in the Arctic. AMAP, 2011. The Impact of Black Carbon on Arctic Climate (2011). Quinn et al. Arctic Monitoring and Assessment Programme (AMAP), Oslo. 72 pp. [Øyvind Christophersen, Norway] | reviewer needs to be specific as to where this fits. Preference goes to peer-reviewed literature. |
| 7-2 | 7 | 0 | | | | This chapter is considerably improved from the ZOD, and I commend the authors for constructing a draft that is more balanced, better organized, covers most of the important issues, and is well written. That having been said, there are still differences in approach and emphasis among the individual sections on clouds, aerosols, and cloud-aerosol interactions. The chapter as a whole would benefit if these differences were reduced. The clouds section, with one exception noted in a future comment, really focuses on the climatic effects of clouds and eschews detailed discussions of the cloud physics that leads to these climate effects, so much so that some of the more important science issues are ignored and others given short shrift. The aerosol section is just the opposite - it reads more like a review article in a journal than an IPCC report section, reviewing all the details of aerosol physics/chemistry, including many things that may or may not turn out to be significant in the future but which have not yet been demonstrated to be important to climate change. It seems to this reviewer that the cloud-aerosol interaction section comes closest to getting the balance right between explaining details of the physics and explaining how they are understood to affect or not affect climate change. The section on cosmic ray effects on climate is too long given that there are as yet no demonstrated, or even likely, effects on long-term climate change; the length of this section conveys to the reader a sense of the importance of this topic that is not indicative of the sense of the community. The geoengineering section strikes a reasonable balance. [Anthony Del Genio, USA] | We concur with these comments about the amounts of detail in each section. We will try to address the imbalances while at the same time reducing the overall length as required by the TSU. |
| 7-3 | 7 | 0 | | | | This Chapter purports comprehensive information on the role of aerosols (both natural and anthropogenic), clouds and their interactions in the present-day climate and climate change scenarios covering from tropics through poles. It provides an overview on the rapid responses and feedbacks between aerosols and clouds vis-a-vis precipitation in changing climate, particularly through their radiative forcing (direct, semi-direct and indirect effects). The state-of-the-art of process-based climate models, their capabilities and requirements for improving them against the in-situ and remote sensing observations has also been detailed in this Chapter. The deviations are discussed mainly in terms of the existing theories and new discoveries. The Chapter concludes with direction and further understanding of unknown feedbacks and aerosol-cloud-climate interactions including recently-introduced stratogies / studies like Solar Radiation Management for geoengineering, cosmic ray influence on new particle formation etc. [Panuganti China Sattilingam Devara, India] | Thanks. |
| 7-4 | 7 | 0 | | | | This chapter gives an overview of our current knowledge and understanding of aerosols, clouds, and their mutual interactions with the climate system by describing results from ground and space borne observations, theoretical models, detailed cloud resolving models and global models. We find that not enough emphasis is given to regional climate models as well as the regional impacts of clouds and aerosols in this chapter. Global climate models are often not detailed enough to give robust information on the regional scale. Such information is however highly relevant for the society and for policy-makers. As has been noted in the chapter, there currently is a gap between high resolution cloud models and global models. Although GCRMs might be able to close this gap in a decade, regional online-coupled climate models with an explicit treatment of atmospheric chemistry and aerosols can close it much earlier, although for a limited area of the globe. We provide some concrete suggestions on how this point can be taken better into account in this review. Additional/further improvements may still be possible. [Andrew Ferrone, Germany] | This chapter focuses on cloud and aerosol processes, rather than the regional climate response to clouds and aerosols. In that sense, a lot of the discussion applies equally to global and regional models. Some regional aspects are discussed in section 7.2 (cloud feedbacks) and 7.6 (precipitation). Regional climates are discussed in chapter 14. |
| 7-5 | 7 | 0 | | | | Overall, I complement the authors on an excellent first draft. [Warren Richard Leaitch, Canada] | Thanks. |
| 7-6 | 7 | 0 | | | | Bell et al. (2008) and Lee (2012) have suggested that aerosol-pollution-induced changes in large-scale circulations affect clouds far removed from an aerosol-pollution source. Stately differently, aerosol pollution can be tele-connected to clouds far away from the pollution source, leading to the redistribution of energy and water in much larger areas than areas directly affected by aerosol pollution. This indicates that just focusing on polluted areas can be misleading and we need to think about the effect of aerosol pollution with temporal and spatial scales larger than those scales for aerosol pollution. This tele-connection of aerosol effect is supported by other studies. Lynn et (2005a,b) simulated a rain event over Florida using MM5 with spectral bin | this chapter focuses on aerosol forcing. We have introduced the concept of adjusted forcing, which includes rapid adjustments to aerosol-cloud interactions and remote effects of aerosols on clouds. The large-scale impact of aerosols on regional climate is discussed in Chapter 14. |

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| | | | | | | microphysics. They showed that in spite of the fact that aerosol pollution tends to intensify squall line over a polluted area, the total precipitation over larger computational area than the polluted area remained unchanged or slightly decreased. Even more pronounced mechanism of aerosol effect on a large scale phenomenon (a tropical cyclone (TC)) was investigated by Rosenfeld et al (2007), Zhang et al (2007), Khain et al (2008-2010). It was shown that aerosol-intensified convection developing at TC periphery competes with that in the eyewall convection leading to TC weakening and to a decrease in precipitation over the entire region. The importance of compensating downdrafts as well as of a cool pool in the boundary layer arising in zones of convective precipitation and expanding toward the TC center was stressed.<br><br>This reviewer believes this tele-connection effect needs to be included in this report, considering increasing spatial gradient of aerosol concentration (which drives this tele-connection) since industrialization. Since industrialization, land-sea contrast in aerosol concentration has increased. Also, increasing pollution in urban areas since industrialization has enhanced the spatial gradient over land. Hence, it is likely that energy and water redistribution by tele-connection has been changing since industrialization. Large-scale circulations have a long-term effect on climate as compared to aerosol pollution itself and associated individual clouds. Thus, aerosol effect on these circulations and the effect of aerosol-induced circulations on much larger areas than polluted areas are likely to be more relevant to climate than aerosol pollution and its effect on clouds over the polluted areas.<br><br>References:<br><br>Bell, T. L., Rosenfeld, D., Kim, K.-M., et al.: Midweek increase in U. S. summer rain and storm heights suggests air pollution invigorates rainstorms, J. Geophy. Res., 113, D02209, doi:10.1029/2007JD008623, 2008.<br>Khain, A. P., N. Cohen, B. Lynn and A. Pokrovsky, 2008: Possible aerosol effects on lightning activity and structure of hurricanes. J. Atmos. Sci. 65, 3652-3667.<br><br>Khain, A. P., B. Lynn and J. Dudhia, 2010: Aerosol effects on intensity of landfalling hurricanes as seen from simulations with WRF model with spectral bin microphysics, J. Atmos. Sci. , 67, 365-384<br><br>Lee, S. S., 2011, Effect of aerosol on circulations and precipitation in deep convective clouds, J. Atmos. Sci., accepted.<br><br>Lynn B., Khain, A. P., J. Dudhia, D. Rosenfeld, A. Pokrovsky, and A. Seifert 2005a: Spectral (bin) microphysics coupled with a mesoscale model (MM5). Part 1. Model description and first results. Mon. Wea. Rev. 133, 44-58.<br><br>Lynn B., Khain, A. P., J. Dudhia, D. Rosenfeld, A. Pokrovsky, and A. Seifert 2005b: Spectral (bin) microphysics coupled with a mesoscale model (MM5). Part 2: Simulation of a CaPe rain event with squall line Mon. Wea. Rev. , 133, 59-71.<br><br>Rosenfeld, D., Khain, A. P., B. Lynn, W. L. Woodley, 2007: Simulation of hurricane response to suppression of warm rain by sub-micron aerosols. Atmos. Chem. Phys. Discuss., 7, 5647-5674,<br><br>Zhang, H., G.M. McFarquhar, S.M. Saleeby, and W.R. Cotton 2007: Impacts of Saharan dust as CCN on the evolution of an idealized tropical cyclone. Geophys. Res. Lett. 34 L14812, doi: 10.2029/2007GL029876<br>[Seoung Soo Lee, United States of America] | |
| 7-7 | 7 | 0 | | | | A well established chapter with clear explanations and statements [Claudia Mäder, Germany] | Thanks. |
| 7-8 | 7 | 0 | | | | As far as aerosols are concerned, about 70% of the global abundance still remains natural.It is also now well recognized that the increased human activity pressure, has significantly altered the natural component also, so that there is a signicicant human-induced nautral component. this is true for dust and BC (urbanization and agriculture/ biomass burning). A thorough assessment should consider all these three aspects; natural, | this is a good remark. Unfortunately there is extremely little literature on anthropogenic modification to the natural aerosols. We do mention now the anthropogenic impact on natural SOA. |

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| | | | | | | anthropogenic and the anthropogenic-modlulated-natural. Moreover, in such a scenario, the aerosol species would have a complex mixing structure, external, internal and core-shell. These aspects are to be pointed out in the report in one section dealing with complexities and not-well-resolved part of the problem and the assessments may be put within the uncertainty range due to these also [K KRISHNA MOORTHY, INDIA] | |
| 7-9 | 7 | 0 | | | | The Term “Geoengineering” is sometimes spelled in different ways (i.e. Geoengineering or Geo-engineering) - Spelling needs to be consistent [Birgit Nabbefeld, Germany] | agreed. Will resolve in next version |
| 7-10 | 7 | 0 | | | | Chapter 7: Most of the fifth-level subsections (e.g. section 7.2.3.3.1) are a single paragraph long. It may not be necessary to highlight the structure at this level of detail. [Robert Pincus, USA] | this will be harmonized in the SOD. |
| 7-11 | 7 | 0 | | | | Chapter 7: Page 7-41, lines 10-11 make the accurate point that the terminology “cloud lifetime” is inaccurate, but the term is used throughout the chapter anyway. Perhaps this can be replaced in revisions. [Robert Pincus, USA] | we are moving to a new terminology, which we try to use consistently in the manuscript. |
| 7-12 | 7 | 0 | | | | For the aerosol forcing discussion it might be useful to add more quantitative model evaluation information. Which temporal/regional bias exists in the model fields? What is the forcing of those models with highest correlation to observed AOD and other aerosol parameters of relevance. [Michael Schulz, Norway] | ideally yes. Looking forward to your paper! |
| 7-13 | 7 | 0 | | | | First of all, I would like to commend the authors for putting together a great chapter. The introductory and background material provide a great overview, and the  gures are wellchosen and well done. The authors have pulled together a substantial amount of material in a short period of time, and they have done a great job. I only have a couple of concerns. [Gregory Schuster, USA] | Thanks. |
| 7-14 | 7 | 0 | | | | Comments References:<br>Balkanski, Y., M. Schulz, T. Claquin, and S. Guibert (2007), Reevaluation of mineral aerosol radiative forcings suggests a better agreement with satellite and aeronet data, Atmospheric Chemistry and Physics, 7 (1), 81{95.<br>Bond, T., and R. Bergstrom (2006), Light absorption by carbonaceous particles: An investigative review, Aerosol Sci. Technol., 40 (1), 27{67.<br>Chen, C., and B. Cahan (1981), Visible and ultraviolet optical properties of single-crystal and polycrystalline hematite measured by spectroscopic ellipsometry, J. Opt. Soc. Am., 71 (8), 932{934.<br>Chowdhary, J., B. Cairns, M. I. Mishchenko, P. V. Hobbs, G. F. Cota, J. Redemann, K. Rutledge, B. N. Holben, and E. Russell (2005), Retrieval of aerosol scattering and absorption properties from photopolarimetric observations over the ocean during the CLAMS experiment, J. Atmos. Sci., 62, 1093{1117.<br>Chylek, P., and J. Coakley (1974), Aerosols and climate, Science, 183, 75{77.<br>Dubovik, O., A. Smirnov, B. Holben, M. King, Y. Kaufman, T. Eck, and I. Slutsker (2000), Accuracy assessments of aerosol optical properties retrieved from Aerosol Robotic Network (AERONET) sun and sky radiance measurements, J. Geophys. Res., 105 (D8), 9791{9806.<br>Dubovik, O., B. Holben, T. Eck, A. Smirnov, Y. Kaufman, M. King, D. Tanre, and I. Slutsker (2002), Variability of absorption and optical properties of key aerosol types observed in worldwide locations, J. Atmos. Sci., 59, 590{608.<br>Egan, W., and T. Hilgeman (1979), Optical Properties of Inhomogeneous Materials, Academic Press.<br>Koch, D., et al. (2009), Evaluation of black carbon estimations in global aerosol models, Atmos. Chem. Phys., 9, 9001{9026.<br>Loeb, N., and W. Su (2010), Direct aerosol radiative forcing uncertainty based on a radiative perturbation analysis, J. Climate, 23, 5288{5293.<br>Myhre, G. (2009), Consistency between satellite-derived and modeled estimates of the direct aerosol e ect, Science, 325, 187{190.<br>Ostr  om, E., and K. Noone (2000), Vertical pro les of aerosol scattering and absorption measured in situ during the North Atlantic Aerosol Characterization Experiment ACE-2, Tellus, 52B, 526{545. | thanks. Not all studies can be references but some of these will be added. |

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| | | | | | | Ramanathan, V., and G. Carmichael (2008), Global and regional climate changes due to black carbon, Nature Geoscience, 1, 221{227, doi:10.1038/ngeo156. Russell, P., J. Redemann, B. Schmid, R. W. Bergstrom, J. M. Livingston, D. McIntosh, and coauthors (2002), Comparison of aerosol single scattering albedos derived by diverse techniques in two north Atlantic experiments., J. Atmos. Sci., 59, 609{619. 5 Sato, M., J. Hansen, D. Koch, A. Lacis, R. Ruedy, O. Dubovik, B. Holben, M. Chin, and T. Novakov (2003), Global atmospheric black carbon inferred from AERONET, PNAS, 100 (11), 6319{6324. Schuster, G. L., O. Dubovik, B. N. Holben, and E. E. Clothiaux (2005), Inferring black carbon content and speci c absorption from Aerosol Robotic Network (AERONET) aerosol retrievals, J. Geophys. Res., 110, D10S17, doi:10.1029/2004JD004548. Schwarz, J., J. Spackman, R. Gao, L. Watts, P. Stier, M. Schulz, S. Davis, S. Wofsy, and D. Fahey (2010), Global-scale black carbon pro les observed in the remote atmosphere and compared to models, Geophys. Res. Lett., 37, L18812, doi:10.1029/2010GL044372. Schwarz, J. P., et al. (2006), Single-particle measurements of midlatitude black carbon and light-scattering aerosols from the boundary layer to the lower stratosphere, J. Geophys. Res., 111, D16207, doi:10.1029/2006JD007076. Sokolik, I., and O. Toon (1999), Incorporation of mineralogical composition into models of the radiative properties of mineral aerosol from uv to ir wavelengths, J. Geophys. Res., 104, 9423{9444. Su, W., G. Schuster, N. Loeb, R. Rogers, R. Ferrare, C. Hostetler, J. Hair, and M. Obland (2008), Aerosol and cloud interaction observed from high spectral resolution lidar data, J. Geophys. Res., 113, D24202, doi:10.1029/2008JD010588. Su, W., N. Loeb, K.-M. Xu, G. Schuster, and Z. Eitzen (2010), An estimate of aerosol indirect e ect from satellite measurements with concurrent meteorological analysis, J. Geophys. Res., 115, D18219, doi:10.1029/2010JD013948. Waquet, F., B. Cairns, K. Knobelspiesse, J. Chowdhary, L. Travis, B. Schmid, and M. Mishchenko (2009), Polarimetric remote sensing of aerosols over land, J. Geophys. Res., 114, doi:10.1029/2008JD010619. [Gregory Schuster, USA] | |
| 7-15 | 7 | 0 | | | | Over all, I think this chapter is very well written, and I would like to thank all authors for producing such a comprehensive review of our current knowledge on clouds, aerosols, and aerosol-cloud interactions. One concern I have is that many recent important studies are not cited or not discussed in detail, such as many studies published in 2011. I would expect this will be improved for the next draft, and would expect many new studies in 2011 and even in 2012 will be included in the final draft. It will be worth for the working group to discuss a strategy how to incorporate the latest literatures in the final version. Another concern is that I feel the methrodolgoy that are used to derive the uncertainty range of aerosol indirect forcing is questionable, as I detailed below. [Minghuai Wang, United States of America] | it is not possible to cite all 2011 studies, but we try to cite the most important ones. See below for the reply to the comment on uncertainty ranges. |
| 7-16 | 7 | 0 | | | | Please consistingly refer to aerosol particles, if you mean the particles. Aerosols (as referred to on page 7-5, line 19) include both the carrier gas plus the particles. Aerosol = Carier Gas + Particles. Aerosol particles are only the particulate matter. This should consistently be distinguished in the whole chapter. [Manfred Wendisch, Germany] | we do distinguish between "aerosols" and "aerosol particles" in Section 7.1 Then we think it is acceptable to use "aerosols" more loosely. |
| 7-17 | 7 | 0 | | | | There is a need to carefully clarify concepts of forcing, adjusted forcing, radiative forcing etc. Refer to Chapter 8, Fig 8.1. (Indirect) adjusted RF and (indirect) RF definitions have to be consistent and robust.. [Thomas Stocker/ WGI TSU, Switzerland] | Section 7.1 does a better job at defining the new terminology and we refer to Chapter 8. |
| 7-18 | 7 | 0 | | | | Concluding assessment statements are lacking. Consider adding clear synthesis paragraphs at the end of each sub-section. See also general comment to all chapters on this point. [Thomas Stocker/ WGI TSU, Switzerland] | good point. |
| 7-19 | 7 | 0 | | | | Consider to add some quantitative information/ranking of top processes to the flowchart figures [Thomas Stocker/ WGI TSU, Switzerland] | not sure how this can be done. Very often it is the combination of processes which matter. |

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| 7-20 | 7 | 0 | | | | Section 7.5: We consider the introduction currently fails to provide the clear message that effects associated with changes in Solar forcing are not equal to effects associated with changes in CO2. Current discussion in this section is not explicit enough. [Thomas Stocker/ WGI TSU, Switzerland] | agreed. These issues were inadvertantly removed in attempts to be brief. They need to be reinserted. |
| 7-21 | 7 | 0 | | | | We suggest the inclusion of a compelling feedback synthesis figure within Section 7.2.4.8. [Thomas Stocker/ WGI TSU, Switzerland] | We now have a diagram illustrating the key feedback mechanisms, and two scatter plots showing recent new quantitaive insights into the H2O and cloud feedbacks. |
| 7-22 | 7 | 0 | | | | Section 7.2.5.2: Reference should be made to the recent SREX assessment of extreme precipitation. [Thomas Stocker/ WGI TSU, Switzerland] | We did not refer to it. |
| 7-23 | 7 | 0 | | | | Chapter is currently short on material regarding present day cloud climatology. For example, a compelling figure/map showing climatology of cloud type, cloud cover, seasonality etc. (similar as for Section 7.9.) would be useful. Likewise, this comment for cloud projections. Some of this material is in Chapter 2 and 12, but it would be useful if this material was also covered in Chapter 7 - coordination required with these chapters. [Thomas Stocker/ WGI TSU, Switzerland] | We have added several new figures in offline consultation with this Reviewer. |
| 7-24 | 7 | 0 | | | | I like the use of the terms iRF and iAF instead of first, second,....nth aerosol effect. For bookkeeping and assessing model simulations this is a good way to break it down. [Robert Wood, USA] | Thanks. |
| 7-25 | 7 | 0 | | | | I like the idea of having a separate chapter on clouds. I think the executive summary does a very nice job. This is a really good advance of previous assessments. It's a great first draft. I commend the authors on a great job. [Robert Wood, USA] | Thanks. |
| 7-26 | 7 | 0 | | | | There's a surprisingly large fraction of the chapter devoted to the aerosol effects (30 pages) and much less to cloud feedbacks (16 pages), even though the latter is the most important problem. Perhaps this reflects the lack of funding for the cloud feedback problem compared with aerosols over the past decade. [Robert Wood, USA] | the chapter is divided evenly between clouds, aerosols, and aerosol-cloud interactions. The new structure will have 7 subsections, with less of a dichotomy between aerosols and clouds. |
| 7-27 | 7 | 1 | 1 | 1 | 1 | The division of the discussion of aerosols is both Chapter 7 and Chapter 8 is confusing. It seems logical to have the full discussion in one place, but if that's not possible, it would be useful to clearly dilineate what is covered in Chapter 7 vs. Chapter 8. If the focus of Chapter 7 is on aerosol-cloud interactions, and not direct aerosol forcing, perhaps the chapter title could be "Clouds and Aerosol-Cloud Interactions." Estimates of the direct RF of aerosols are given in both chapters, and it confuses the discussion in Chapter 8. [Susan Anenberg, USA] | Chapter titles cannot be changed at this stage. It is made explicit that Chapter 8 relies on Chapter 7 for the aerosol RF and AF estimates. |
| 7-28 | 7 | 1 | 1 | 1 | 1 | The chapter is in very good shape for an early draft! I have also tried to include comments on where to shorten as well as add. [Daniel Murphy, United States of America] | Thanks. |
| 7-29 | 7 | 1 | 1 | 1 | | Clouds and Aerosols [Medani Bhandari, Nepal] | Not a comment. |
| 7-30 | 7 | 1 | 1 | 55 | 50 | Overall: I have read this chapter section-by-section, and did not have the chance to step back and consider broader questions of scope and completeness. I'm hoping to do more of that, if the opportunity arises, in the next round of reviews. I think the chapter does an impressive job highlighting a lot of progress in our understanding of aerosol-cloud interactions, and conveys, at least in some of the details, many of the key uncertainties. This is a monumental job in itself. And I know it is a persistent challenge for the IPCC process to give quantitative values and "error bars" where all the factors that contribute uncertainty cannot even be enumerated. I'm not sure the chapter takes the assessment of uncertainty to that level. In particular, I worry that the true uncertainty might not be reflected adequately when it comes to discussing the possibility of geoengineering. [Ralph Kahn, United States of America] | we will add sentences discussing uncertainty relative to geonegingeering to the next draft |
| 7-31 | 7 | 1 | 1 | 60 | 14 | I found the tenor of this document to be somewhat dismissive of the aerosol effects. Moreover this document seems to minimize uncertainties and seems too optimistic about the progress and the prospects for reductions of uncertainties. Until the natural aerosol sources can be better defined the anthropogenic aerosol effects cannot be determined. There has been very little progress with the natural aerosol. I spent 5 weeks in the middle of the Pacific Ocean in 2007 as far away from anthropogenic sources as possible making CCN | the point is well taken that natural aerosols do influence the atmospheric cycling and climatic effects of anthropogenic aerosols. The report does discuss natural aerosols but the focus has to be on the RF by anthropogenic aerosols. The assessment of |

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| | | | | | | measurements on 14 long flights attempting to better define the DMS source of CCN. What we found was unexpectedly high CN and CCN concentrations throughout all flights. This PASE project failed miserably because any new production of material condensed on the existing particles, which probably came all the way from South America—probably anthropogenic.<br>Hudson, J.G. and S. Noble, 2009: CCN and cloud droplet concentrations at a remote ocean site. Geophys. Res. Let., 36, L13812, doi:10.1029/2009GL038465.<br><br>[James Hudson, USA] | uncertainties has been modified and some justification has been added. |
| 7-32 | 7 | 1 | 1 | 121 | 5 | General remark: I think it is not very easily readable and it will be accessible only to experts who have the time to devote to reading a daunting text. There is no story, it is a list of both processes and climatic impacts. [Patrick CHAZETTE, France] | the chapter has been restructured into 7 sections. Some synthesising text has been added. |
| 7-33 | 7 | 1 | 1 | 121 | 5 | The report definitely would benefit from a sorting hand. For instance, the term dust should never been used without an adjective (actually most of the references cited with dust use such an adjective), for example mineral dust etc. Contributing authors have not seen how their contribution is merged into the whole chapter. It would be beneficial to any chapter if first agreed and reviewed by all authors (and not only their contribution). At several occasions I'm mentioning a new review about primary biological particles now in print in TellusB. Certainly that review was not available to the lead authors when writing their contribution. But the review article has about 32 reference pages. That indicates the vast amount of literature available. That literature could have been screened by the lead authors and certainly would indicate the important role the biological aerosol particles play also with regard to radiative forcing.<br>Having the review process in mind and the many changes expected, this review is by no means complete and in depth. [Ruprecht Jaenicke, Germany] | this chapter is not a review on "aerosols", it is an assessment on "aerosols" in the context of climate change. Natural aerosols and biogenic aerosols are covered in the chapter, although possibly not in as much details as requested by the reviewer. PBAP will be discussed in the SOD. |
| 7-34 | 7 | 1 | 1 | 121 | 5 | The report avoids the quantification of source strength (as mass) of the components of the atmospheric aerosol. There might be good reasons to do so. However, not giving source strengths "veils", how important the individual sources are and how they should be ranked. The same argument holds in terms of radiative forcing. [Ruprecht Jaenicke, Germany] | Estimate of source strength has been added in the aerosol table. Estimates of RF are only provided for anthropogenic aerosols. |
| 7-35 | 7 | 1 | 1 | 121 | 5 | The atmospheric aerosol is the "Jester" in the climate forcing discussion. As long as we do not understand the role and contribution of primary biological aerosols (at least to the level of seasalt and mineral dust), the radiative forcing error bars of the aerosol will not getting smaller. Primary biological particles scatter and absorb radiation and act perfectly as ice and condensation nuclei. Their portion in the atmospheric aerosol is sufficient large to influence those properties of the total aerosol. [Ruprecht Jaenicke, Germany] | we have added more discussion of primary biological aerosols in section 7.3. Although there is evidence for their role as IN, there is little evidence that their atmospheric concentration is large enough to have a significant direct or indirect effect. |
| 7-36 | 7 | 1 | 1 | 121 | 5 | Number concentration is the key parameter in estimating the indirect aerosol effects on climate forcing (cloud formation etc). There is only one location in the report with a number concentration (IN). How could one estimate a change in indirect effects if such numbers are not given? And if such numbers are not available, please state it. [Ruprecht Jaenicke, Germany] | we mention that there is a shift from measuring mass concentrations to number concentrations. We also discuss CCN concentrations. Given the large variability in aerosol concentrations, it is difficult and somewhat pointless to pick up and report concentrations. |
| 7-37 | 7 | 1 | 12 | 1 | 12 | Change "Steve Ghan" to "Steven Ghan" [Steven Ghan, USA] | Done |
| 7-38 | 7 | 1 | | 118 | | Generally this chapter is well written and clear. I found that there was erratic use of the certainty terminaology where the state of knowledge was summarized but not assessed using the agreed-upon terminology. When those terms are used it is not clear to me if they are used in the formal sense or that it is an accident. Also, there are a number of categorical statements (some of which are listed above) that are not referenced and appear to be either the conclusion of the authors based on a review of the test or simply their own opinions. I would prefer to see references for important statements whenever possible but if they are the conclusion after reivew this should be made clear. [Larry Thomason, United States of America] | specific comments will be answered elsewhere. We try to make it clear in the SOD what is part of our assessment. |
| 7-39 | 7 | 1 | | 121 | | The chapter on Clouds and Aerosols is well written and assesses all the aspects related to aerosols and clouds. [Ramachandran Srikanthan, India] | Thanks. |
| 7-40 | 7 | 1 | | 121 | | There are a few grammatical mistakes in the text. [Ramachandran Srikanthan, India] | OK. |

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| 7-41 | 7 | 1 | | | | In general this is much improved. I still think there is an imbalance with aerosols, maybe that is okay, but it does not really reflect their climate imporance relative to clouds. I am not a big fan of including Solar Radiation Management, but the executive summary bullets on SRM are good statements [Andrew Gettelman, USA] | I acknowledge Andrew's comment, but he is in the minority, and no modification (other than deletion) would satisfy him |
| 7-42 | 7 | 1 | | | | The FOD is much improved over the ZOD, and the authors should be congratulated. They have done a nice job summarizing some key outstanding issues (Galactic Cosmic Rays, Cloud Feedbacks, Solar Radiation Management). The dicussion of aerosols is still larger than the discussion of clouds, even though they may be less important for climate. I understand the point (aerosols have not been assessed) but it could be perhaps made more pithy. Also, as noted the definitions of quantitative aerosol-cloud interactions are difficult to comprehend, and could use some more explanation (or maybe a table) [Andrew Gettelman, USA] | Thanks. The "aerosols", "clouds" and "aerosol-clouds interactions" sections have about the same length. |
| 7-43 | 7 | 1 | | | | The recent UNEP report on short-lived forcers should be cited regarding estimates of BC forcing. If the Bounding study (IGAC/SPARC, Bond, Doherty, Forster, et al.) is submitted in time, that should definitely be included. [Marcus Sarofim, USA] | The Bond paper is now referred to, it couldn't be cited in the FOD because it wasn't submitted in time. The UNEP report is not proper peer-reviewed literature. |
| 7-44 | 7 | 1 | | | | Regarding BC, it might be good in the chapter summary to note that BC can have different interactions with clouds than more reflective and more hygroscopic aerosols. [Marcus Sarofim, USA] | The role of BC on cloud adjustment (Afari) is now mentioned in section 7.3.4.2 and in section 7.5 |
| 7-45 | 7 | 1 | | | | There's some interesting work by Jacobson, Koren, and Ten Hoeve on absorbing aerosols inhibiting cloud formation above a certain threshold of CCN: it would be good to include this as an aerosol effect? Also, Jacobson's cloud absorption & water vapor effects should be mentioned and placed into context with the other aerosol-cloud research? [Marcus Sarofim, USA] | this is now mentioned although our understanding is still limited and for that reason this effect is not included in our estimate of Afari. |
| 7-46 | 7 | 1 | | | | I found Table 7.10a and 7.10b to be particularly clear summations of aerosol-cloud interactions in AR4: could those tables be updated with sign, magnitude, LOSU, and maybe some of these additional effects? [Marcus Sarofim, USA] | we are moving away from naming and discussing each aerosol-cloud interactions for reasons explained in the chapter (eg compensating effects). LOSU is discussed in chapter 8 for the various RF mechanisms. |
| 7-47 | 7 | 3 | 1 | 4 | 43 | Specific Comments for Executive Summary: This section is too long and many details about the results are given. It needs shortened. Because, an executive summary is always brief. Besides, this section should begin with a concise statement. The authors should add. [SELAHATTIN INCECIK, TURKEY] | we will remove unnecessary material. 2-3 pages is the typical length for executive summaries in the IPCC reports. |
| 7-48 | 7 | 3 | 1 | | | MISSING from the chapter is a discussion of the likelihood that any abrupt (or not so abrupt) decrease in fossil fuel combustion needed to control GH warming would likely result in increase in temp if emissions of aerosols and precursors assoc with fossil fuel combustion were halted at the same time. This is discussed elsewhere in the report, so a cross reference might suffice. [Stephen E Schwartz, USA] | this is not really a subject for this chapter. This is dealt with in the "Near-term prediction" chapter. |
| 7-49 | 7 | 3 | 1 | | | Also missing is a quant discussion of the consequences of emissions of SO2 assoc with new power plants in China. What is the level of controls; what are the increases of SO2? Especially important because as a new plant comes on line that is emitting SO2 together with CO2, the cooling forcing of the power plant wins for 10 years or so (depends on emission ratio of SO2 to CO2 and on forcing strength of SO2; Schwartz 1993). This might be more approp in forcing chapter, with cross reference here.<br><br>Schwartz, S. E., Energy Internatl. J. 18, 1229-1248 (1993).Does fossil fuel combustion lead to global warming? (http://www.ecd.bnl.gov/steve/pubs/Fossil.pdf). [Stephen E Schwartz, USA] | chapter 7 is a "process" chapter and is not intended to discuss specific trade-offs between aerosols and GHG. |
| 7-50 | 7 | 3 | 1 | | | Also not treated adequately in the chapter is the quant examination of intensive aerosol variables such as yields (fraction of emitted precursor gas that forms aerosol) and residence times (ratio of amount in atmosphere to removal rate) and mass scattering efficiency (scat coeff per mass of material). These vary quite a bit across models, as discussed in AEROCOM papers and also in CCSP SAP 2.3, tables 3.2 and 3.3; what improvements if any since then? How well do these have to be known. Use of these intensive variables to compare models that are getting similar optical depths (extensive variable) perhaps for very different reasons. This sort of analysis should be made on the aerosol components of the several GCMs representing aerosols. I think it could be done, and would be very instructive. A lot more instructive than just reporting extensive variables. | we will add a subsection on aerosol optical properties in the SOD. |

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| | | | | | | Climate Change Science Program (U.S.). Atmospheric Aerosol Properties and Impacts on Climate, Synthesis and Assessment Product 2.3, M. Chin, R. A. Kahn, and S. E. Schwartz, Eds., Washington, DC, 2009. http://downloads.climatescience.gov/sap/sap2-3/sap2-3-final-report-all.pdf<br><br>I suggest insist that the modelers report these quantities. Make that a condition of playing in the game. Not strictly a WGI issue. [Stephen E Schwartz, USA] | |
| 7-51 | 7 | 3 | 1 | | | Related to aerosol intensive properties is the very valuable figure in Kinne ACP 06, Figure 3, which showed very different mix of composition for models almost all of which are getting more or less same optical depth. Assessment should assess whether the situation has changed appreciably?<br><br>Kinne S. et al, 2006 An AeroCom initial assessment – optical properties in aerosol component modules of global models Atmos. Chem. Phys., 6, 1815–1834, 2006 www.atmos-chem-phys.net/6/1815/2006/ [Stephen E Schwartz, USA] | there will be more AEROCOM and uncertainty discussion in the SOD. |
| 7-52 | 7 | 3 | 3 | 3 | 5 | You do not have the data to derive such a confident "average" which depends on your absurd radiation-obsessed climate models which, like a greenhouse, ignore the real climate. Clouds warm the atmosphere by the mere action of condensing water vapour and releasing latent heat. At night they warm the earth by reflectring radiatation from the earth [VINCENT GRAY, NEW ZEALAND] | This comment does not suggest any useful change. |
| 7-53 | 7 | 3 | 3 | 3 | 5 | Order of magnitude of the uncertainties on these figures ? What is the basis of those estimates ? [Michel Petit, France] | all figures now have uncertainty ranges. |
| 7-54 | 7 | 3 | 3 | | | "Clouds cool the Earth on average, by about 17 W m–2." NO. Emphatically No. Cooling (decrease in temperature) is measured in K, not W m-2.<br><br>Direct declarative sentences are good and to be commended but they have to be correct.<br><br>Clouds exert a cooling influence on Earth's climate system of 17 W m-2.<br><br>Similarly for the rest of the para.<br><br>Sloppiness such as this totally undermines the credibility of the chapter and the report. [Stephen E Schwartz, USA] | change made. |
| 7-55 | 7 | 3 | 3 | | | (infrared) ---> (terrestrial) or (thermal infrared) [Manfred Wendisch, Germany] | Infrared is unequivocal and does not require an extra modifier. |
| 7-56 | 7 | 3 | 4 | 3 | 4 | Proposal: add the sign to the number in parantheses (~+ 30 Wm-2) [Claudia Mäder, Germany] | done |
| 7-57 | 7 | 3 | 5 | 3 | 5 | I would recommend a negative sign infront of the 47. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | done |
| 7-58 | 7 | 3 | 5 | 3 | 5 | Proposal: add the sign to the number in parantheses (~- 47 Wm-2) [Claudia Mäder, Germany] | done |
| 7-59 | 7 | 3 | 7 | 3 | 10 | Confusing bullet: I suggest: "New satellite observations and advances to models simulations, that can explicitly resolve some types of cloud, have led to improved understanding of cloud interactions at the synoptic scale that impact climate processes." [Richard Allan, UK] | This bullet has been deleted. |
| 7-60 | 7 | 3 | 7 | 3 | 10 | Discordant with bullet below saying 'likely positive' on L16. Might remove the whole bullet [Andrew Gettelman, USA] | these bullets reworded. |
| 7-61 | 7 | 3 | 7 | 3 | 11 | This bullet does not belong in the Executive Summary, except for possibly the last sentence, which might be expanded. First of all, I do not see how new satellite observations have given us global cloud-resolving model simulations. Advances in computing power have done that. Second, I do not see how GCRMs have given us improved understanding of cloud interactions with meteorology and climate. If there is published evidence of this, it should be cited in the chapter. Third, these models have not been used for climate change science and | The bullet has been drastically shortened to include only a modified version of the last sentence. |

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| | | | | | | thus have no place in an IPCC report. [Anthony Del Genio, USA] | |
| 7-62 | 7 | 3 | 7 | | 9 | Now lets look at the substance of the para. The para, as revised, starts:<br><br>New satellite observations and advances to models have provided global simulations that can explicitly resolve some types of clouds. Comparison to detailed observations with such models has led to improved understanding of cloud interactions with the meteorology and the climate.<br><br>Do observations really provide global simulations? two subjects of verb "have provided"; one object, "simulations."<br><br>And change the beginning of the next sentence from:<br><br>Comparison to detailed observations with such models<br><br>(comparison of what?) to<br><br>Comparison of the output of such models with detailed observations<br><br>so that prompts examination of the previous sentence. Seems that there are two thoughts. That it should really read<br><br>New satellite observations explicitly resolve some types of clouds, and advances to models have provided global simulations that can explicitly resolve such clouds.<br><br>Here I am guessing that the clouds referred to are the same kinds, but that should be checked and explicitly stated, supporting the comparison of the modeled and observed clouds. [Stephen E Schwartz, USA] | This text reworded. |
| 7-63 | 7 | 3 | 7 | | 9 | Continuing. If the above re-wording is what is intended then we have, so far,<br><br>New satellite observations explicitly resolve some types of clouds, and advances to models have provided global simulations that can explicitly resolve such clouds. Comparison of the output of such models with detailed observations...<br><br>We can probably skip "detailed;" its just self backpatting; And we can skip the "can" while we are at it; if the models do it, permitting comparison, it is implicit that they _can_ do it.<br><br>New satellite observations explicitly resolve some types of clouds, and advances to models have provided global simulations that explicitly resolve such clouds. Comparison of the output of such models with observations...<br><br>Ok, we have begun the para; now lets look at the rest of the second sentence. It reads<br><br>"has led to improved understanding of cloud interactions with the meteorology and the climate"<br><br>Clouds are part of the meteorology. (well, what meterology, the use of "the" implies a specific one); For that matter clouds are part of the climate too. So How does a subset of something interact with the set that it is part of?<br><br>and do we want "meterorology", the discipline; or do we mean weather, which is more parallel to climate; we are not saying "climatology".<br><br>How about:<br><br>"has led to improved understanding of the interactions of clouds with other components of weather and | this text has been reworded/replaced. |

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| | | | | | | climate"<br><br>or more simply<br><br>"has led to improved understanding of the role of clouds in weather and climate"<br><br>Maybe certain interactions are intended; but if so it seems what clouds are interacting with needs to be specified. In any event it would seem essential that a lot more thought needs to be given to what is intended by these sentences. [Stephen E Schwartz, USA] | |
| 7-64 | 7 | 3 | 7 | | | And throughout the chapter. Restrict first person plural to chapter authors (if at all);<br><br>"New satellite observations and advances to models have given us global simulations"<br><br>given whom? the chapter authors? How about simply:<br><br>New satellite observations and advances to models have provided global simulations [Stephen E Schwartz, USA] | Done |
| 7-65 | 7 | 3 | 9 | 3 | 9 | Because more-constraining measurements are certainly possible, this might read: "However, current observations alone…" [Ralph Kahn, United States of America] | Change made. |
| 7-66 | 7 | 3 | 9 | | 10 | Next sentence reads:<br><br>"However observations alone do not provide a robust constraint on the sign and magnitude of cloud feedbacks."<br><br>This seems incompatible with what went before, which spoke to the advances in models that have provided global simulations that explicitly resolve such clouds. So its not observations alone.<br><br>So now the reader is left with a puzzle. We have models as well as observations. I thought the models were intended to and capable of providing such robust constraints. So something is missing here. You can represent the clouds in global models, you can compare with observations. Are the comparisons good or bad? If bad, maybe there is a direction forward to finding out why and getting to the desired robust constraints. But if the comparisons are good, then we are in a pickle. Even though different models represent clouds accurately we still can't provide the robust constraints. Now what? [Stephen E Schwartz, USA] | The meaning of this statement seems clear enough: observations by themselves do not give us the answer. There are several published studies claiming otherwise, but their analysis assumptions have been refuted. This is one of the important conclusions of the chapter. It does not preclude the value of observations in helping to constrain models indirectly. We have reworded to help clarify. |
| 7-67 | 7 | 3 | 10 | 3 | 10 | It is not clear which forcing has been considered to deduce these feedbacks, please provide a short information here, as the sign and/or the magnitude of the feedback might depend on the forcing [Andrew Ferrone, Germany] | this comment is unclear. By definition, feedbacks depend only on surface temperature change. Anything that depends on the forcing is part of a rapid adjustment. |
| 7-68 | 7 | 3 | 12 | 3 | 12 | It is not clear which forcing has been considered to deduce these feedbacks, please provide a short information here, as the sign and/or the magnitude of the feedback might depend on the forcing [Andrew Ferrone, Germany] | this comment is unclear. By definition, feedbacks depend only on surface temperature change. Anything that depends on the forcing is part of a rapid adjustment. |
| 7-69 | 7 | 3 | 12 | 3 | 14 | Unless I missed it, this bullet is longer than the entire discussion of water vapor and lapse rate feedback in the chapter, which needs to remedied. More on this later. [Anthony Del Genio, USA] | No response needed here. The bullet has been edited for clarity. |
| 7-70 | 7 | 3 | 12 | 3 | 14 | This statement is vague. For example, the robustness of what (feedback estimates?)? Is the feedback only the component from water vapor/lapse rate or is it the total feedback including clouds? Is the likelihood statement dependent (solely) on models which seems to be implied? [Haroon Kheshgi, United States of America] | clarified in revised draft. |
| 7-71 | 7 | 3 | 12 | 3 | 23 | Again based on the absurd radiation-obsessed climate models [VINCENT GRAY, NEW ZEALAND] | this comment deserves no response. |

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| 7-72 | 7 | 3 | 12 | 7 | 18 | Clarify postive/negative feedbacks. Does positive just mean higher temperature or does it mean reinforcement? [James Hudson, USA] | a positive feedback always means reinforcment; see e.g. the glossary. |
| 7-73 | 7 | 3 | 12 | | | Change "has increased" to "remains" since the postive feedback remains robust and the previous sentence did not seem to be correct English. [Richard Allan, UK] | this has been rewritten. |
| 7-74 | 7 | 3 | 12 | | | "Evidence for a net positive feedback from water vapour and lapse rate changes has increased robustness." Is this observational evidence? I will look in the chapter itself for the evidence, which if it is really to address feedback must be rooted in a change in these quantities with GMST.<br><br>"has increased robustness". Not clear robustness of what. Prev para referred to "robust constraint on the sign and magnitude of cloud feedbacks' but this para isnt dealing with clouds, so I think whatever is having its robustness increased needs to be specified.<br><br>"parameter"; I think not. A parameter is something that is adjusted, rather than something that is diagnosed. I think the quantity is "feedback strength" [Stephen E Schwartz, USA] | The relevant text was accidentally omitted from FOD. New bullet reworded and supported by text. |
| 7-75 | 7 | 3 | 12 | | | has increased robustness'; to me, the terms used in this section do not seem to strictly adhere to the degree of certainty terminaology that 'all the authors agreeed to use' based on chapter one. [Larry Thomason, United States of America] | fixed |
| 7-76 | 7 | 3 | 13 | 3 | 14 | It might not be completely clear to the reader whether the 0.9-1.06 W/m2 range in this sentence refers to net (water vapour plus lapse rate) mentioned in the previous sentence, or whether it is describing the net sum of all feedbacks except the Planck feedback (i.e. water vapour plus lapse rate, clouds and surface albedo). I think from the values quoted it must be the former. Perhaps something like "The sum of the net long term feedbacks for water vapour and lapse rate is very likely positive..." would be clearer? [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | new text added addresses this |
| 7-77 | 7 | 3 | 16 | 3 | 23 | I do not believe that the conclusions expressed in this bullet are appropriate and defensible. I applaud the courage of the authors in trying to take a step forward on cloud feedback, but they have gone too far. I will have detailed comments on the "likely positive" conclusion later. Within this bullet, however, even the description of the reason for that conflicts with what is the text later. The text concludes that the robust components are cloud top height and storm track latitudinal shifts, but the bullet includes subtropical low cloud decreases instead. The question then arises as to what "robust" means. If robust means that all models agree, or that at least no model gets a conflicting result, then that should be stated explicitly. But consensus is not understanding. There is reasonable underatanding of the cloud height feedback (with a caveat to be mentioned later) and of storm track latitudinal shifts, but not of low cloud feedback. Until such understanding exists, it is not appropriate to be confident of the feedback. [Anthony Del Genio, USA] | "Likely" means a 2/3 chance of being right. Our assessment is that the net cloud feedback is likely positive in this sense, and we will make revisions to the Chapter to strengthen the basis for this assessment. The bullet has been edited to explicitly mention the lower level of confidence in low-cloud feedbacks. cows |
| 7-78 | 7 | 3 | 16 | 3 | 23 | At the end of this paragraph, you make the point that the sign of the *net* cloud feedback is as yet uncertain. However, the rest of the paragraph seems to take a more confident position than might be warranted, given current uncertainties. It might be a bit subtle, but my thought here is that it could be more appropriate to end line 16 with "... changes are more likely positive than negative." [Ralph Kahn, United States of America] | This should be clearer now. |
| 7-79 | 7 | 3 | 16 | | 17 | "Robust positive feedback mechanisms have been established while no mechanism for strong negative global cloud feedback has convincing observational or model-based support. "<br><br>Use "while" for simultaneity; for simple contrast use "whereas", or just a semicolon. .<br><br>"established" would be strengthened by reference to models and observations. The observations would seem to require change with increasing GMST, but I await. .<br><br>It seems that "robust" is the favorite word of the day; suggest limit its use. [Stephen E Schwartz, USA] | This has been reworded. All uses of “robust” checked. |

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| 7-80 | 7 | 3 | 16 | | | A one-sentence de nition of \feedback" may be helpful for policymakers and others who won't read the full report. [Gregory Schuster, USA] | This is given in the glossary  Some words are added to the summary bullets for the benefit of casual readers. |
| 7-81 | 7 | 3 | 16 | | | "Cloud feedbacks on long-term greenhouse-gas induced surface temperature change are likely positive."<br><br>After a brief interruption to talk about water vapor and lapse rate feedback we return to cloud feedbacks. Suggest make this bullet immediately follow the earlier one about clouds. And the prev bullet just told us that there was no robust constraint on the sign, but this bullet says likely positive. Need to reconcile.<br><br>"long term"; again, feedbacks are inherently long term; strike.<br><br>" greenhouse-gas induced surface temperature change" . Not just ghg induced change in GMST; The feedback would be apply to any forcing-induced change.   [Stephen E Schwartz, USA] | This has been reorganised. |
| 7-82 | 7 | 3 | 17 | 3 | 19 | I don't believe the mechanisms describe are scientifically robust at all, and in fact the cirrus cloud top feedback is far from mature, not observed in the manner propsoed and definitely not robust. See more in depth comment 10 below   [Graeme Stephens, USA] | We cannot find any peer-reviewed literature supporting the claims of the reviewer.  We have redrafted the relevant sections to clarify the evidence behind this assessment. |
| 7-83 | 7 | 3 | 18 | 3 | 19 | The statement that cloud feedbacks are likely positive is a strong one but it's not supported by the chapter. How would one reconcile this strong language with page 7-14, line 39: "No robust feedback mechanisms involving .... low clouds have yet been established."? One would certainly not want to make this statement on the basis of climate model behavior alone.  [Robert Pincus, USA] | We now present a more explicit analysis showing how strongly this claim is supported even with reasonable allowance for uncertainty.  "Likely" only means 66% chance, where we obtain an 85% chance even with very generous inflation of uncertainty. |
| 7-84 | 7 | 3 | 18 | 3 | 19 | I am not sure if there's really  a 'robust' positive cloud feedback in the subtropics [Bennartz Ralf, US] | This is a fair comment,  Text has been reworded. |
| 7-85 | 7 | 3 | 18 | 3 | 19 | Reading through section 7.2.3.4, where the feedbacks are discussed in detail, I believe there really is not currently a 'robust' positive cloud feedback in the subtropics… I'd suggest to rephrase/change... [Bennartz Ralf, US] | This is a fair comment,  Text has been reworded. |
| 7-86 | 7 | 3 | 18 | | 19 | "The robust mechanisms include a rise in the heights of cirrus cloud tops and a reduction in subtropical cloudiness."<br><br>Need to specify "with time" or better "with increase in GMST" or whatever is the indep variable.  [Stephen E Schwartz, USA] | this is now worded differently. |
| 7-87 | 7 | 3 | 18 | | | State clouds are largest uncertainty [Andrew Gettelman, USA] | Done |
| 7-88 | 7 | 3 | 19 | 3 | 20 | About the sentence: "The range of the cloud feedback parameter in the CMIP5 models is [xx to yy] W m–2  K–1". Do not forget to replace xx and yy by the corresponding numbers.  [Rubén D Piacentini, Argentina] | thanks |
| 7-89 | 7 | 3 | 19 | | 20 | The range of the cloud feedback parameter in the CMIP5 models is [xx to yy] W m-2 K-1 One expects to see the values; and sign; And as noted above"feedback strength"; not "feedback parameter." And elsewhere in this chapter.  [Stephen E Schwartz, USA] | fixed |
| 7-90 | 7 | 3 | 19 | | | "a rise in the height of cirrus clouds" will only generate postive cloud feedback if the cloud top temperature declines relative to the surface temperature. It is more accurate to state "an approximately isothermal rise in the altitude of cirrus" [Richard Allan, UK] | for clarity, the original wording has been retained. |
| 7-91 | 7 | 3 | 20 | 3 | 20 | The values are missing [Patrick CHAZETTE, France] | fixed. |
| 7-92 | 7 | 3 | 20 | | | "Inconsistent prediction"; Careful here. Do you mean "prediction" vs "projection" (see chapter 11 for chapter and verse on this). Or do you really mean "representation" which is present time vs changes in future. [Stephen E Schwartz, USA] | wording changed |
| 7-93 | 7 | 3 | 22 | | 23 | "Since all cloud types are crudely represented by climate models, values outside the current spread of climate | this has been rworded. |

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| | | | | | | models cannot be ruled out."<br><br>Better "As" or "Because" than "Since"; in formal writing "since" means subsequent to.<br><br>Substantively, try the following sentence:<br><br>Additionally, because of the crude representation of clouds in current climate models, values outside the current spread of climate models cannot be ruled out.<br><br>But still need to specify values of what. values of climate sentitivity; values of cloud feedback strength? climate sensitivity?<br><br>Additionally, because of the crude representation of clouds in current climate models, values of climate sensitivity outside the current spread of climate models cannot be ruled out.<br><br>And then is it the current spread of climate models or the spread of current climate models; I think the latter.<br><br>Additionally, because of the crude representation of clouds in current climate models, values of climate sensitivity outside the spread of current climate models cannot be ruled out.<br><br>And perhaps better "range" than "spread"<br><br>Additionally, because of the crude representation of clouds in current climate models, values of climate sensitivity outside the range of current climate models cannot be ruled out. [Stephen E Schwartz, USA] | |
| 7-94 | 7 | 3 | 23 | | | "cannot be ruled out" sounds as if it is very unlikely. I don't think we know how likely or unlikely it is. A more modest wording would be appropriate. [Henning Rodhe, Sweden] | this is more explicit now. |
| 7-95 | 7 | 3 | 25 | 3 | 27 | It is noted that the statement on "observations, theoretical considerations and models that indicate that the strength of extreme precipitation events which can cause flooding, tends to increase is in contradiction with the statement in the executive summary of chapter 2, page 5, lines 7 to 9. There is a clear need for further clarification which should be provided in both chapters, 2 and 7. [Klaus Radunsky, Austria] | This has been reworded. Also there is new evidence from trends presented in chapter 2 that will likely make their own conclusions stronger now. |
| 7-96 | 7 | 3 | 25 | 3 | 27 | "strongly": 1) is vague quantaitatively, and 2) exceeds the current capability of majority of GCMs. Also, without a clearly indicated domain the first sentence conflicts with the second one. [Chien Wang, United States of America] | wording change. |
| 7-97 | 7 | 3 | 25 | 4 | 41 | Based on absurd maoels and biased opinion [VINCENT GRAY, NEW ZEALAND] | useless comment. |
| 7-98 | 7 | 3 | 25 | | 27 | It seems that a more quantitative assessment of extreme precipitation is lacking in this paragraph. I agree with the last sentence of this paragraph, but the previous sentence can be more quantitative. For example, the rate of extreme precipitation intensity with surface warming exceeds that implied by the Clausius-Claperon relationship for the atmospheric humidity increase. You may include another sentence at the end of paragraph to provide a direct comparison to the present climate. “Future extreme precipitation events are likely more severe than those occurred in the past few years.” [Kuan-Man Xu, USA] | The wordig has been changed, but we do not want to make prejections in this chapter so have not adopted the suggested wording. |
| 7-99 | 7 | 3 | 26 | 3 | 26 | Would it be possible to quantify the "strongly" ? [Michel Petit, France] | changed. |
| 7-100 | 7 | 3 | 26 | | | "which can cause flooding"; phrase can be omitted; this is known to most people and weakens the statement. [Stephen E Schwartz, USA] | agreed |
| 7-101 | 7 | 3 | 27 | | | moisture content rather than humidity (i.e. not relative humidity) [Henning Rodhe, Sweden] | wording changed. |
| 7-102 | 7 | 3 | 29 | 3 | 30 | "current levels of coverage": please clarify which coverage is considered here [Andrew Ferrone, Germany] | This seems self-explanatory. |
| 7-103 | 7 | 3 | 29 | 3 | 32 | In particular for the forcing by the aviation sector, emphasis should be put on the regional differences of the | this is now noted, though true of other forcings too. |

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| | | | | | | forcing, which is much larger than the global one. [Andrew Ferrone, Germany] | |
| 7-104 | 7 | 3 | 29 | 3 | 32 | In this paragraph it is stated, that contrails and contrail-cirrus from aircraft exert only a small adjusted forcing of 0.03 Wm-2. However, this AF is of the same magnitude as the forcing from CO2-emissions from aircraft. Should the cirrus AF really be expressed as “only small”? [Claudia Mäder, Germany] | this has been changed. |
| 7-105 | 7 | 3 | 30 | 3 | 30 | Please specify for which reference year the AF of contrails and contrail-cirrus is given here. [Andrew Ferrone, Germany] | Done. |
| 7-106 | 7 | 3 | 30 | 3 | 30 | Defintion of adjusted forcing should be given in a foot-note [Michel Petit, France] | agreed, RF and AF defined at start of ES in revision |
| 7-107 | 7 | 3 | 36 | 3 | 38 | Trivial statement [Henning Rodhe, Sweden] | agreed, text reworded to assess rather than describe CMIP5 models |
| 7-108 | 7 | 3 | 36 | | | Second sentence might be introduced by "However, ..." [Stephen E Schwartz, USA] | agreed,text reworded to assess rather than describe CMIP5 models |
| 7-109 | 7 | 3 | 38 | 3 | 38 | Clearly define RF. I think this means anthropogenic? For instance the numbers in L3-5 are natural and therefore not forcings? [James Hudson, USA] | RF and AF defined at start of ES in revision |
| 7-110 | 7 | 3 | 40 | | 41 | "with all ranges indicative of 5%-95% confidence intervals"; should be parenthetical (after first range) and not a part of the first sentence of the bullet. [Stephen E Schwartz, USA] | Agreed, confidence interval now written inline with IPCC guidence |
| 7-111 | 7 | 3 | 40 | | | "direct effect"; I think "direct forcing " is meant; Better "Direct forcing by aerosols" (no "the") [Stephen E Schwartz, USA] | Agreed, text reworded |
| 7-112 | 7 | 3 | 41 | | | Its not totally clear to me where in the chapter the best estimate of aerosol DRF is derived. Sulphate forcing seems to be lower than in AR4. It might be also possible that the BC forcing is more positive and would result in a less negative total RF. [Michael Schulz, Norway] | agreed, Rfari estimate more carefully made in SOD |
| 7-113 | 7 | 3 | 42 | 3 | 43 | Again a bit subtle, but given the need to improve observational constraints on the models, perhaps it might be more appropriate to reword something like: "… using results from aerosol models and limited constraint from observations." More generally, I'm thinking it is especially important for the IPCC to clearly convey the pressing need for better observational constraints on aerosol amount and properties, and for better integration of available constraints into climate models. Both require considerable work by several research communities as well as next-generation measurement capabilities, though at present, resources for accomplishing these advances are actually *diminishing,* rather than increasing or even remaining stable. [Ralph Kahn, United States of America] | Generally agreed on rewording approach. Note that role is not to define research goals here. However, text has been more carefully worded to assess role of observations |
| 7-114 | 7 | 3 | 48 | 3 | 54 | This RF statement is not very systematic, it is rather selective. If components are left out, like sea salt and biological particles, that should be stated clearly.<br>If page limit is a problem, then balancing the chapter is much needed.<br>If you discuss only components used in current large-scale models, that limits any conclusion to preconceived ideas and excludes newer ideas. Please then point to limitations of your discussion.<br>If you discuss only components with scientific progress since the last IPCC report, certainly there is not much progress with biological particles. However, since they are not mentioned at all in the last report, there is much progress in the awareness of this component. [Ruprecht Jaenicke, Germany] | focus is on anthropogenic changes, but point is well made and more context has been added |
| 7-115 | 7 | 3 | 48 | 3 | 54 | A table would be much better. [Ruprecht Jaenicke, Germany] | interesting idea I like it |
| 7-116 | 7 | 3 | 48 | 3 | 54 | It should be mentioned, that the numbers refer to the direct RF. [Claudia Mäder, Germany] | agreed, text reworded |
| 7-117 | 7 | 3 | 48 | | | State: these are direct effects? [Andrew Gettelman, USA] | yes, text reworded |
| 7-118 | 7 | 3 | 48 | | | preface RF with "direct" [Warren Richard Leaitch, Canada] | agreed, text reworded |
| 7-119 | 7 | 3 | 50 | 3 | 51 | Note : the knowledge of the vertical profile is also an important uncertainty source for all the aerosol species (chemical components). [Patrick CHAZETTE, France] | agreed, the vertical structure point now made more generally |

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| 7-120 | 7 | 3 | 50 | | | It is probably overempasizing to single out as "largest" uncertainty the vertical distribution of BC. Other factors, such as emissions,removal and transport, optical properties etc - especially when combined - are responsible for large uncertainty in BC burdens. [Michael Schulz, Norway] | agreed a more comprehensive uncertainty estimate is now made |
| 7-121 | 7 | 3 | 51 | 3 | 51 | The RF of biomass-burning OC should also be given, for consistency with the RF numbers given for BC. [Nicolas Bellouin, United Kingdom] | agreed biomass burning from OC now added |
| 7-122 | 7 | 3 | 55 | 4 | 2 | The term "equivalent radiative forcing" looks not very scientific. The sentence "This radiative forcing is 2-4 time more effective at causing global mean temperature changes than an equivalent radiative forcing from CO2" can be removed. Since temperature change depends not only on radiative forcing, but also on the feedback in climate system, and the mechanism for BC/BrownC snow/ice forcing and CO2 forcing is different, it is not suitable to simply use the climate efficacy (change in temperature per unit forcing) to describe the contrast difference between anthropogenic absorbing aerosols BC/BrownC snow/ice forcing and CO2 forcing. [Tijian Wang, China] | text reworded but retained. The point being made is associated with the first order difference which is related to the efficacy, but other differences are ackowledged in the reworded text. Especiallly the role at high latitudes and on snow have been highlighted |
| 7-123 | 7 | 3 | 55 | 4 | 2 | The mechanism for BC/BrownC snow/ice forcing and CO2 forcing is different, so it is not suitable to simply use the climate efficacy to describe the difference between anthropogenic absorbing aerosols BC/BrownC snow/ice forcing and CO2 forcing. [Hua Zhang, China] | text reworded but retained. The point being made is associated with the first order difference which is related to the efficacy, but other differences are ackowledged in the reworded text. |
| 7-124 | 7 | 3 | 56 | 3 | 56 | I slightly have problem in putting BC and brown carbon here in parallel as two independent viariables. Actually BC( I guess it is EC here?) from emission sources were generally also brown carbon. [Xuemei Wang, China] | agreed, text now clarified as absorbing aerosol and types now specified explicitly |
| 7-125 | 7 | 3 | 56 | 4 | 2 | I do not understand the exact meaning of this sentence. [Patrick CHAZETTE, France] | text reworded for clarity |
| 7-126 | 7 | 3 | 56 | 4 | 2 | I do not clear see the point to emphasize the global mean forcing here. This quantity is rather small and does not reflect the regional effects. Relating it to the global mean response leads to one of the weakest aspects of current knowledge regarding aerosol climate effects. Why not just focus on the local effectn instead. [Chien Wang, United States of America] | text reworded but retained. The point being made is associated with the first order difference which is related to the efficacy, but other differences are ackowledged in the reworded text. Especiallly the role at high latitudes and on snow have been highlighted |
| 7-127 | 7 | 3 | 56 | 4 | 2 | Concerning “radiative forcing of BC in snow is 2-4 times more effective at causing global mean temperature changes than an equivalent radiative forcing from CO2,” I feel it is inappropriate to put this into Executive Summary. Comparing with other items in ES, this idea didn't get strong support from multiple literatures, and in the arctic snow and ice the fact of decreasing trend of BC (see in Ch08) must be respected. A continuous decreasing trend of BC concentration on the snow surface will definitely result in declining of darken the snow and produced warming if had. This constantly reduced effect should not be mentioned in ES. [Bin Zhu, China] | change is related to size of forcing since pre industrial times and this is the time period considered here. However, text has been reworded for clarity. |
| 7-128 | 7 | 3 | 56 | | | State: these are direct effects? [Andrew Gettelman, USA] | agreed. Now explitly stated |
| 7-129 | 7 | 3 | | 8 | | Exec Summary: In linking clouds and aerosols I consider that it is essential to note that there are likely to be fast cloud responses, akin to aerosol indirect effects (e.g. Gregory and Webb 2008 J Climate) [Richard Allan, UK] | agreed, link made |
| 7-130 | 7 | 3 | | 121 | | Chapter 7 is carried out with care and authority and contains very interesting elements. This chapter is interesting and comprehensive in treating the subject; it gives a valid contribution to the studies of clouds and aerosols. The chapter sections and its ideas are well arranged and carried out with care and authority. The results are in good agreement with other studies concerning with these issues. The authors discussed all their results in details. The illustrations are very clear and high resolution and thirty nine pages contains a comprehensive list of references are given. [Prof. Dr. S. M. Robaa, Egypt] | Thanks. |
| 7-131 | 7 | 3 | | | | There is a notable difference in the level of detail provided in the first five bullet points on clouds, which convey general results, and bullet points 6-10 and 13, which provide detailed numerical results along with uncertainty ranges. It would be useful to harmonize the style here. Readers often prefer general information to specifics in a summary. [Robert Pincus, USA] | agreed, statement harmonised |
| 7-132 | 7 | 4 | 4 | 4 | 7 | It could help the reader to specify that these feedbacks exclude cloud-aerosol interactions, otherwise this might seem contradictory to the following paragraph [Andrew Ferrone, Germany] | agreed, text reworded to show direct effect focus |

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| 7-133 | 7 | 4 | 5 | 4 | 5 | "low agreement in" is poorly worded. Better: "little agreement between" [Jón Egill Kristjánsson, Norway] | agreed text reworded |
| 7-134 | 7 | 4 | 9 | 4 | 20 | I suggest a bullet saying that new research is moving from the effect of aerosols on individual clouds to effects on cloud systems and that in some cases this has led to reduced estimates of the aerosol indirect effects. [Daniel Murphy, United States of America] | Not sure that this statement is true. |
| 7-135 | 7 | 4 | 10 | | | change "mixed phase" to "mixed-phase" for consistency; there are several other places in this chapter that need the same correction. [Hailong Wang, USA] | agreed |
| 7-136 | 7 | 4 | 15 | 4 | 16 | Where is the upper bound? [Patrick CHAZETTE, France] | bullett item reworded |
| 7-137 | 7 | 4 | 15 | 4 | 19 | The summary would be clearer and more accurate if it specified that the semi-direct effect differs from Cloud Absorption Effects I and II, which are the effects on cloud heating of absorbing inclusions in hydrometeor particles and of absorbing aerosol particles interstitially between hydrometeor particles at their actual relative humidity (RH), respectively. These are not part of the semidirect effect and have not been treated as part of the semidirect effect in the literature, as discussed in detail in Jacobson, M.Z., Investigating cloud absorption effects: Global absorption properties of black carbon, tar balls, and soil dust in clouds and aerosols, J. Geophys. Res., doi:10.1029/2011JD017218, in press, 2012, http://www.agu.org/pubs/crossref/pip/2011JD017218.shtml. The semi-direct effect is defined as the "change in cloudiness due to the decrease in near-cloud relative humidity and increase in atmospheric stability caused by absorbing aerosol particles below, within, or above a cloud [Hansen, J., M. Sato, and R. Ruedy (1997) Radiative forcing and climate response, J. Geophys. Res., 102, 6831-6864.7; Ackerman, A. S., O.B. Toon, D.E. Stevens, A.J. Heymsfield, V. Ramanathan, and E.J. Welton (2000) Reduction of tropical cloudiness by soot, Science, 288, 1042-1047; Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, J. Geophys. Res., 107, (D19), 4410, doi:10.1029/ 2001JD001376, 2002). This definition is given in Section 3.7 of Jacobson (2002) and applies only to aerosols at the RH of the ambient air, not at the RH of the cloud, as discussed in Jacobson (2012), above. [Mark Z. Jacobson, U.S.A.] | All adjustments are summarized in the AF estimates but are not detailed in the executive summary due to space limitations. |
| 7-138 | 7 | 4 | 15 | 4 | 19 | A new and confusing terminology is used here and throughout this chapter. The IPCC should use the same terminology as the published literature. Please change this. As it stands, this section is not even consistent with chapter 8! The phrase “indirect adjusted forcing” gets only 3 google hits. Also, please make it clear that this para refers to aerosols. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland] | new terminology has been introduced |
| 7-139 | 7 | 4 | 15 | 4 | 19 | I encourage you to consider a best estimate for indirect aerosol effect [Gunnar Myhre, Norway] | there is now a best estimate for Afari+aci |
| 7-140 | 7 | 4 | 15 | 4 | 19 | I am concerned about this statement. This statement is based on the last paragraph in Section 7.4.6 (Synthesis of Aerosol Effects, page 49, lines 25-40). The lower bound of -1 W/m-2 for iRF is based on the AVERAGE of the GCM estimates. It is not convincing to me that you can use the AVERAGE of the GCM estimates as the lower bound. It makes much more sense to use the lowest 95 percentile or 90 percentile of the GCM estimates as the lower bound(I think this is what the AR4 did). The other lower bounds are also derived in the same way. This approach is really troubling, and therefore the smaller uncertainty cited here compared with those in the AR4 can be misleading. [Minghuai Wang, United States of America] | the chain of arguments has been changed and now the CMIP5/ACCMIP/Aerocom model mean amongst other lines of evidence is used to provide a best estimate of Afari+aci |
| 7-141 | 7 | 4 | 15 | | | -1 is the lower bound? Not obvious. [Andrew Gettelman, USA] | changed, -1 W/m2 now is the best estimate of Afari+aci |
| 7-142 | 7 | 4 | 15 | | | Lower absolute number or lower - more negative - number? [Henning Rodhe, Sweden] | bullett item rewritten |
| 7-143 | 7 | 4 | 15 | | | This entire paragraph, lifted from the main text is confusing and doesn’t make much sense - both here and in the main body of the chapter. For exampl the range -1 to -.1 is clear but the lower bound is then described as -0.4-to -0.2 which doesnt jive with the lower bound of the stated range which strictly is -1. Similar confusion exists for rest of the paragraph. [Graeme Stephens, USA] | bullett item rewritten |
| 7-144 | 7 | 4 | 16 | 4 | 17 | This should probably be an upper bound instead of lower bound [Gunnar Myhre, Norway] | bullett item rewritten |
| 7-145 | 7 | 4 | 17 | | | Not sure how these two pairs of numbers relate? Awkward phrasing [Andrew Gettelman, USA] | bullett item rewritten |

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| 7-146 | 7 | 4 | 17 | | | I don't think argumentation is an english word. [Andrew Gettelman, USA] | bullett item rewritten |
| 7-147 | 7 | 4 | 17 | | | line of argument (not argumentation) [Robert Pincus, USA] | bullett item rewritten |
| 7-148 | 7 | 4 | 18 | 4 | 18 | Why two ranges of assessement? [Patrick CHAZETTE, France] | bullett item rewritten |
| 7-149 | 7 | 4 | 19 | | | Again, I do not understand two sets of numbers [Andrew Gettelman, USA] | bullett item rewritten |
| 7-150 | 7 | 4 | 19 | | | Ice? (see comments later). [Andrew Gettelman, USA] | there is insufficient literature to provide an estimate of aerosol effects on ice clouds that it warrants an entry in the executive summary. It is now mentioned in section 7.5 |
| 7-151 | 7 | 4 | 23 | | | "medium evidence" awkward; it appears that this terminology is used throughout the report and is specified in some overall guideline. Despite that, it is still awkward. Evidence can be strong; it can be weak; but cannot be medium. It can be of medium strength. "Medium", referring to evidence, requires some indication of the attribute of the evidence that it is modifying; it could, for example be referring to the directness of the evidence, rather than the strength of the evidence. So "medium" cannot stand alone. You might wish to introduce some measure of strength of evidence; high, medium, low. Just like a level of scientific understanding, high, medium, low. [Stephen E Schwartz, USA] | we follow IPCC guidance note on uncertainty here |
| 7-152 | 7 | 4 | 26 | 4 | 27 | I suggest deleting the first sentence on this bullet. Cosmic ray ionization has been well established for some time. [Daniel Murphy, United States of America] | bullet has been rewritten. |
| 7-153 | 7 | 4 | 26 | 4 | 28 | Some quantification of this is highly desirable. [K KRISHNA MOORTHY, INDIA] | bullet has been rewritten. Ionization rate is not directly relevant to climate, so quantification in the exec summary is not required. |
| 7-154 | 7 | 4 | 26 | | | Should empahsize the 'robust effects of cosmic rays' are only in laborarorty conditions, whereas in the free atmosphere the effects are far from 'robust' [Graeme Stephens, USA] | text changed to avoide word "robust" |
| 7-155 | 7 | 4 | 30 | 4 | 31 | About the sentence: "From a physical-science assessment basis, model studies, observations of the effects of volcanic<br>eruptions, and physical arguments suggest that some Solar Radiation Management (SRM) strategies for geoengineering may be effective in offsetting the global average surface temperature increase." If the AR5-WRI will talk about Geoengineering, it will be important to consider all forms of them (plant cultivation with ligther colors, reflexion of solar radiation at ground and from space,etc). [Rubén D Piacentini, Argentina] | Agreed. Other strategies are now discussed in the executive summary and chapter. |
| 7-156 | 7 | 4 | 30 | | | SRM stragtegies have not been demonstrated as being in anyway effective or even partially effective -large uncertainties exist both with cloud brightening and with stratospheric aerosol - the former has all sorts of uncertainties that arent yet understood (see comments below re indirect rffects) and the latter too has effects involving influences of staropsheric aeroosl on cirrus that are just not yet quantified but can be shown to be important in cases. [Graeme Stephens, USA] | Agreed. These issues are now dealt with in the executive sumarry and throughout the section |
| 7-157 | 7 | 4 | 33 | 4 | 33 | The word "inexact" lacks meaning. I suggest instead rephrasing the sentence as follows: "The spatial distribution of the RF from SRM would be quite different from that by greenhouse gases, which would produce regional differences in temperature and precipitation patterns." [Steven Ghan, USA] | It is not just the spatial difference, but also the temporal as discussed in the FAQ. This detail cannot be discussed in the executive summary, but it is important to communicate that SRM will not precisely cancel GHG forcing. |
| 7-158 | 7 | 4 | 35 | 4 | 35 | There are huge uncertainties and risks associated with the possible environmental impacts of large-scale geoengineering. My suggestion here is rewording something like "… and will likely have other, unanticipated impacts on the climate system…" [Ralph Kahn, United States of America] | Agreed. See new bullets |
| 7-159 | 7 | 4 | 36 | 4 | 37 | Proposal to add: “Termination of SRM (for example in the case of serious, undesirable side effects) would produce a reappearance of most of the avoided global warming within about a decade. [Claudia Mäder, Germany] | termination is now discussed. |

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| 7-160 | 7 | 4 | 39 | 4 | 39 | Evidence suggests, not suggest. [Steven Ghan, USA] | Agreed |
| 7-161 | 7 | 4 | 39 | 4 | 43 | Give an indication on the life time of these stratospheic aerosols. [Michel Petit, France] | Some issues of timescale are now discussed. |
| 7-162 | 7 | 4 | 40 | 4 | 41 | Suggest rewording something like: " Evidence from major volcanic eruptions in the past, as well as modeling studies, suggests that increasing the amount of aerosol in the stratosphere can temporarily inrease Earth's albedo to counteract… doubling of CO2 (within…" This would highlight, at least qualitatively, both the magnitude of the required aerosol injection and the transience of its hoped-for beneficial effect. [Ralph Kahn, United States of America] | these issues are now discussed in more detail |
| 7-163 | 7 | 4 | 41 | 4 | 43 | About the sentence: "The effectiveness and potential of SRM through cloud brightening is more uncertain than through stratospheric aerosol injection because of our limited understanding of aerosol indirect effects on clouds". I consider that a general statement about the limited knowledge of the possible feedback effects of all types of Geoengineering projects related to climate change should be added so that it can be clear that the present AR5-WRI do not suggest to consider these two techniques to be possible in the present time (as can eventually be deduced at least for the latter in this sentence). [Rubén D Piacentini, Argentina] | See response to comment 163. We now indicate that the LOSU for any geoengineering strategy is very low in this bullet. |
| 7-164 | 7 | 4 | 43 | 4 | 43 | "on clouds" is superfluous and confusing. Both the direct and indirect effects refer to aerosol effects on climate, so "on clouds" does not make sense. The indirect effect, however, works via clouds. Suggestion: either skip "on clouds" or replace it by "via clouds". [Jón Egill Kristjánsson, Norway] | Agreed. Dropped the words "on clouds" |
| 7-165 | 7 | 5 | 3 | 5 | 5 | About these sentences: "The atmosphere, although mostly composed of gases, is full of particles. It is usual to partition these particles into cloud particles, atmospheric aerosols, and falling hydrometeors according to their size, water content and sedimentation velocity". Even if aerosol can be covered by a layer of water, a large variety is almost without water, so the classification must include the mass content. Consequently, please replace "water content" by "water and other mass contents". [Rubén D Piacentini, Argentina] | We have added "chemical composition" as a way to distinguish the different types of particles. |
| 7-166 | 7 | 5 | 3 | | 5 | "The atmosphere, although mainly composed of gases, is full of particles. It is usual to partition these particles into cloud particles, atmospheric aerosols, and falling hydrometeors according to their size, water content and sedimentation velocity."<br><br>Let me suggest a re-write of the above para; why? awkwardness; casual language "full of particles" vs formal "hydrometeors" that imply differing levels of familiarity on the part of the reader. And some value added in the last sentence which motivates the importance of clouds and aerosols to the subject at hand.<br><br>In addition to gases, which comprise most of the atmosphere, the atmosphere also contains liquid and solid matter in the form of particles. It is usual to distinguish these particles according to their size, composition (water content), and sedimentation velocity into atmospheric aerosol particles, cloud particles, and falling hydrometeors (rain, snow, ...). Despite their small mass or volume fraction, particles in the atmosphere exert major influences on radiation, mass transport, weather, and climate. [Stephen E Schwartz, USA] | thank you for the suggestion. The text was rewritten to "The atmosphere is mostly composed of gases, but also contains liquid and solid matter in the form of particles. It is usual to distinguish these particles according to their size, chemical composition, water content and sedimentation velocity into atmospheric aerosol particles, cloud particles and falling hydrometeors. Despite their small mass or volume fraction, particles in the atmosphere exert major influences on radiation, precipitation, weather and climate." |
| 7-167 | 7 | 5 | 3 | | | Imbalanced: mostly about aerosols and clouds. Need a bit more summary of clouds and precipitation first. [Andrew Gettelman, USA] | precipitation is now mentioned. Hydrometeors are also mentioned. |
| 7-168 | 7 | 5 | 3 | | | Replace first sentence by: "The atmosphere is composed of gases and particles." [Manfred Wendisch, Germany] | rephrased as explained in reply to 7-168. |
| 7-169 | 7 | 5 | 7 | 5 | 17 | Much of this paragraph is stated better on page 7-7 lines 5ff. [Daniel Murphy, United States of America] | Keep both. |
| 7-170 | 7 | 5 | 7 | | | expands ---> adiabatically expands [Manfred Wendisch, Germany] | done. |
| 7-171 | 7 | 5 | 8 | | | "Cloud particles are generally larger than aerosols"; better "Cloud particles are generally larger than aerosol particles"; yes, it is tedious but it is important. the aerosol is the system consisting of the particles and the surrounding air; so the comparison is illogical; there might be places where it is ok to lapse, but this sentence doesnt seem like one of them. Especially at the beginning. Precision in language is a strength of writing, not a weakness. [Stephen E Schwartz, USA] | done. |

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| 7-172 | 7 | 5 | 8 | | | aerosols ---> aerosol particles [Manfred Wendisch, Germany] | done. |
| 7-173 | 7 | 5 | 9 | | | water ---> liquid water or ice (solid water) [Manfred Wendisch, Germany] | done. |
| 7-174 | 7 | 5 | 9 | | | delete "which is usually a visible body" [Manfred Wendisch, Germany] | Done |
| 7-175 | 7 | 5 | 12 | 5 | 12 | Add "hailstone" after "graupel". [Chien Wang, United States of America] | done. |
| 7-176 | 7 | 5 | 13 | | | the flow of radiation at the top of atmosphere' yes but also within the atmosphere and at the surface and important feedbacks that relate to the hydrological cycle are infleucned by these effects that occur deeper into the column than just TOA - this is in fact a serious omission from the chapter [Graeme Stephens, USA] | "at the top of atmosphere" has been changed to "in the atmosphere". Latent heat release is also mentioned. Minor additional rewording. |
| 7-177 | 7 | 5 | 13 | | | replace "flow of radiation" by "flux density of radiant energy" [Manfred Wendisch, Germany] | changed to "radiative fluxes" |
| 7-178 | 7 | 5 | 16 | 5 | 16 | The description of "frequency and distribution…is...sink of aerosol" is incorrect. The sink of aerosols should be precipitation, not its freqency and distribution. The frequency and distribution of precipitation determines to a certain level the lifetime and spatial distribution of aerosols. [Chien Wang, United States of America] | rephrased as "but also because precipitation influence, and is influenced, by the concentrations and properties of aerosol particles" |
| 7-179 | 7 | 5 | 16 | | 17 | "because the frequency and distribution of precipitation is an important sink of aerosol particles"; no it is not frequency and distribution which is the sink; [note also gram: plural subject "frequency and distribution" requires plural verb]<br><br>I think the following would be better; it also gets in the two-way coupling:<br><br>"because the frequency and distribution of precipitation, the major means of removal of aerosol particles from the atmosphere, influence, and are influenced by, the concentrations and properties of aerosol particles.:<br><br>Or perhaps even better:<br><br>"Precipitation is the major means by which aerosol particles are removed from the atmosphere. Because the frequency and distribution of precipitation influence, and are influenced by, the concentrations and properties of aerosol particles, ..." [Stephen E Schwartz, USA] | rephrased as "but also because precipitation influence, and is influenced, by the concentrations and properties of aerosol particles" |
| 7-180 | 7 | 5 | 19 | 5 | 19 | Should state the size of aerosol particles here as 'relatively small' is vague. Relative to what? [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | "relatively" has been omitted. |
| 7-181 | 7 | 5 | 19 | | | This introduction is imbalanced between clouds and aerosols. Need another paragraph here that briefly summarizes cloud feedbacks, and identifies them as the largest uncertainty, mentioning Stephens 2005, Bony et al 2006 and IPCC 2007 here. Note excutive summary that cloud forcing is net 17 Wm-2, so small perturbations are large feedbacks. [Andrew Gettelman, USA] | We do not agree with this comment. Among the first four paragraphs, aerosols and clouds have one paragraph each while two paragraphs are about both. Clouds are discussed in further detail in paragraph 5. |
| 7-182 | 7 | 5 | 21 | | | Replace "through absorption, scattering and emission" by "mostly through absorption and emission" [Manfred Wendisch, Germany] | text unchanged. Scattering is often overlooked and is not negligible for dust. |
| 7-183 | 7 | 5 | 22 | 7 | 31 | To shorten I suggest deleting "They also .. aerosol layers (lines 22 to 28) and eliminating the paragraph break. [Daniel Murphy, United States of America] | suggestion declined. |
| 7-184 | 7 | 5 | 26 | | | Replace "Cloud and aerosol amounts and properties" by "Cloud and aerosol particles" [Manfred Wendisch, Germany] | it is fine here to omit the "particles" |
| 7-185 | 7 | 5 | 27 | | | Variations in the horizontal can not be rapid - that's a temporal measure. “Large-amplitude, small-scale,” perhaps? Just “fine-scale” would work. [Robert Pincus, USA] | changed to "fine-scale" |
| 7-186 | 7 | 5 | 29 | 5 | 31 | The statement about the albedo continuum is not universally accepted. In most circumstances the division between clear and cloudy skies is unambiguous *given high enough spatial resolution and sensor sensitivity* as a consequence of droplet activation. I am unaware of any measurements that show, for example, substantial concentrations of particles between the size of large aerosols (say, 1 micron, to be generous) and small cloud drops (say 6 microns). It would be fair to say that it can be challenging to distinguish clouds from | we now say that this may happen at times. |

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| | | | | | | aerosols with passive remote sensors at current resolutions. [Robert Pincus, USA] | |
| 7-187 | 7 | 5 | 33 | 5 | 3 | Change "a major source" to "major sources" because of the very different influence of clouds and aerosols, with cloud feedback being a major source of uncertainty in climate sensitivity, and aerosol forcing being a major source of uncertainty in the total radiative forcing of climate change. [Steven Ghan, USA] | done. |
| 7-188 | 7 | 5 | 33 | 6 | 34 | Everything has to be interpreted from your absurd model, which ignores the real climate of meteorology [VINCENT GRAY, NEW ZEALAND] | comment unclear. Define what is meant by 'climate of meteorology'. |
| 7-189 | 7 | 5 | 34 | | | I wouldn't call the feedbacks "positive or negative", rather call them "warming or cooling". [Manfred Wendisch, Germany] | it is usual to call feedbacks positive or negative. |
| 7-190 | 7 | 5 | 36 | 5 | 36 | Suggest reformulating "surrounding climate change" to "associated with our understanding of changes in the climate system" [Andrew Ferrone, Germany] | Accept. |
| 7-191 | 7 | 5 | 37 | 7 | 38 | Suggest deleting phrase starting with "but with the exception…" [Daniel Murphy, United  States of America] | Done. Agree. |
| 7-192 | 7 | 5 | 38 | 5 | 40 | There is no mention in this sentence of stratocumulus, one of the largest if not the largest contributor to model radiation errors and cloud feedback disagreement, nor of the cumulus-stratocumulus transition.  How can this be? [Anthony Del Genio, USA] | Accept. |
| 7-193 | 7 | 5 | 46 | | | "radiative forcing of climate"; NO. Better: "responsible for a radiative forcing of climate _change_".  It is climate change which is being forced, not climate.  [Stephen E Schwartz, USA] | done |
| 7-194 | 7 | 5 | 47 | | | change the end of the sentence: "with clouds)." ---> "with clouds and respective radiative consequences)." [Manfred Wendisch, Germany] | sentence was rephrased for the new terminology. |
| 7-195 | 7 | 5 | 48 | 6 | 2 | This text is quite 'heavy'. It is scientifically sound, but it does not read well. Especially, the sentence "While previous attempts …." is too dense, and needs to be reformulated. [Jón Egill Kristjánsson, Norway] | text has been changed slightly towards the end of the paragraph. |
| 7-196 | 7 | 5 | 51 | 5 | 51 | positive in what sense?  Higher temp or reinforcement? [James Hudson, USA] | positive in the sense "greater than 0". |
| 7-197 | 7 | 5 | 52 | 6 | 1 | A recent reference that might be relevant here is: Schwartz et al., (2010),  Why hasn't Earth warmed as much as expected? J. Climate 23, 2453-2464. [Ralph Kahn, United  States of America] | thanks but not every citation can be quoted. We now quote an earlier citation. |
| 7-198 | 7 | 5 | 53 | 5 | 53 | Change 'and primarily that' to 'primarily those'  [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | changed. |
| 7-199 | 7 | 5 | 53 | 5 | 53 | "that" should be "those" [Jón Egill Kristjánsson, Norway] | done. |
| 7-200 | 7 | 5 | 54 | | | The following reference is also relevant here: Schwartz, S.E., Charlson, R.J., Kahn, R.A., Ogren, J.A. and Rodhe, H. 2010, Why hasn't Earth warmed as much as expected? J. Climate 23, 2453-2464.<br>DOI 10.1175/2009JCLI3461.1<br>[Henning Rodhe, Sweden] | thanks but not every citation can be quoted. We now quote an earlier citation. |
| 7-201 | 7 | 5 | 54 | | | Some addl key citations;  this issue has been raised for some time:<br><br>Schwartz S. E. and Andreae M O. Uncertainty in climate change caused by anthropogenic aerosols.  Science 272, 1121-1122 (1996).<br><br>Gregory, J. M., R. J. Stouffer, S. C. B. Raper, P. A. Stott, and N. A. Rayner (2002), An Observationally based estimate of the climate sensitivity, J. Climate, 15, 3117-3121.<br><br>Schwartz S. E., Uncertainty requirements in radiative forcing of climate change. J. Air Waste Management Assoc. 54, 1351-1559 (2004).<br><br>Schwartz S. E., Charlson R. J., Kahn R. A., Ogren, J. A., and Rodhe H. Why Hasn't Earth Warmed as Much as Expected? J. Climate 23, 2453-2464 (2010); doi: 10.1175/2009JCLI3461.1. [Stephen E Schwartz, USA] | thanks but not every citation can be quoted. We now quote an earlier citation (Schwartz and Andreae 1996). |

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| 7-202 | 7 | 5 | 55 | 5 | 55 | "to" should be "with" [Jón Egill Kristjánsson, Norway] | done. |
| 7-203 | 7 | 5 | 55 | 5 | 56 | In light of this finding, I very much hope that AR5 will include a table and figure showing climate sensitivity, total anthropigenic forcing, and anthropogenic aerosol forcing for each of the climate models included in AR5. A Table added to section 9.7.4.3 would be an appropriate place to include this information. [JOHN OGREN, USA] | Chapter 7 includes a table with aerosol AF from available CMIP5 models (not all of them have done the calculations). |
| 7-204 | 7 | 5 | 56 | 6 | 2 | Misleading statement, I think. If climate sensitivity is determined by calibrating a model against observed warming during the 20th century the uncertainty in climate sensitivity due to aerosol forcing uncertainty will translate into a corresponding uncertainty in projeced future temperature despite a possible decline in aerosol forcing. [Henning Rodhe, Sweden] | statement changed to "This will translate into a corresponding uncertainty in future projected temperatures because anthropogenic aerosol emissions tend to stabilise or decrease in future scenarios (Dufresne et al., 2005; van Vuuren et al., 2011)." |
| 7-205 | 7 | 5 | 56 | 6 | 2 | The emission scenarios used for CMIP5 stand out in this respect, because they show unusually strong decreases in aerosol emissions (see also chapter 11, page 2, lines 36-39). Uncertainties in aerosol radiative forcings will therefore be important for future climate projections, in particular during the first half of the 21st century. [Twan Van Noije, Netherlands] | true, we can only touch on this issue here. It is one for chapter 11. |
| 7-206 | 7 | 6 | 1 | 6 | 2 | Surely, it is not the scenarios that "tend to stabilise or decrease in the future", but the emissions. Please rephrase! [Jón Egill Kristjánsson, Norway] | statement changed to "This will translate into a corresponding uncertainty in future projected temperatures because anthropogenic aerosol emissions tend to stabilise or decrease in future scenarios (Dufresne et al., 2005; van Vuuren et al., 2011)." |
| 7-207 | 7 | 6 | 1 | 6 | 2 | Lamarque et al. (2010) is about historical emissions, and is not a correct reference for future scenarios of anthropogenic aerosol emissions. [Twan Van Noije, Netherlands] | right, we refer to van Vuuren et al (2011° now. |
| 7-208 | 7 | 6 | 2 | | | Addl key citation<br><br>Schwartz, S. E., Charlson R. J. and Rodhe H. Quantifying climate change — Too rosy a picture? Nature Reports – Climate Change 1, 23-24 (2007). doi:10.1038/climate.2007.22 [Stephen E Schwartz, USA] | thanks but not every citation can be included. |
| 7-209 | 7 | 6 | 4 | 6 | 9 | Much of this paragraph is stated better on p. 7-10. [Daniel Murphy, United States of America] | Keep both. |
| 7-210 | 7 | 6 | 4 | | 9 | The multi-scale modeling framework is another type of models with complexity between conventional GCMs and the high-resolution global models (e.g., Grabowski 2001; Khairoutdinov and Randall 2001). The most recent progress is the inclusion of a sophisticated higher-order turbulence in the cloud-resolving model component of the MMF to better represent the boundary-layer turbulence and low-level clouds (Cheng and Xu 2011). A short addition to this paragraph could be “upgraded multi-scale modeling framework with more sophisticated subgrid CRM physics.” [Kuan-Man Xu, USA] | Rejected. Too much detail. This section has been shortened. |
| 7-211 | 7 | 6 | 11 | 6 | 21 | Here it should be noted that, compared to the efforts during 3rd and 4th ARs, the regional characterization of aerosols have considerably improved over Asia & Africa and the roles of complex interplays between natural and anthropogenic aerosols over the landmass and oceans are better characterized. More accurate measurements, and forcing estimates with bracketted uncertainties over specific regions are also available. These should be included in one paragraph here, so that this, as well as future assessments, could be built on those, and the current understanding is put in perspective. [K KRISHNA MOORTHY, INDIA] | this is too much detail for Section 7.1. However this is largely discussed in Section 7.3 |
| 7-212 | 7 | 6 | 28 | 6 | 41 | The concepts of RF and AF are applied to aerosols in chapter 7, but they are only introduced in chapter 8. The division between these chapters is not logical. [Twan Van Noije, Netherlands] | unfortunately there is way around that, which is why we have pointers to Chapter 8 in section 7.1. We also have a couple of figures to introduce the concepts right away. |
| 7-213 | 7 | 6 | 29 | | | A bridge sentence or phrase (“In this chapter we use the word ‘forcings’ to mean...”) would be useful. [Robert Pincus, USA] | first sentence has been modifed to "Figure 7.1 provides an overview of the concepts used in this |

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| | | | | | | | chapter when discussing clouds and aerosols in the context of climate change. " |
| 7-214 | 7 | 6 | 30 | | | "radiation budget ---> "radiant energy budget" [Manfred Wendisch, Germany] | "radiation" changed to "radiative" |
| 7-215 | 7 | 6 | 34 | 6 | 36 | It would be useful to rewrite this sentence to highlight out the central point, i.e. that feedbacks are changes mediated by changes in temperature. [Robert Pincus, USA] | sentence was rephrased to "Feedbacks are associated to changes in climate variables that are mediated by a change in temperature; they contribute to amplify or damp the changes to the global mean surface temperature via their impact on the radiative budget" |
| 7-216 | 7 | 6 | 34 | | | I think that adjustments are thought to occur on a timescale of weeks (or sub-seasonal as mentioned later) rather than 'less than a week'. It might also be helpful to mention stratospheric adjustment in this paragraph. [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | "within a week" changed to "within a few weeks" |
| 7-217 | 7 | 6 | 35 | 6 | 35 | The word "greater" should be removed. [Jón Egill Kristjánsson, Norway] | correction made |
| 7-218 | 7 | 6 | 38 | 6 | 38 | delete "new" or replace with "a new emphasis on"; the concept has been around a while [Daniel Murphy, United States of America] | text changed to "relatively new concept of adjusted forcing " |
| 7-219 | 7 | 6 | 38 | 6 | 39 | Noet that the definition of AF in Chapter 1 (see the comment below) is different if not incorrect comapring to the definition provided in 8.1.1. The brief description here also appears inaccurate (e.g., "land" surface rather than just "surface", etc.). [Chien Wang, United States of America] | this definition is accurate. Even in fixed SST experiments, can surface fluxes (ie LH and SH) adjust over the ocean. The ocean surface can also adjust in Gregory experiments. |
| 7-220 | 7 | 6 | 39 | 6 | 40 | Aerosols affect clouds in many more ways than the semi-direct and indirect effects. These include cloud absorption effects (CAE I and II) (Jacobson, M.Z., Investigating cloud absorption effects: Global absorption properties of black carbon, tar balls, and soil dust in clouds and aerosols, J. Geophys. Res., doi:10.1029/2011JD017218, in press, 2012, http://www.agu.org/pubs/crossref/pip/2011JD017218.shtml), the rainout effect (Section 3 of Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, J. Geophys. Res., 107, (D19), 4410, doi:10.1029/ 2001JD001376, 2002), and others discussed in that last paper, Section 3. Please clarify. [Mark Z. Jacobson, U.S.A.] | this is too much detail for this section. |
| 7-221 | 7 | 6 | 48 | 6 | 48 | What is the "comprehensive but climate-focused way"? [Chien Wang, United States of America] | paragraph has been changed. |
| 7-222 | 7 | 6 | 51 | 6 | 53 | I don't think the discussion of solar radiation management techniques should not be part of this chapter. Some of those techniques indeed rely on modification of aerosols and clouds, but changing the surface albedo has little connection to what this chapter is meant to be about. The discussion of geoengineering options would be better placed in the WG3 report. [Twan Van Noije, Netherlands] | the aim is to discuss all SRM techniques in one place. Physical aspects of SRM techniques are relevant to WGI. |
| 7-223 | 7 | 6 | 54 | 6 | 54 | "response to the" is unclear, especially because the word "response" is superfluous. Please replace by "to changes in". [Jón Egill Kristjánsson, Norway] | Text changed |
| 7-224 | 7 | 6 | 56 | | | Section 7.2: This section is very well-written - clear, accurate, and insightful. [Robert Pincus, USA] | Thanks. |
| 7-225 | 7 | 7 | 0 | | | Subsection 7.2.1.1 should also contain a description of major microphysical processes in cloud evolution (such as coagulation [collision + coalescence], homogeneous and heterogeneous ice nucleation, deposition/condensation/immersion/contact freezing, Bergeron Findeisen process, riming, aggregation, Hallet Mossop process). This may help to understand the immanent problems associated with cloud modeling and parameterizations of cloud evolution in global climate models. [Manfred Wendisch, Germany] | The chapter now includes more discussion of this, though it mostly occurs in section 7.4 |
| 7-226 | 7 | 7 | 3 | 8 | 11 | Sections 7.2.1.1 and 7.2.1.2 should include a short discussion of possible sources of error and their magnitude, in particular for the satellite derived quantities [Andrew Ferrone, Germany] | such information has been given where important for report conclusions. Otherwise readers must refer to the cited studies for more details. |
| 7-227 | 7 | 7 | 5 | 7 | 5 | Delete "vapor" after "with respect to water". [Chien Wang, United States of America] | Done, thanks |

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| 7-228 | 7 | 7 | 11 | | 21 | What is impt here is advance in understanding (avoid 1st person plural); not which chapters reviewed aerosol forcing in previous ipcc repts. [Stephen E Schwartz, USA] | 1st person avoided; references to past reports are essential are crucial and have been retained. |
| 7-229 | 7 | 7 | 12 | 7 | 13 | Change to "…can be objectively isolated through the use of satellite data". (Presumably the assemblages aren't being separated from the satellite data as the sentence suggests.) [Patricia Quinn, US] | Thanks, change made |
| 7-230 | 7 | 7 | 13 | | 13 | A different approach for isolating cloud regime or system was based directly upon satellite footprint data, which is called “cloud object” approach. Tropical deep convection, boundary-layer cumulus, stratocumulus and overcast cloud object types have been studied with TRMM, Terra and Aqua satellite data in the past few years. The reference to be added to this line is “Xu et al. 2005.” [Kuan-Man Xu, USA] | This material is now deleted |
| 7-231 | 7 | 7 | 13 | | | doi:10.1029/2005GL024584 belongs in this reference list. [Robert Pincus, USA] | Thanks but this material has been removed. |
| 7-232 | 7 | 7 | 16 | 7 | 16 | Replace "on at " with "at". [Steven Ghan, USA] | Done, thanks |
| 7-233 | 7 | 7 | 19 | 7 | 29 | About Figure 7.2: b) Even if the hand-made drawing is very nice, do not forget to make it in its final form. In this figure as well in Figure 7.2.c) (and in fact in all the other figures), please incresase the font size. [Rubén D Piacentini, Argentina] | Yes the figure has changed and is no longer hand-drawn. |
| 7-234 | 7 | 7 | 19 | | | I realize the purpose of Fig 7-2 but the figs need work and should be made a little more current - I would use the cloudsat view of real fronts - e.g. Fig 4a of Posselt et al., BAMS, 2008 which was nicely contrasted against the notional model of Bjerknes - similarly the zonal distribution of clouds should not be from CALIPSO as given as that is incomplete and misses too many clouds below high clouds- rather it should be the combined cloudsat/calipso view as reported by Mace et al.,. JGR, 2009. [Graeme Stephens, USA] | Yes the figures have been updated. |
| 7-235 | 7 | 7 | 28 | | 41 | The para makes impt point of the distinction betw forcing and adjusted forcing and makes the case for AF as pertinent; I expected the para to go on to make the case that the AF in some sense salvages the important concept relating forcing and _climate_ response. [Stephen E Schwartz, USA] | The discussion of these concepts has been revised and we hope it is clearer |
| 7-236 | 7 | 7 | 31 | 7 | 37 | The first cloud climatology derived from spaceborne lidar at global scale could be found in Berthier, S., Chazette, P., Pelon, J., and Baum, B. (2008), Comparison of cloud statistics from spaceborne lidar systems, Atmos. Chem. Phys., 8, 6965–6977. [Patrick CHAZETTE, France] | Thank you for providing this reference. |
| 7-237 | 7 | 7 | 32 | 7 | 32 | Please specify which threshold value has been considered [Andrew Ferrone, Germany] | Now given |
| 7-238 | 7 | 7 | 32 | | | Cloud fraction; screening to remove thin cloud of little radiative significance.<br><br>One would like to know more; what is the threshold; what is the radiative effect of clouds below threshold. If it's 1 W m-2, say in the long wave for cirrus, this is already 6% of the net cloud radiative effect of 17 W m-2. [Stephen E Schwartz, USA] | Info now given |
| 7-239 | 7 | 7 | 34 | | | CALIPSO not Calipso [Larry Thomason, United States of America] | Thanks, fixed |
| 7-240 | 7 | 7 | 34 | | | change "Calipso" to "CALIPSO" for consistency; same on Page 7 line 41. [Hailong Wang, USA] | Thanks, fixed |
| 7-241 | 7 | 7 | 35 | 7 | 36 | Please add the corresponding heights for 400 hPa and 700 hPa. [Sabine Wurzler, Germany] | It would be too burdensom to quote altitudes and pressures every time a height is quantified in the report. |
| 7-242 | 7 | 7 | 35 | 7 | 37 | About the sentence: "Clouds appearing above roughly the 400 hPa level (which are nearly all ice) are typically considered “high clouds”, while those appearing below roughly 700 hPa (which are mostly liquid but often contain ice outside the tropics) are considered “low” (Zelinka et al., 2011a).", there is no definition of middle clouds. Please, include one. [Rubén D Piacentini, Argentina] | Middle clouds noted explicitly |
| 7-243 | 7 | 7 | 35 | 7 | 37 | Units for height should be the same in the text and the figure the text refers to, i.e., hPa or km ASL but not both. [Patricia Quinn, US] | Thanks, this has been changed. |
| 7-244 | 7 | 7 | 40 | 7 | 43 | With respect to Figure 7.3, several changes must be introduced in order to clarify figures a) and b). | This figure has been revised |

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| | | | | | | Figure7.3.a): i) the symbol a) is included two times; ii) after the word CALIPSO there is a number:0.67 that it is not explained; iii) in the Pacific Ocean and to the East of Hawai islands, there is a grey segment that it is not explained; iv) in the corresponding figure caption "(a) Geographical mean, with thin cloud (SR < 5) removed", the symbol SR has not been explained before. Figure7.3.b): i) in the upper part of the figure, "ANN" is not defined; ii) in the corresponding figure caption: "(b) latitude-height section of zonal mean cloud cover", the "zonal mean" is not defined (for example, 1° latitude). Please take these comments into account when preparing the refinements for the Second Order Draft. [Rubén D Piacentini, Argentina] | |
| 7-245 | 7 | 7 | 45 | 7 | 47 | Move the figure in section 7.2.1.2 [Patrick CHAZETTE, France] | yes, figure moved |
| 7-246 | 7 | 7 | 45 | 7 | 47 | Figure 7.4 is not well positioned, because the reference to this figure follows in the next section 7.2.1.2. [Claudia Mäder, Germany] | yes, figure moved |
| 7-247 | 7 | 7 | 45 | 7 | 47 | move this figure (caption) to section 7.2.1.2 [Hailong Wang, USA] | yes, figure moved |
| 7-248 | 7 | 7 | 46 | 7 | 47 | About Figure 7.4: The vertical box that displays the colour scale has very small marks (tips) and it is difficult to associate each mark with the corresponding number. Please, modify the colour box in this sense. [Rubén D Piacentini, Argentina] | Figure has been updated |
| 7-249 | 7 | 7 | 46 | | 47 | The color scales of CRE plots are not consistent. The cancellation between LW and SW CREs in the tropics is not easily visualized with such different color scales. A unified color scale will solve this visual problem. What is the CERES-EBAF version used in these CRE plots? The most recent version is 2.6. [Kuan-Man Xu, USA] | Figure has been changed |
| 7-250 | 7 | 7 | 49 | | | This section is rather superficial and in fact not really very reflective of key advances made since AR4 - per my comment 5 above the imapct of clouds on atmosheric heating and the surface energy balance has significant implications for radiation-hydroliogical feedbacks tht are barely touched on - the effects of clouds on these other components of the energy balance have now been quantified much better with A-Train data - publications relvant are L'Ecuyer et al., 2009 (JGR); Kato et al, 2011 (J. Climate). [Graeme Stephens, USA] | Revisions have been made accordingly |
| 7-251 | 7 | 7 | 49 | | | replace "Radiation Budget" by "Radiative Energy Budget", similar replacements in the subsequent text. [Manfred Wendisch, Germany] | done |
| 7-252 | 7 | 7 | 51 | 8 | 11 | Everything has to be interpreted from your absurd model, which ignores the real climate of meteorology [VINCENT GRAY, NEW ZEALAND] | no change suggested by the reviewer |
| 7-253 | 7 | 7 | 52 | | | "longwave" ---> "terrestrial", similar replacements in the subsequent text. [Manfred Wendisch, Germany] | longwave is common terminology and we retain it. |
| 7-254 | 7 | 7 | 52 | | | "fluxes" ---> "flux densities", similar replacements in the subsequent text. [Manfred Wendisch, Germany] | fluxes is common terminology and is retained in lieu of the more cumbersome suggestion. It is understood that the flux is per unit area. |
| 7-255 | 7 | 7 | 53 | 7 | 58 | It may be useful to note (if this section is indeed considered relevant) that "the SWCRE is manifest as a cooling at the surface whereas the positive global LWCRF is a combination of heating of the moist tropical atmosphere and a surface heating over stratocumulus regimes and higher latitudes (e.g. Allan, 2011)." [Allan, R. P. (2011) Combining satellite data and models to estimate cloud radiative effect at the surface and in the atmosphere, Meteorological Applications, 18, p.324-333, doi:10.1002/met.285] [Richard Allan, UK] | Revisions have been made to address these concerns |
| 7-256 | 7 | 7 | 55 | 7 | 59 | Note these are not forcings but natural effects. [James Hudson, USA] | terminology revised |
| 7-257 | 7 | 7 | 55 | 8 | 3 | Comparing the contribution of clouds to the Earth's energy budget and 2xCO2 radiative forcing is confusing. It should be made even clearer that only anthropogenically-driven changes in cloud radiative effects can be meaningfully compared to a radiative forcing. I suggest writing the other way around: start with the statement of page 8 lines 2-3, then support the statement by the fact that SWCRE and LWCRE are large contributions. [Nicolas Bellouin, United Kingdom] | Text reworded to address this concern. |
| 7-258 | 7 | 7 | 58 | | | It would be useful to quote B.J. Sohn's studies here which demonstrate that the observations-based definition of the LWCRE includes a water vapour contribution (e.g., Sohn et al., Atmos. Chem. Phys, 10, 11641-11646, 2010) [Johannes Quaas, Germany] | This point is now made more clearly (though several papers made this point well before the noted reference) |

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| 7-259 | 7 | 7 | 59 | | | LW estimates vary quite a bit more than 10% I think: different versions of the same instrument (CERES) have recently varied by over 10%. [Andrew Gettelman, USA] | Text revised |
| 7-260 | 7 | 7 | | | | Section 7.2.1. and the figures 7.3/7.3: The portion does not provide the required importance to the tropical clouds, where multi-layer and multiple cloud system persist. This region is also amongst the most cloud-covered regions of the world. The deep convections lift the cloud even above the tropopause and lead to modifications of the humidity structure of the lower stratosphere. This aspect needs a better address [K KRISHNA MOORTHY, INDIA] | The new text better discusses deep clouds and their radiative effects |
| 7-261 | 7 | 8 | 3 | 8 | 4 | This figure is surely subject to enormous inaccuracies. Cooling of te earth in the daytime is mainly by the atmosphere, Clouds have an influence that is intermittent, and their effects depend on highly inaccurate estimates of quantity, plus more highly inaccurate calculations for the effect of diffeent clouds. Your figure is not only implausible it has very large inaccuracies yiou do not reveal [VINCENT GRAY, NEW ZEALAND] | The comment is incorrect and not supported by citations to the literature |
| 7-262 | 7 | 8 | 3 | | | Suggest add following language and citation:<br><br>Likewise rather small error in fractional cloudiness and/or cloud reflectance in climate models would be expected to result in rather large error in modeled shortwave and longwave fluxes at the top of the atmosphere, at the surface, and throughout the atmospheric column. Such errors have been shown to result in large systematic errors in albedo in climate models that vary substantially as a function of latitude and season, and from model to model, despite models getting fairly accurate global mean albedo (Bender et al 2006).<br><br>Bender F.A.M., H. Rodhe, R.J. Charlson, A.M.L. Ekman, and N. Loeb, 2006: 22 views of the global albedo - comparison between 20 GCMs and two satellites, Tellus A, 58 (3): 320-330. [Stephen E Schwartz, USA] | Thanks for the suggestion, this point is now made. |
| 7-263 | 7 | 8 | 7 | 8 | 8 | Is Figure 7.4 meant? [Claudia Mäder, Germany] | yes, thanks |
| 7-264 | 7 | 8 | 8 | 8 | 8 | Please specify which part of figure 7.2 (a,b or c?) this statement is referring to [Andrew Ferrone, Germany] | error corrected |
| 7-265 | 7 | 8 | 8 | | | change "7.2" to "7.4d"? [Hailong Wang, USA] | error corrected |
| 7-266 | 7 | 8 | 8 | | | "7.2" ---> "7.4" [Manfred Wendisch, Germany] | error corrected |
| 7-267 | 7 | 8 | 15 | | | change "undrafts" to "updraughts" for consistency; there are other places too (for "updraft" and "downdraft"). [Hailong Wang, USA] | error corrected |
| 7-268 | 7 | 8 | 20 | | | "statistical correlations over" ---> "statistical correlations between updrafts and precipitation over" [Manfred Wendisch, Germany] | error corrected |
| 7-269 | 7 | 8 | 26 | | | change "depth of tropopause" to "depth of troposphere" or "height of tropopause" [Hailong Wang, USA] | error corrected |
| 7-270 | 7 | 8 | 31 | 8 | 39 | I consider that the uncertainty in low cloud response merits further discussion. In particular, Zhang et al. (2009) suggest that present day LTS-low cloud amount relationships may not be good proxies for future changes in dynamical regime. [Zhang et al 2009 "Low-cloud Fraction, Lower-Tropospheric Stability, and Large-Scale Divergence" J. Clim, 22, 4827-4844] Additionally, the occurrence of open/closed cell convection and spatial structure of marine low cloud are not well understood (Jensen et al. 2008; Wood and Hartmann, 2006) [Jensen et al 2008 "Investigation of Regional and Seasonal Variations in Marine Boundary Layer Cloud Properties from MODIS Observations" J. Clim, 21 4955-4973; Wood and Hartmamn 2006 Spatial Variability of Liquid Water Path in Marine Low Cloud: The Importance of Mesoscale Cellular Convection J Cli vol19, 1748-1764] [Richard Allan, UK] | These issues are discussed further in the feedback subsection and aerosol-cloud interactions sections. |
| 7-271 | 7 | 8 | 31 | | 39 | This paragraph is rather narrowly focused on the relationship between LTS and boundary-layer cloudiness. The stratification control on low-level clouds is overstated because it may be only valid for a narrow range of LTS value (~18 K). Although there is overwhelmed evidence that LTS is highly correlated with low-level stratocumulus cloudiness on seasonal time scale, such a relationship is not universally valid (Sun et al. 2011), i.e., modulated by seasonal cycle and ENSO. The control on liquid water content of these low-level clouds was not discussed at all. The control of large-scale dynamic processes on low-level clouds was not | This material has been revised. |

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| | | | | | | mentioned, which was generally weak on monthly time scales (Eitzen et al. 2011). However, the short-term temporal (synoptic) variability of large-scale subsidence (or surface divergence) plays an important role in explaining the relationship between low-level cloudiness and LTS using a mixed-layer modeling approach (Zhang et al. 2009), whereas the monthly-mean subsidence does not. The latter agrees with Eitzen et al. (2011). [Kuan-Man Xu, USA] | |
| 7-272 | 7 | 8 | 31 | | | (section 7.2.1.3.2) While the statistical statement is of course true, the second part of this paragraph is misleading: In the present-day climate, LTS and EIS are fairly correlated to the low-level cloud fraction, but predicting cloud cover from such quantities alone is erroneous (PhD thesis Christine Nam, Reports on Earth System Science 88, Max Planck Institute for Meteorology, 132 pp, 2011; ISSN 1614-1199; available at http://www.mpimet.mpg.de/fileadmin/publikationen/Reports/WEB_BzE_88.pdf; Section 4.7; a publication is in preparation). [Johannes Quaas, Germany] | This mateiral has been revised |
| 7-273 | 7 | 8 | 36 | 8 | 36 | I think the interaction between the clouds and oceans should be also mentioned here. For example, Huang and Hu (2007) found that the influence of the low cloud fluctuation is an important component in the evolution of the southeastern tropical Atlantic SST anomalous events in observations. [Zeng-Zhen HU, USA] | There is not room in this chapter to go into this type of detailed interaction, but such matters are taken up in Chapter 14. |
| 7-274 | 7 | 8 | 36 | 8 | 36 | Huang, B. and Z.-Z. Hu, 2007: Cloud-SST feedback in southeastern tropical Atlantic anomalous events. J. Geophys. Res. (Ocean), 112, C03015, doi: 10.1029/2006JC003626. [Zeng-Zhen HU, USA] | There is not room in this chapter to go into this type of detailed interaction, but such matters are taken up in Chapter 14. |
| 7-275 | 7 | 8 | 36 | 8 | 37 | Correct the Reference 'Wood;Bretherton 2006" to "Wood and Bretherton, 2006" [Panuganti China Sattilingam Devara, India] | fixed |
| 7-276 | 7 | 8 | 41 | 8 | 52 | To shorten I suggest deleting this section. Arctic clouds are interesting but not critical to this section. [Daniel Murphy, United States of America] | Other reviewers have made the opposite suggestion. We have retained this section. |
| 7-277 | 7 | 8 | 42 | 8 | 52 | There is redundancy between this paragraph and that on page 7-44 section 7.4.4.2 but the longevity of a well-documented SHEBA case (Zuidema et al. 2005) is also worth mentioning either here or on page 7.44.In addition the Morrison et al. (2011) reference is probably to his paper in JAMES rather than the one in QJRMS this citation refers to. [Paquita Zuidema, USA] | revisions have addressed the redundancy issue. Thanks for pointing out the references. |
| 7-278 | 7 | 8 | 44 | | | This could use a reference: Kay, J. E. and A Gettelman, Cloud influence on and response to seasonal Arctic sea ice loss, J. Geophys. Res., doi:10.1029/2009JD011773, 2009 [Andrew Gettelman, USA] | Thanks |
| 7-279 | 7 | 8 | 52 | | | Additional to Klein the following reference would be appropriate here: Ehrlich, A., M. Wendisch, E. Bierwirth, J.-F. Gayet, G. Mioche, A. Lampert, and B. Mayer, 2009: Evidence of ice crystals at cloud top of Arctic boundary-layer mixed-phase clouds derived from airborne remote sensing. Atmos. Chem. Phys., 9, 9401–9416, www.atmos-chem-phys.net/9/9401/2009/ [Manfred Wendisch, Germany] | suggestion appreciated |
| 7-280 | 7 | 8 | 54 | 7 | 54 | Change to "The response of high-latitude boundary-layer cloud cover to the fractional cover…." [Patricia Quinn, US] | thanks |
| 7-281 | 7 | 8 | 54 | 8 | 54 | Delete second occurrence of "response". [Anthony Del Genio, USA] | Done. |
| 7-282 | 7 | 8 | 54 | 8 | 54 | Typo, "response" appears twice. [JOHN OGREN, USA] | Thanks |
| 7-283 | 7 | 8 | 54 | 8 | 56 | This sentence is not clear: "The response of high-latitude boundary-layer cloud cover response to the fractional cover of underlying sea ice could be an important climate feedback and is discussed (including relevant observations) in Section 56 7.2.4.3.5.". Maybe the second time that the word "response" appears should be omitted. [BEGONA ARTINANO, SPAIN] | text changed |
| 7-284 | 7 | 8 | 54 | 8 | 56 | And vice-versa: The response of the sea ice to the longwave effects of clouds could be an important feedback on the sea ice as well. The sentence makes it sound like a one-way interaction even though the word "feedback" is used. [Anthony Del Genio, USA] | The text has been deleted. |
| 7-285 | 7 | 8 | | | | Next paragraph: A very commendable effort [K KRISHNA MOORTHY, INDIA] | Thanks |

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| 7-286 | 7 | 8 | | | | At the end of Section 7.2.1.2 it would be required to mention, based on observations, what are, if any, the changes in net cloud radiative forcing over the last 50 years or so, since when systematic observation of global clouds system have started. Is there any evidence of trends in cloud cover and net CRE, which could be above uncertainties? Even regional trends are worth mentioning and considering, especially over tropics. [K KRISHNA MOORTHY, INDIA] | These are topics for Chapter 2 |
| 7-287 | 7 | 9 | 1 | | | This paragraph should note that these circulations in fact are quite fundamental to the way clouds process aerosol and to the aerosol indirect effect (comment 22 below) [Graeme Stephens, USA] | Moot: paragraph deleted to save space |
| 7-288 | 7 | 9 | 3 | 9 | 3 | Are really downdrafts meant? In fact, updrafts should produce more clouds. [Claudia Mäder, Germany] | Moot: paragraph deleted to save space |
| 7-289 | 7 | 9 | 11 | 9 | 11 | Correct the Reference "Houze 1993" to "Houze, 1993" [Panuganti China Sattilingam Devara, India] | Moot: paragraph deleted to save space |
| 7-290 | 7 | 9 | 13 | | | Summarize here: The small scale interactions thus feedback on cloud organization and persistence in stratiform and convective regimes and thus may be significant for cloud radiative effects. [Andrew Gettelman, USA] | Moot: paragraph deleted to save space |
| 7-291 | 7 | 9 | 17 | 9 | 49 | I miss a word here on the problems related to precipitation modelling. Especially in determining the effects of wind drift on precipitation and the location of where the precipitation reachs the ground. At least in some regional climate models this poses a problem especially when simulating precipitation near mountain ridges, which trigger often precipitation. [Sabine Wurzler, Germany] | now mentioned as an advantage of hi-res small-domain models in 7.2.2.1.1 |
| 7-292 | 7 | 9 | 17 | 9 | 49 | How about the problems with the treatment of density perturbations in climate models? Talking about effects of convection on relative humidity and supersaturation. Are those meanwhile solved? [Sabine Wurzler, Germany] | Not sure what the reviewer is concerned about in the first question. Modern cumulus parameterizations do account for virtual and loading effects on updraft buoyancy if that is what is meant. Yes, cumulus parameterizations do affect relative humidity and those with explicit updraft velocity do feed back into aerosol activation |
| 7-293 | 7 | 9 | 19 | 9 | 21 | Rephrase: "Cloud formation processes span scales from the submicrometer scale of cloud condensation nuclei to single cloud scales of up to several kilometres (e.g., thunderstorms) to cloud system scales of up to thousands of kilometres (e.g., hurricanes). This range of scales (10^-6 m to 10^+6 m).....". [Sabine Wurzler, Germany] | Point taken, but current text captures the essence with less text |
| 7-294 | 7 | 9 | 19 | | 49 | It is not true that there are only two modeling approaches for clouds; LES for boundary-layer clouds and CRM for deep convection. CRMs with advanced higher-order turbulence closure can reasonably well simulate both cloud types and their transition (Cheng and Xu 2006, 2008). This approach has also been tested within an upgraded multiscale modeling framework model (Cheng and Xu 2011). The challenge for this approach is the computationally cost, which can be twice as expensive as a standard CRM with a low-order turbulence closure. [Kuan-Man Xu, USA] | Paragraph paraphrasing this information added to end of 7.2.2.1.1 |
| 7-295 | 7 | 9 | 21 | | | Might add: This situation requires that processes that take place on scales smaller than those that are explicitly resolved in climate models be parameterized, which in turn requires assumptions about the representation of the subgrid processes and the controlling variables. [Stephen E Schwartz, USA] | This point is taken up in 7.2.3 |
| 7-296 | 7 | 9 | 23 | 9 | 34 | We suggest to add the following sentence at the end of this paragraph: "Regional online-coupled climate models including an explicit treatment of atmospheric chemistry and aerosols, that currently can be applied down to a resolution of 2-3 km, can help to close this gap. Although cloud process need to be parameterized also for the regional scale, their representation is more detailed than in global models and they allow to study the interaction between clouds and mesoscale dynamic patterns." [Andrew Ferrone, Germany] | Have added the following sentence: 'It [LES/CRM] is useful not only in simulating cloud and precipitation characteristics, but also in understanding how turbulent circulations within clouds transport and process aerosols and chemical constituents.' This turns the reviewer comment into a point germane to the models being discussed in this section.(CB) |
| 7-297 | 7 | 9 | 24 | 9 | 24 | What's so "unusual" about this strategy? [JOHN OGREN, USA] | Reworded to avoid use of 'usual' |
| 7-298 | 7 | 9 | 28 | 9 | 31 | It would be useful to support the claim of CRM skill with references. [Robert Pincus, USA] | Added |

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| 7-299 | 7 | 9 | 29 | 9 | 29 | It is written "...characterize statistical characteristics.." . It could be better ".. simulate statistical characteristics.." or " characterize statistical parameters" or "characterize statistical properties" [BEGONA ARTINANO, SPAIN] | Corrected to 'properties' |
| 7-300 | 7 | 9 | 29 | | | change "skilfully" to "skillfully"? [Hailong Wang, USA] | Corrected |
| 7-301 | 7 | 9 | 37 | | | It would be also good to inform the reader that these high-resolved resolutions often remain idealised in their boundary conditions (surface, lateral forcing, biogeochemical processes including CCN description). [Johannes Quaas, Germany] | Added sentences to this effect. |
| 7-302 | 7 | 9 | 47 | | | it may be useful to add a phrase explaining that cloud thickness is climatically relevant. [Robert Pincus, USA] | Changed 'thickness' to 'liquid water path', whose climatic relevance was already discussed in 7.2.1 |
| 7-303 | 7 | 9 | 49 | | | Can this be subsection be summarized and related back to a climate impact? [Andrew Gettelman, USA] | Have added a paragraph motivating the relevance of high-resolution models to climate modeling, which is more multifaceted than using them to assess a particular climate impact. |
| 7-304 | 7 | 9 | 56 | | | Reverse first two sentences of this paragraph [Andrew Gettelman, USA] | Thanks, revised. |
| 7-305 | 7 | 10 | 4 | 10 | 4 | Satellites singular [James Hudson, USA] | corrected |
| 7-306 | 7 | 10 | 4 | | | "Satellites observations" --> "Satellite observations" [Richard Allan, UK] | corrected |
| 7-307 | 7 | 10 | 9 | 10 | 9 | The passive instruments referred to in this sentence retrieve liquid water path, not liquid water content. [Anthony Del Genio, USA] | Corrected. |
| 7-308 | 7 | 10 | 9 | | | why only liquid water content? [Johannes Quaas, Germany] | This section has been completely rewritten. |
| 7-309 | 7 | 10 | 14 | 7 | 14 | "After “day time” add: But multiple cloud layers can be detected and retrieved by using multi-spectral measurements (Huang et al., 2005, 2006). References:<br>1. Huang J., P. Minnis, B. Lin, Y. Yi, M. M. Khaiyer, R. F. Arduini, and G. G. Mace, 2005: Advanced retrievals of multilayered cloud properties using multispectral measurements, J. Geophys. Res., 110, D15S18, doi:10.1029/2004JD005101.<br>2. Huang J., P. Minnis, B. Lin, Y. Yi, S. Sun-Mack, T. Fan, and J. R. Ayers, 2006: Determination of ice water path in ice-over-water cloud systems using combined MODIS and AMSR-E measurements, Geophys. Res. Lett., 33, L21801, doi:10.1029/2006GL027038. " [Jianping Huang, China] | This section has been completely rewritten. |
| 7-310 | 7 | 10 | 14 | 10 | | After "during daytime", add ", while use of multiple channels as those from the MODIS can detect thin cirrus over a lower water clouds (Chang and Li, 2005a,b). Add references: Chang, F.-L., and Z. Li, 2005a, A new method for detection of cirrus-overlapping-water clouds and determination of their optical properties, J. Atmos. Sci., 62, 3993–4009.<br>Chang, F.-L., and Z. Li, 2005b, A near-global climatology of single-layer and overlapped clouds and their optical properties retrieved from Terra/MODIS data using a new algorithm, J. Climate, 18, 4752-4771. [Zhanqing Li, USA] | This section has been completely rewritten. |
| 7-311 | 7 | 10 | 18 | 10 | 34 | May be mention the Global Precipitation Measurement' (GPM) / MEGATROPIC launched in 2011. [Patrick CHAZETTE, France] | We do not discuss upcoming platforms. |
| 7-312 | 7 | 10 | 18 | | 18 | Wielicki et al. (1995) should be cited. [Kuan-Man Xu, USA] | This section has been completely rewritten. |
| 7-313 | 7 | 10 | 21 | 10 | 21 | Correct the Reference "Stephens, Kummerow 2007" to "Stephens and Kummerow, 2007" [Panuganti China Sattilingam Devara, India] | The reference has been deleted. |
| 7-314 | 7 | 10 | 25 | 10 | 34 | add that CALIOP only acquires observations along CALIPSO ground tracks. [Patrick CHAZETTE, France] | This section has been completely rewritten. |
| 7-315 | 7 | 10 | 26 | | | This statement is neglecting the LITE and GLACE missions. [Johannes Quaas, Germany] | This section has been completely rewritten. |

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| 7-316 | 7 | 10 | 27 | | | “accurate” is certainly questionable. [Johannes Quaas, Germany] | Agreed, change made |
| 7-317 | 7 | 10 | 34 | | | A reference to doi:10.1175/2011BAMS2856.1 on the “new simulators” would be appropriate. [Robert Pincus, USA] | simulators discussed in Chapter 9 |
| 7-318 | 7 | 10 | 37 | 10 | 40 | Change to "Over the last 15 years, monitoring programs in different climate regimes…." (The sites themselves are not doing the measuring.) [Patricia Quinn, US] | This section has been completely rewritten. |
| 7-319 | 7 | 10 | 41 | 10 | 41 | Suggest to add an example study for surafce observations, i.e., “For example, long-term surfaece measurements at DOE ARM permanent site Southern Great Plain (SGP) greatly enhanced our understanding of cloud properties in different aerosol evivronment (Li et al., 2011) ”. Reference: Li Z., F. Niu, J. Fan et al (2011), Long-term Net Impacts of Aerosols on Cloud and Precipitation, Nature Geoscience, doi:10.1038/ngeo1313. [Jiwen Fan, United States of America] | This section does not discuss aerosol-cloud interactions |
| 7-320 | 7 | 10 | 44 | 10 | | Change "two recent examples" to "three recent examples" [Zhanqing Li, USA] | change made |
| 7-321 | 7 | 10 | 46 | 10 | | Change "(May et al., 2008) and", to "(May et al., 2008), " [Zhanqing Li, USA] | This section has been completely rewritten. |
| 7-322 | 7 | 10 | 47 | 10 | | Add the following at the end of the paragraph. ", and the EAST-AIRE (Li et al. (2007a) and EAST-AIRC (Li et al. 2011a) for studying the impact of heavy aerosol loading on regional climate due to drastic alterations of radiation, cloud, temperature and precipitation by aerosols over the East Asia. Add references: Li, Z., et al. (2011a), East Asian Studies of Tropospheric Aerosols and their Impact on Regional Climate (EAST-AIRC): An overview, J. Geophys. Res., 116, D00K34, doi:10.1029/2010JD015257. Li, Z., et al., (2007a), Preface to special section: Overview of the East Asian Study of Tropospheric Aerosols: an International Regional Experiment (EAST-AIRE), J. Geophys. Res. D22S00, doi:10.1029/2007JD008853. [Zhanqing Li, USA] | This section has been completely rewritten. |
| 7-323 | 7 | 10 | 49 | | | Section 7.2.3: While acknowledging my own biases, it does seem odd that this section does not discuss changes in the way cloud overlap is treated in radiation schemes. Many models participating in CMIP5 will have changed the representation from something analytic to a method based on drawing random samples (doi:10.1029/2002jd003322). The change is widespread and can change the radiation budget by amounts commensurate with the forcing by doubled CO2 (doi:10.1175/MWR3257.1). [Robert Pincus, USA] | cloud overlap is discussed in the revised section. |
| 7-324 | 7 | 10 | 53 | 10 | 53 | This sentence is incorrect. Air does not supersaturate with respect to water vapor. Rather, water vapor in rising air supersaturates with respect to liquid or ice ( or more precisely, with respect to the equilibrium value over liquid or ice). [Anthony Del Genio, USA] | sentence changed |
| 7-325 | 7 | 10 | 53 | 10 | 53 | Same as above. [Chien Wang, United States of America] | sentence changed |
| 7-326 | 7 | 10 | 53 | 19 | 44 | Again according to your absurd models. You do not even admit that the clouds warm the atmosphere when they are formed, withenergy that has been removed from the surface by evaporation. [VINCENT GRAY, NEW ZEALAND] | unsure what change the reviewer is suggesting |
| 7-327 | 7 | 10 | 53 | | | I would suggest "with respect to liquid water or ice." [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | sentence changed |
| 7-328 | 7 | 10 | 53 | | | Sentence almost identical to 7/7/7 [Manfred Wendisch, Germany] | sentence changed to remove redundancy |
| 7-329 | 7 | 10 | 55 | 10 | 55 | Should "across model grid cells" be read as "within each model grid cell"? [Chien Wang, United States of America] | changed |
| 7-330 | 7 | 11 | 1 | | | "Unfortunately"; perhaps from a modeler's perspective. but not necessarily from the perspecitive of earth climate; suggest strike. [Stephen E Schwartz, USA] | Sentence removed so point is moot |
| 7-331 | 7 | 11 | 2 | 11 | 2 | delete "unfortunately" [Daniel Murphy, United States of America] | Sentence removed so point is moot |
| 7-332 | 7 | 11 | 3 | 8 | 34 | "grid cell", "gridbox", and "model cell" are used for the same thing. [Hailong Wang, USA] | All replace by 'grid cell' |
| 7-333 | 7 | 11 | 8 | | | This paragraph is accurate but doesn’t provide quite enough information. Readers will need to be reminded | Sentence added in this spirit: 'This variability must be |

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| | | | | | | that many processes depend nonlinearly on cloud properties, so that computing process rates using grid-mean properties leads to biases (http://dx.doi.org/10.1175/1520-0469(2001)058<1117:SBITMA>2.0.CO;2, doi:10.1029/2000JD900504). [Robert Pincus, USA] | represented to accurately simulate cloud-radiation interaction, condensation, evaporation and precipitation, and other cloud processes that crucially depend on how cloud condensate is distributed across each grid box (Cahalan et al. 1994; Pincus and Klein 2000; Larson et al. 2001). ' |
| 7-334 | 7 | 11 | 10 | 11 | 19 | This paragraph is accurate but does not follow from the preceding one. Inconsistent assumptions across parameterizations are indeed undesirable but they aren't "because of" the unresolved variability, as the first sentence implies. [Robert Pincus, USA] | Wording fixed - 'Because' removed |
| 7-335 | 7 | 11 | 18 | | 19 | In summary, realistic simulation of clouds and their response to climate change forms one of 19 the greatest challenges of climate modelling.' I found the discussion that ends with this sentence (the proceeding several pages to be very illuminating (as a non-cloud guy); clear, well written, much appreciated. [Larry Thomason, United States of America] | thanks |
| 7-336 | 7 | 11 | 21 | 11 | 51 | This section should include a discussion about the cloud treatments in models whose results are used in several parts of this assessment and that include more detailed treatments than the CMIP5 models. A comparison of the characteristics of the most detailed and data-evaluated online chemistry-climate models is given in Zhang, Y., Online coupled meteorological and chemistry models: history, current status, and outlook, Atmos. Chem. Phys., 8, 2895-2932, 2008. One such model is GATOR-GCMOM. Among global models, it is the only one to treat (a) multiple subgrid cumulus clouds in every grid cell with convection of gases and size/composition-resolved aerosols in each subgrid cloud, (b) evolution of discrete (as opposed to modal) size- and composition-resolved hydrometeor particle number and mass (two moments) from size- and composition-resolved aerosol particles, (c) explicit size- and composition-resolved coagulation interactions among aerosols, between aerosols and hydrometeors, and among hydrometeors, and (d) tracking of aerosol particles and their components through precipitation (The algorithms are described in Jacobson, M. Z., Development of mixed-phase clouds from multiple aerosol size distributions and the effect of the clouds on aerosol removal, J. Geophys. Res., 108 (D8), 4245, doi:10.1029/2002JD002691, first used in M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, J. Geophys. Res., 107, (D19), 4410, doi:10.1029/ 2001JD001376, 2002 and recently used in Jacobson, M.Z., Short-term effects of controlling fossil-fuel soot, biofuel soot and gases, and methane on climate, Arctic ice, and air pollution health, J. Geophys. Res., 115, D14209, doi:10.1029/2009JD013795, 2010) [Mark Z. Jacobson, U.S.A.] | The following paragraph was added to the end of 7.2.3.1: 'Cloud process parameterization is important for specialized chemical-aerosol-climate models (see review by Zhang et. al. 2008) and for regional climate models as well as for CMIP5-class global models. A few of these models have added complexity in representing subgrid cloud variability and the cloud particle size distribution (e. g. GATOR-GCMOM, Jacobson 2003).' |
| 7-337 | 7 | 11 | 23 | | | Papers describing CMIP5 models are still emerging but it may not be fair to say that "most" microphysics schemes treat subgrid variability. Even those that do (e.g. 10.1175/2008JCLI2105.1) typically treat inhomogeneity only with respect to autoconversion and not to accretion, etc. [Robert Pincus, USA] | The intended meaning was that CMIP5 class models have fractional cloud cover schemes, not that they treat heterogeneity within parts of the grid box filled by cloud (which you correctly point out is dealt with in some microphysical schemes and not others). Reworded to make these points more clearly |
| 7-338 | 7 | 11 | 29 | 11 | 29 | distributions singular [James Hudson, USA] | done |
| 7-339 | 7 | 11 | 29 | 11 | 29 | Change to "..an assumed drop size distribution…" [Patricia Quinn, US] | done |
| 7-340 | 7 | 11 | 33 | | | It is surprising that in this paragraph reviewing the AR4 models, parameterisations developed thereafter (Storelvmo et al. 2009; Ghan et al. 2011b; Hoose et al. 2009) are referred to. [Johannes Quaas, Germany] | Point taken… reworded to avoid mixing of papers pre and post AR4. However, the Hoose et al. 2009 paper is kept because it is an assessment of AR4 models, not a new parameterization approach. |
| 7-341 | 7 | 11 | 34 | | | It may be useful to specify that for both, cloud water and precipitation water, liquid and ice phases are usually distinguished in distinct classes. [Johannes Quaas, Germany] | Have added sentence: 'Many AR4 models used a single class for both cloud water and cloud ice, and a single class for both rain and snow, with a temperature-dependent partitioning between them. Many climate models now include separate, |

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| | | | | | | | physically-based prognostic equations for cloud water vs. cloud ice, and rain vs. snow, allowing a more realistic treatment of mixed-phase processes.' |
| 7-342 | 7 | 11 | 36 | 11 | 36 | I have not attempted to copy edit the text in general, but I note here that by "prognosed," you probably meant "predicted," or "simulated"... or maybe "prognosticated"? [Ralph Kahn, United States of America] | Changed 'prognosed' to 'predicted' (the former is common specialist usage, but may befuddle others) |
| 7-343 | 7 | 11 | 40 | | | Did any CMIP3 models use activation based on vertical velocity? Or should this be present tense? [Andrew Gettelman, USA] | No, and the cited reference should have been to Storelvmo et al 2009. Reworded. |
| 7-344 | 7 | 11 | 44 | 11 | 44 | Change "cloud. with" to "cloud with" [Panuganti China Sattilingam Devara, India] | Removed extra '.' |
| 7-345 | 7 | 11 | 44 | 11 | 45 | The second dot in the sentence " More models participating in CMIP5 will use two moment schemes for liquid stratiform cloud. with the following advances" should be discarded. [BEGONA ARTINANO, SPAIN] | Removed extra '.' |
| 7-346 | 7 | 11 | 49 | 11 | 51 | Since this is an assessment, it would be appropriate to assess the sufficiency of the observational data so support these sophisticated schemes (both for determining parameter values and verifying validity of parameterization). [JOHN OGREN, USA] | Added sentence about the use of ARM-like ground sites and modern global satellite datasets being good tests of whether new schemes improve cloud microphysics |
| 7-347 | 7 | 11 | 49 | | | "cloud drop activation" is inconsistent with "cloud droplet activation" (on page 12, line 37) [Hailong Wang, USA] | changed to 'cloud droplet activation' |
| 7-348 | 7 | 11 | 55 | 12 | 5 | "New representations of the B-W-F process in mixed phase clouds…" The first and still only global climate model to treat the explict representation of liquid, ice, and mixed-phase cloud microphysics and composition, thus the B-W-F process, is in the GATOR-GCMOM model, as described in 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 using algorithms described and analyzed in Jacobson, M. Z., Development of mixed-phase clouds from multiple aerosol size distributions and the effect of the clouds on aerosol removal, J. Geophys. Res., 108 (D8), 4245, doi:10.1029/2002JD002691, 2003. The most recent application of the algorithms was in Jacobson, M.Z., Short-term effects of controlling fossil-fuel soot, biofuel soot and gases, and methane on climate, Arctic ice, and air pollution health, J. Geophys. Res., 115, D14209, doi:10.1029/2009JD013795, 2010 [Mark Z. Jacobson, U.S.A.] | This paragraph is about what has changed since AR4. However, to note that other sophisticated treatments were proposed before then, the following was added to the end of 7.2.3.1: '... A few of these models have added complexity in representing subgrid cloud variability and the cloud particle size distribution (e. g. GATOR-GCMOM, Jacobson 2003).' |
| 7-349 | 7 | 11 | 56 | | | (section 7.2.3.3.1) To my knowledge, no CMIP5 model is applying the Lohmann and Hoose (2009) or Lohmann (2008) parameterisations. It would be useful to clarify this. [Johannes Quaas, Germany] | Done |
| 7-350 | 7 | 11 | 57 | | | Could add Gettelman et al 2010 which also has a description of the Bergeron process in a GCM parameterization: Gettelman, A., X. Liu, H. Morrison, S. J. Ghan, S. Klein, J. Boyle, S. Park, A. J. Conley, D. L. Mitchell, Global Simulations of Ice Nucleation and Ice Supersaturation with an improved Cloud Scheme in the Community Atmosphere Model, J. Geophys Res., 115, D18216, 10.1029/2009JD013797, 2010 [Andrew Gettelman, USA] | done |
| 7-351 | 7 | 12 | 1 | 12 | 1 | Correct the Reference "Korolev 2007" to "Korolev, 2007" [Panuganti China Sattilingam Devara, India] | done |
| 7-352 | 7 | 12 | 7 | 12 | 14 | "…only one AR4 GCM allowed ice superstaturation." This statement is incorrect. 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, relied on as part of AR4, solved explicit size- and composition-resolved depositional growth onto ice crystals accounting for the actual RHi in the layer of the cloud (Paragraph 24). The specific equations for growth are given in Jacobson, M. Z., Development of mixed-phase clouds from multiple aerosol size distributions and the effect of the clouds on aerosol removal, J. Geophys. Res., 108 (D8), 4245, doi:10.1029/2002JD002691, 2003. [Mark Z. Jacobson, U.S.A.] | include |
| 7-353 | 7 | 12 | 7 | | | Perhaps more relevant to chapter 9, but there has been quite some advance in the treatment of ice in models with marked improivements in the mean in CMIP5 models - this is described in a paper by Li et al., 2012; JGR and I can send this to ch 7 Las as needed. [Graeme Stephens, USA] | Email exchange with Graeme concluded this paper more relevant to Ch. 9 |

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| 7-354 | 7 | 12 | 23 | 12 | 33 | The GISS GCM has had what is called here an "adaptive" treatment of lateral entrainment (similar to that in MIROC) for a longer time than any of the models cited (Del Genio et al., 2007, GRL, 34, L16703, doi:10.1029/2007GL030525). Like the models cited, it has been shown to improve the model's simulation of the MJO (Del Genio et al., 2012, J. Climate, in press, doi:10.1175/JCLI-D-11-00384.1; Kim et al., 2012, J. Climate, in press, doi:10.1175/JCLI-D-00447.1). It should be mentioned as well. [Anthony Del Genio, USA] | Done. |
| 7-355 | 7 | 12 | 23 | 12 | 33 | It would be useful to start this paragraph with a topic sentence to let readers know the overall message: that models are quite sensitive to relatively small changes to convection parameterizations. [Robert Pincus, USA] | added |
| 7-356 | 7 | 12 | 23 | 12 | 33 | In this paragraph, other efforts in advancing cumulus parameterization should also be mentioned. They lead to important improvements of model simulations in the mean state and variability. These include the work of considering convective organization and modifying convective closure (Mapes and Neal 2011; Song and Zhang 2009). [Guang Zhang, United States of America] | Refs added |
| 7-357 | 7 | 12 | 36 | | 36 | Chikira and Sugiyama (2010) should be cited. [Kuan-Man Xu, USA] | added |
| 7-358 | 7 | 12 | 42 | | | Is the IFS model contributing to CMIP5, or does the EC-Earth contribution to CMIP5 apply these new developments? [Johannes Quaas, Germany] | EC-Earth uses EDMF but not the dual-MF enhancement of Neggers et al. However, we assess that this development will be significant for upcoming climate model improvements, and the wording was chosen not to imply that we were referring only to CMIP5 models. |
| 7-359 | 7 | 12 | 53 | 12 | 54 | Could the advances in computer capabilities since AR4 with regard to resolution be quantified in a graph or a table? [Manfred Wendisch, Germany] | we have declined the suggestion due to space limitations. This would really belong in Ch. 9 anyway. |
| 7-360 | 7 | 12 | 53 | 13 | 5 | "There have been three types of development..." Another type of model is one that treats multiple subgrid clouds in a grid column, each with a different base and top height: 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 as described in more detail in Jacobson, M. Z., Development of mixed-phase clouds from multiple aerosol size distributions and the effect of the clouds on aerosol removal, J. Geophys. Res., 108 (D8), 4245, doi:10.1029/2002JD002691, 2003. [Mark Z. Jacobson, U.S.A.] | This is a type of subgrid parameterization, alluded to in the revised 7.2.3.1 |
| 7-361 | 7 | 12 | 57 | | | Even the high-resolution models referred to here have grid scale of order several km - that's a *very* large "individual cumulus cloud". Would "thunderstorm" or something similar be more evocative? [Robert Pincus, USA] | Agreed, wording changed. |
| 7-362 | 7 | 13 | 3 | 13 | 4 | Convection-permitting scales have been applied to longer climate simulations for limited area regional climate models. For example Kendon et al (2012) carried out a 20-year length simulation with a 1.5km RCM over a region of the UK; and Wakazuki et al (2008) carried out 10-year time-slice experiments with a 5km RCM over Japan. [References: Wakazuki, Y., M. Nakamura, S. Kanada, and C. Muroi (2008) Climatological reproducibility evaluation and future climate projection of extreme precipitation events in the Baiu Season using a high-resolution non-hydrostatic RCM in comparison with an AGCM. J. Meteorol. Soc. Jpn, 86 (6), 951-967; Kendon E. J., N. M. Roberts, C. A. Senior, and M. J. Roberts (2012) Realism of rainfall in a very high resolution regional climate model. Submitted to J. Climate] [Elizabeth Kendon, United Kingdom of Great Britain & Northern Ireland] | These experiments are really too short to be sufficiently relevant, though the text acknowledges that regional climate simulations (of which there are many) do permit explicit clouds. |
| 7-363 | 7 | 13 | 7 | 13 | 29 | I do not see why a section like this belongs in an IPCC document. The CC in IPCC stands for climate change. GCRMs have not at this point in history been used to simulate climate change. If this document were a review of the overal progress of numerical modeling, such a section would be appropriate, along with a section on NWP models. But it is not. Maybe by the time of AR6 a GCRM will be used for an IPCC 21st Century scenario. Until that happens, though, it is not approapriate to have an entire section devoted to them in an IPCC report. All this does is to mislead the public into thinking that there are now super high resolution IPCC models that are not subject to the parameterization issues that coarser models have. This would not even be true for current GCRMs if they had done IPCC runs, given issues with at least microphysics and boundary layer clouds, but it certainly is not true for the current models which have not been used for IPCC runs. | The section has been shortened considerably. We regard global cloud-resolving models as global "process" models. As such, they are very relevant to testing the global models that are used in CMIP5. |

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| | | | | | | [Anthony Del Genio, USA] | |
| 7-364 | 7 | 13 | 7 | 13 | 29 | It is good to mention NICAM as in this subsection 7.2.3.5.1. NICAM is used not only for short-term weather simulations, but also for climate sensitivity studies such as cloud changes under global warming condition.<br><br>Overview papers of NICAM are Tomita and Satoh (2004) and Satoh et al. (2008), so it is appropriate to refer to them for the reference in the first line (L.7), instead of (Tomita et al. 2005; Miura et al. 2005).<br>Tomita et al. (2005) and Miura et al. (2007) are good examples of the multiscale structure of convective systems simulated by NICAM, so it is better to refer to them in the next paragraph, after the first sentence: "Tomita et al. (2005) and Miura et al. (2007) showed that multiscale structure of deep convective systems associated with a Madden-Julian Oscillation event or super cloud clusters."<br><br>Reference:<br>Tomita, H., Satoh, M. (2004) A new dynamical framework of nonhydrostatic global model using the icosahedral grid. Fluid Dyn. Res., 34, 357-400.<br>Tomita, H., Miura, H, Iga, S. Nasuno, T., and Satoh, M. (2005) A global cloud-resolving simulation: preliminary results from an aqua planet experiment. Geophys. Res. Lett., vol.32, L08805, doi:10.1029/2005GL022459.<br>Satoh, M., T. Matsuno,T., H. Tomita, H. Miura, T. Nasuno, S. Iga (2008) Nonhydrostatic Icosahedral Atmospheric Model (NICAM) for global cloud resolving simulations. J. Comp. Phys., 227, 3486-3514.<br>Miura, H., Satoh, M., Nasuno, T., Noda, A.T., Oouchi, K. (2007) A Madden-Julian Oscillation event realistically simulated by a global cloud-resolving model. Science, 318, 1763-1765. [Masaki Satoh, Japan] | Thanks, NICAM is mentioned in the revised text. |
| 7-365 | 7 | 13 | 8 | 13 | 13 | Usually the description of NICAM framework is referred to as Tomita et al. (2005) and Satoh et al. (2008, J. Comp. Phys., 227, 3486-3514). Miura et al. (2007, Science, 318, 1763-1765) is an innovative paper for the first MJO simulation without relying on cumulus parameterization. [Teruyuki Nakajima, Japan] | We cannot cite all NICAM references but have attempted to hit the most important ones. |
| 7-366 | 7 | 13 | 15 | 13 | 25 | Better to summarize this and say what NICAM tells us about parameterizations. Not clear from current text. [Andrew Gettelman, USA] | text has been changed |
| 7-367 | 7 | 13 | 15 | 13 | 25 | Additionally, Sato et al. (Satoh, M., Oouchi, K., Nasuno, T., Taniguchi, H., Yamada, Y., Tomita, H., Kodama, C., Kinter III, J., Achuthavarier, D. Manganello, J, Cash, B., Jung, T., Palmer, T. and Wedi, N., 2011: Intra-Seasonal Oscillation and its control of tropical cyclones simulated by high-resolution global atmospheric models. Climate Dyn., doi10.1007/s00382-011-1235-6) used NICAM in a series of boreal summer seasonal simulations to show that global cloud-system resolving models have the potential to simulate and improve our understanding of the statistics of tropical cyclones and the relationship between the phase of the Madden-Julian Oscillation and tropical cyclone formation. [James Kinter, United States of America] | We cannot cite all NICAM references but have attempted to hit the most important ones. |
| 7-368 | 7 | 13 | 25 | 13 | 25 | Cloud change study using NICAM can be refered to here, and also in Sections 7.2.4.1 and 7.2.4.4.<br>"Miura et al. (2005), Collins and Satoh (2009), and Satoh et al. (2011) studied cloud change using NICAM and showed that cloud response is very different from that obtained GCMs with cumulus parameterization, especially for high clouds. Satoh et al. (2011) showed using NICAM simulations that high-cloud fraction generally increases under a warmer climate condition, and that the longwave feedback might be enhanced. They also showed that the FAT mechanism is working for some choice of physics parameters (Section 7.2.4.1). They further discussed that cloud optical thickness would be reduced mainly because the circulation becomes weaker as the climate becomes warmer (Section 7.2.4.4). "<br><br>Reference:Collins, W. D., Satoh, M. (2009) Simulating Global Clouds, Past, Present, and Future. Chap 20 in Heintzenberg, J., and R. J. Charlson, eds. 2009. Clouds in the Perturbed Climate System: Their Relationship to Energy Balance, Atmospheric Dynamics, and Precipitation. Struengmann Forum Report, vol. 2. Cambridge, MA: The MIT Press, pp.469-486.<br>Satoh, M., Iga, S., Tomita, H., Tsushima, Y., Noda, A.T. (2011) Response of upper clouds due to global warming tested by a global atmospheric model with explicit cloud processes. J. Climate, accepted. doi: http://dx.doi.org/10.1175/JCLI-D-11-00152.1.<br>Miura, H., Tomita,H., Nasuno,T., Iga, S., Satoh,M., Matsuno,T., 2005: A climate sensitivity test using a global cloud resolving model under an aqua planet condition. Geophys. Res. Lett., 32, L19717, doi:10.1029/2005GL023672. [Masaki Satoh, Japan] | Thanks, NICAM is mentioned in the revised text. |

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| 7-369 | 7 | 13 | 25 | | 25 | The simulation duration with global CRM is too short to produce climate. Mean states of the simulation may be a better terminology here. [Kuan-Man Xu, USA] | The simulations are sufficient to produce an atmospheric equilibrium, at least approximately. |
| 7-370 | 7 | 13 | 27 | 13 | 29 | We suggest adding the following sentence at the end of this paragraph: "This information can be complemented by regional models, that can be used at similar resolutions than GCRMs but on longer time-scales in order to study the relevant process in a limited area of the globe (Vogel et al., 2009)." REFERENCE: Vogel, B., Vogel, H., Bäumer, D., Bangert, M., Lundgren, K., Rinke, R., and Stanelle, T.: The comprehensive model system COSMO-ART – Radiative impact of aerosol on the state of the atmosphere on the regional scale, Atmos. Chem. Phys., 9, 8661-8680, doi:10.5194/acp-9-8661-2009, 2009 [Andrew Ferrone, Germany] | Regional models are now mentioned |
| 7-371 | 7 | 13 | 28 | 13 | 29 | This comment is moot since I am recommending that the section be deleted, but if the authors do not take that advice, then regarding this specific sentence, I would like to see a citation of a paper that helps understand something about a lower resolution GCM by using a GCRM (and what it is that we now underastand about that lower resolution GCM as a result). I agree that GCRMs do a better job in simulating certain aspects of e.g. deep convection; I am not aware of their contribution to understanding something that we did not already understand from CRMs, observations, etc.. [Anthony Del Genio, USA] | The section has been shortened and this part of the chapter reorganised. |
| 7-372 | 7 | 13 | 31 | 13 | 52 | It is worth to mention Wang et al. (2011, GMD) here for the Super-parameterization model as the PNNL-MMF is the first MMF that treats aerosol-cloud interactions. (Wang, M., Ghan, S., Easter, R., Ovchinnikov, M., Liu, X., Kassianov, E., Qian, Y., Gustafson, W. I., Larson, V. E., Schanen, D. P., Khairoutdinov, M., and Morrison, H.: The multi-scale aerosol-climate model PNNL-MMF: model description and evaluation, Geosci Model Dev, 4, 137-168, 10.5194/Gmd-4-137-2011, 2011.) [Minghuai Wang, United States of America] | The Wang et al paper is highlighted in Section 7.4. |
| 7-373 | 7 | 13 | 37 | | | Include dot at the end of the sentence. [Manfred Wendisch, Germany] | yes, thanks |
| 7-374 | 7 | 13 | 39 | 13 | 47 | Summarize: SPCAM solves some problems related to convection, but many GCM biases remain. [Andrew Gettelman, USA] | thanks for the suggestion |
| 7-375 | 7 | 13 | 39 | | | It's more fair to say that the SP-CAM gives a "more realistic" simulation of the diurnal cycle etc. [Robert Pincus, USA] | agreed |
| 7-376 | 7 | 13 | 43 | | 43 | The following sentence should be added: The under-prediction of marine stratocumulus clouds is remedied by an upgrade of the turbulence closure in the embedded CRM with an advanced third-order turbulence closure (Cheng and Xu 2011). [Kuan-Man Xu, USA] | The suggested reference will be considered in the revised draft. |
| 7-377 | 7 | 13 | 49 | 13 | 50 | To a limited extent. Super-parameterization GCMs are not to my knowledge being used for the full suite of large ensembles of 21st Century (and longer) runs, coupled chemistry, carbon cycle, etc., runs that conventional GCMs are doing. Rather they are being applied to one or a few selected long simulations. This is a welcome development, but again it is important that the public know that moast of what IPCC will be reporting is based on conventional GCMs. [Anthony Del Genio, USA] | The text has been modified. |
| 7-378 | 7 | 13 | 51 | | 51 | Add the reference of Cheng and Xu (2011) after "turbulence." [Kuan-Man Xu, USA] | The suggested reference will be considered. |
| 7-379 | 7 | 13 | 54 | 19 | 44 | The discussion on water vapour/lapse rate feedback is severely underdone in this section, and in the chapter as a whole, considering the central importance of water vapour and lapse rate changes in determining climate sensitivity, and consequently the central importance of assessing our confidence in those feedbacks for our confidence in overall projections of climate change. There are a sentence or two only, no assessment of confidence in the range of feedbacks or of model skill in representing them. Lapse rate feedback itself is barely mentioned. These feedbacks should have a separate section, where the basis for confidence is discussed and assessed, including:<br>(1). The magnitude of the impact on climate sensitivity of water vapour/lapse rate feedback (Bony et al 2006, Randall et al, 2007, Soden and Held, 2006), including AR5 update when available.<br>(2). The offsetting relationship between water vapour/lapse rate feedbacks, our physical understanding of it and its implication for climate sensitivity (Soden and Held, 2006, ...).<br>(3) The basis of these feedbacks in quasi-unchanging relative humidity, our improved understanding of the robustness of this (Ingram, 2010, 2011, Sherwood et al, 2010), and the observational support on seasonal, inter annual and climate change timescales (Chung et al, 2010, Dessler and Wong, 2009; Minschwaner et al, | Two pages were accidentally left out of the FOD that covered this. After the CLA subsequently circulated missing pages were subsequently circulated to this reviewer by the CLA, and the reviewer sent in the following: "Page 1, line 44: should read 'tropopause' not 'troposphere' Page 1, Line 51: suggest change anthropomorphic and unnecessarily loaded expression 'participates aggressively' to 'plays a fundamental role in' or similar. Page 2, lines 19-21. This sentence is wrong as it stands (maybe words have been left out?). Ingram (and others) show that it is (close to) unchanging RH that gives the overall strength of these combined feedbacks. The changes in RH near the tropopause were found by Ingram to |

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| | | | | | | 2006, Soden).<br>(4). Our knowledge of importance of upper tropospheric humidity in particular, and advances in means of detecting this and testing models.<br>(5). Updates on model representation of upper tropospheric humidity and its variations. Also evidence that the feedback is not sensitive to control climate biases in models (John and Soden, 2007), and why it is not (robustness of RH response in models).<br>(6) Evidence that models represent lapse rate changes on a range of timescales with skill (including trends – with appropriate caveats).<br>(7). An overall conclusion of (now higher than in AR4, and at a very high level) confidence in combined water vapour/lapse rate feedbacks, and the implications for projection confidence.<br>[Robert Colman, Australia] | be the source of differences between models. Page 2, lines 29-30. Similar comment to previous: this sentence can be read as if saying the total strength of this feedback depends mainly on RH changes. But RH change is only a secondary affect - the quasi unchanging RH is the critical thing. Also clarify the argument on error in lapse rate. Overall comment on sections 7.2.4.1/2: Given the critical nature of these feedbacks, there needs to be summarizing text to (1) pull all this together, and (2) provide an overall conclusion on confidence in combined water vapour/lapse rate feedbacks (now higher than in AR4, and at a very high level), and the implications for confidence in overall climate sensitivity of models. " We have addressed all the reviewer's subsequent comments through rewording or minor corrections. |
| 7-380 | 7 | 13 | 54 | | | Comment on Section 7.2.4: Despite the section heading, there is no treatment of the water vapor feedback in this section, it is only about clouds. Since water vapor is probably the most dominant positive feedback, it is worth spending some attention on post-AR4 progress in its understanding, [Chris Colose, United States] | Yes, two pages were accidentally left out of the FOD containing the requested text. This has now been added back in. |
| 7-381 | 7 | 13 | 54 | | | Sec 7.2.4 Cite new paper, Davies and Molloy GRL 39, L03701, that suggests negative cloud feedback “If sustained, such a decrease would indicate a significant measure of negative cloud feedback to global warming” [Paul Matthews, United Kingdom of Great Britain & Northern Ireland] | This paper is discussed. |
| 7-382 | 7 | 13 | | | | Some kind of overall summary of the state of cloud representation in models is missing. What of the major gaps can realistically be overcome in the near future? [Manfred Wendisch, Germany] | The text has been revised and should be clearer about this. |
| 7-383 | 7 | 14 | 6 | 14 | 10 | I strongly recommend against using the feedback factor. First, W m-2 is much easier to compare to forcings. Second, I disagree that the blackbody feedback is a good starting point. There are other feedbacks (e.g. lapse rate) that are almost as fundamental. [Daniel Murphy, United States of America] | We now use a more conventional dimensional feedback. |
| 7-384 | 7 | 14 | 6 | 14 | 10 | For example, the observed longwave short-term feedback is 0.44 K W-1 m2 with very high confidence from either ERBE or CERES. [Daniel Murphy, United States of America] | We now use a more conventional dimensional feedback. |
| 7-385 | 7 | 14 | 6 | 14 | 13 | In my point of view, the feedback factor requires more explanation in particular the multiplication with 0.31 K W^-1 m^2. [Manfred Wendisch, Germany] | We now use a more conventional dimensional feedback. |
| 7-386 | 7 | 14 | 6 | | 9 | Notation: Feedback factor. The quantity 1/(1-f) has long been denoted the feedback factor in climate science<br><br>Hansen J, Lacis A, Rind D, Russell G, Stone P, Fung I (1984) Climate Sensitivity: Analysis of Feedback Mechanisms, in Climate Processes and Climate Sensitivity. In: Hansen JE, Takahashi T (eds) AGU Geophysical Monograph 29. American Geophysical Union, p 130-163.<br><br>The quantity so defined is is a multiplier between the sensitivity calculated without feedback and that with feedback, thus a _factor_, as the word is defined and commonly used in algebra. The terminology is discussed by<br><br>Schwartz S. E. Feedback and sensitivity in an electrical circuit: An analog for climate models. Climatic Change 106, 315-326 (2011). doi: 10.1007/s10584-010-9903-9.<br><br>who denotes the quantity f here the total normalized feedback strength. The normalized feedback strengths associated with several processes are additive to give the overall normalized feedback strength.<br><br>Friedlingstein et al J Clim 06 (multi author paper; includes fung, joos, roeckner) refers to the quantity f (in the above; they call it g in the context of carbon cycle feedbacks) as the "gain". It is really a normalized gain, | We now use a more conventional dimensional feedback. |

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| | | | | | | certainly not the "open circuit gain" of electronic feedback circuits. [Stephen E Schwartz, USA] | |
| 7-387 | 7 | 14 | 8 | 14 | 8 | Explain the basis for the 0.31 value (Planck feedback?) [Steven Ghan, USA] | We now use a more conventional dimensional feedback. |
| 7-388 | 7 | 14 | 8 | 14 | 9 | Please assess whether the individual feedback factors are independent and additive. [JOHN OGREN, USA] | We now use a more conventional dimensional feedback. |
| 7-389 | 7 | 14 | 8 | | | "multiplying by 0.31 K W–1 m2" is rather oblique. I suggest "normalising by the no-feedback or black-body feedback response of 3.2 Wm-2K-1" [this was also detailed in the IPCC 1990 report and by Cess (1989) Nature and Bony et al. (2006) J Climate). [Richard Allan, UK] | We now use a more conventional dimensional feedback. |
| 7-390 | 7 | 14 | 8 | | | Why the factor? State in a sentence. [Andrew Gettelman, USA] | We now use a more conventional dimensional feedback. |
| 7-391 | 7 | 14 | 8 | | | Readers will be perplexed by the factor used to non-dimensionalize feedbacks without a phrase of explanation. [Robert Pincus, USA] | We now use a more conventional dimensional feedback. |
| 7-392 | 7 | 14 | 15 | 16 | 28 | AMIP3 and AR4 are cited mostly in these sections, what about the post-AR4 and CMIP5 results. In addition, in many places the discussions are about model performance rather than the feedbacks. [Chien Wang, United States of America] | The currently available peer-reviewed literature is based mainly on CMIP3 or other models. The final version of the report will contain more studies including CMIP5 models. |
| 7-393 | 7 | 14 | 15 | | | This feedback, and the tenor of this discussion is way too Northwest US centric and in my view is just not robust and is incomplete. We have known for a long time that changes to one component of the radiation balance are offset by changes to the other so changes to TOA LW flux under this feedback are offset by changes to the SW flux and in fact observations show that the higher cirrus clouds over warmer SSTs are also thicker and brighter. This result is well illustrated in a recent followup study of Dessler who has extended his 2010 observational study breaking down 'feedback estimates' by different cloud types - and during the ENSO high cloud changes have the largest impact in the tropics in the LW and also SW but these almost entirely offset one another and the net feedback is negative whereas all CMIP5 models have it as positive - again the point is there is more to be done and understood and tropical high cloud feedbacks are far from robustly positvie! Will have Dessler send a copy of the submitted paper of this new study to LAs [Graeme Stephens, USA] | The reviewer is confusing geographic variations of clouds in the current climate, which do not involve changes in tropopause height (or in fact involve a slightly lower tropopause over warmer surfaces), with changes in a globally warmer climate, where the tropopause rises. The commonly-observed correlation between SW and LW anomalies due to cloud amount changes in the deep tropics is simulated correctly by the same models that are among those simulating the feedback noted here, which does not involve a change in cloud amount. The Dessler 2010 and 2011 studies together show that correlations between cloud property and SST changes during ENSO-related variability are uninformative about cloud feedbacks, as discussed later in the chapter. These studies do reveal that most GCMs do not reproduce some aspects of the lagged SST-OLR regressions, but show that this is due to these models' poor simulation of ENSO. |
| 7-394 | 7 | 14 | 17 | 14 | 18 | This is somewhat misleading, because the near-cancellation between longwave and shortwave effects only occurs in the tropics in the presence of thick anvils. At other latitudes and if the high clouds are thin, the longwave effect tends to dominate. [Jón Egill Kristjánsson, Norway] | Agreed, text revised |
| 7-395 | 7 | 14 | 17 | 14 | 19 | The compensation strongly depends on microphysical and optical properties, this should be stressed. [Manfred Wendisch, Germany] | Agreed, text revised |
| 7-396 | 7 | 14 | 21 | 14 | 32 | This will be easier to follow if the description of the FAT mechanism starts the paragraph. [Robert Pincus, USA] | Thanks, change made |
| 7-397 | 7 | 14 | 21 | 14 | 36 | See the above comments. [Masaki Satoh, Japan] | No response |
| 7-398 | 7 | 14 | 21 | 14 | 36 | In the discussion of high-level clouds in differing warming climate scenarios, I think there should be explicit mention of tropical tropopause interactions. Radiative heating due to the presence of cirrus clouds is a significant component of the TTL thermal budget [Yang, Q., Fu, Q., and Hu, Y.: Radiative impacts of clouds in | These are good points and are now made in an earlier section concerning recent observational advances. |

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| | | | | | | the tropical tropopause layer, J. Geophys. Res., 115, D00H12, doi:10.1029/2009JD012393, 2010]. Direct observations of TTL cirrus have indicated direct heating rates of 2.5 - 3.2 K/day [Bucholtz, A., et al.: Directly measured heating rates of a tropical subvisible cirrus cloud, J. Geophys. Res., 115, D00J09, doi:10.1029/2009JD013128, 2010]. [Jeffrey Taylor, United States of America] | |
| 7-399 | 7 | 14 | 27 | 14 | 27 | But Zelinka and Hartmann now feel that the PHAT version of their hypothesis, which takes into account static stability changes with warming, is a better description of the physics than the original FAT, as their recent papers indicate. [Anthony Del Genio, USA] | Yes this subtlety is now noted. |
| 7-400 | 7 | 14 | 29 | 14 | 30 | If the cirrus outflow temperature is relatively constant, this would suggest a fixed injection of water vapour into the subtropical upper troposphere despite warming temperatures which is consistent with simulated declines in upper tropospheric humidity (Allan 2012 Surv. Geophys.DOI 10.1007/s10712-011-9157-8) . However, the mid-latitudes also supply moiture to the sub-tropical upper troposphere (Roca et al. 2012 Surv. Geophys in press; Galewsky et al. 2005 J Atmos Sci 62:3353–3367). [Richard Allan, UK] | These points are made In a section on the water vapour feedback, which has now been restored after accidental removal from the submitted FOD. |
| 7-401 | 7 | 14 | 31 | | | I believe, the explanation is rather that the surface warms while the cloud-top temperature remains constant, so the difference in clear-sky and cloudy-sky brightness temperatures, and thus the cloud greenhouse effect, increases. [Johannes Quaas, Germany] | Yes this is perhaps a better way of saying it, thanks. |
| 7-402 | 7 | 14 | 40 | | 40 | Xu et al. (2007) also provided a verification of the FAT hypothesis, in addition to Eitzen et al. (2009). [Kuan-Man Xu, USA] | reference added |
| 7-403 | 7 | 14 | 43 | | | What is a 'middle' or 'high' clouds? Does shallow cumulus count here or below? Perhaps a discussion of regimes or altitude. How do you make a distinction? [Andrew Gettelman, USA] | These clouds were defined in the introduction to the section. |
| 7-404 | 7 | 14 | 46 | 14 | 46 | Correct the References "Trenberth and Fasullo 2009; Zelinka and Hartmann 2010" to "Trenberth and Fasullo, 2009; Zelinka and Hartmann, 2010" [Panuganti China Sattilingam Devara, India] | thanks, correction made |
| 7-405 | 7 | 14 | 50 | | | It is unclear why the cloud response to a change in convective mass flux should be particularly difficult to simulate by GCMs. [Johannes Quaas, Germany] | model limitations are discussed in an earlier section. |
| 7-406 | 7 | 14 | 52 | 14 | 53 | I don't understand this sentence. GCMs have been talking about the important role of the cloud height feedback since the 1980s. Hansen et al. (1984) first estimated it for the GISS GCM, Cess et al. (1990) discussed it in the first intercomparison of climate models, and so on. [Anthony Del Genio, USA] | Text has been changed here and where PHAT is introduced, to acknowledge the history of recognition of this. |
| 7-407 | 7 | 15 | 1 | 15 | 7 | This material seems to belong with/be quite similar to the last paragraph of the section (lines 16-26). [Robert Pincus, USA] | Thanks, the order of the paragraphs has been switched. |
| 7-408 | 7 | 15 | 9 | 15 | 14 | I just don’t agree with any of this paragraph. There are very distinct high cloud feedbacks that occur both in cloud process models and even GCMs with hints in observations through their effects on heating the atmosphere and feedbacks on convection. My own study (Stephens et al., 2008; outlines how these feedbacks operate in a CRM run in radiative convective equilibrium. Other references are given in that paper that also point to these feedbacks. Anecdotally, ECMWF in tuning their high clouds to CLoudSat (to make more ice that produces more upper tropospheric heating) get strong feedbacks on the coenvective preciptation. The high cloud radiative feedbacks on precipitation may well be highly significant. [Graeme Stephens, USA] | The phenomena noted in the cited papers are not feedbacks on the planetary radiative budget and climate, which is the subject of this section, but rather are measures of the impact of cloud radiative effects on dynamics, and then on patterns of rainfall and cloud formation. These impacts are well known and occur also in GCMs (Slingo and Slingo 1990, Randall et al. 1991, Sherwood et al. 1994). They can be thought of as "feedbacks," but only in the sense of positive local feedbacks that help organise convection or negative feedbacks on lapse rate changes. Such phenomena are discussed briefly in an earlier section of Chapter 7. |
| 7-409 | 7 | 15 | 11 | 15 | 12 | It is recognized that cirrus clouds exert a warming effect that could be significant but it is claimed that "no compelling evidence from observations" shows that there have been significant changes. Large, persistent, cirrus clouds on the order of 3000 km have been observed over the tropical pacific which would seem to refute this claim and have significant climate impacts [Taylor, J., Randel, W., Jensen, E.J., 2011: Cirrus cloud-temperature interactions in the tropical tropopause layer: A case study. Atmospheric Chemistry and Physics, 11, 10085-10095, 10.5194/acp-11-10085-2011]. [Jeffrey Taylor, United States of America] | The cited study only notes that large cirrus clouds have been observed. This does not address whether they produce feedbacks by responding to global warming. |

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| 7-410 | 7 | 15 | 11 | 15 | 14 | I'm wondering whether it might be fair to reword something like this: "Observational evidence identifying this feedback is lacking, and the mechanism is not significant in current process models and GCMs (see Chapter 9); the CMIP3 multimodel mean change of thin high cloud fraction is actually smaller than for other cloud types (Zelinka et al., 2011a)." [Ralph Kahn, United States of America] | Thanks, we have made the suggested change. |
| 7-411 | 7 | 15 | 16 | 15 | 18 | good explanation [Daniel Murphy, United States of America] | Thanks |
| 7-412 | 7 | 15 | 20 | | 20 | Zhou et al. (2011) also explored the trend of Hadley cell boundaries with GPCP and ISCCP data. [Kuan-Man Xu, USA] | reference will be considered |
| 7-413 | 7 | 15 | 22 | | | New Paper recently submitted also shows variance in cloud feedback attributed to these regions: Gettelman et al 2011a: Gettelman, A., J. T. Fasullo and J. E. Kay, Simulated Climate Sensitivity, Feedbacks and Mean State, Submitted to J. Geophys. Res. Atmospheres [Andrew Gettelman, USA] | The paper has now been referenced. |
| 7-414 | 7 | 15 | 24 | 15 | 26 | Trenberth and Fasullo do discuss the storm track shift component of the feedback. But their main point was that the CMIP3 models systematically underestimated the cloudiness in the storm tracks (which in reality are almost overcast). Since the models predict an increase in cloud with warming in the storm tracks, and since the storm tracks are nearly overcast in reality and thus can't have such a feedback, they concluded that the CMIP3 models have a negative component of cloud feedback that must be an artifact. What is written in this section gives no indication of that important result. [Anthony Del Genio, USA] | Thanks, text adjusted to reflect this |
| 7-415 | 7 | 15 | 28 | 16 | 28 | This section focuses solely on the change in cloudiness. The impact of liquid water content on low cloud feedback is totally ignored. This is probably an artifact (buried) of using the ISCCP simulator output in examining cloud feedback strengths. These two feedbacks are closely related, but the liquid water content feedback may be stronger (Eitzen et al. 2011). In a latter section (7.2.4.4), discussion on cloud optical depth feedback was rather brief. There seems to be no progress on this topic. [Kuan-Man Xu, USA] | The section discusses both cover and water content changes, but in some places only "content" was noted. This has been changed. |
| 7-416 | 7 | 15 | 30 | 15 | 37 | Karlsson et al. 2008 suggest that CMIP3 models tend to overestimate feedback by low level subtropical clouds. Karlsson, J., Svensson, G. and Rodhe, H. 2007. Cloud radiative forcing of subtropical low level clouds in global models. Climate Dynamics. http://www.springerlink.com/content/b4r17t0614104114/?p=3321f972ed6443a99724fe9ec0d9dd2e&pi=0 [Henning Rodhe, Sweden] | The biases noted in this paper do not tell us anything about whether the feedback is too strong or too weak. |
| 7-417 | 7 | 15 | 33 | | | Dufresne and Bony 2008 is a global study which does not I think say anything about low clouds. They cite earlier papers which do (e.g. Bony and Dufresne 2005, Webb et al 2006, Wyant et al 2006). [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | reference changed |
| 7-418 | 7 | 15 | 39 | 15 | 40 | I agree completely with this sentence. How, then, can you say in the Executive Summary (p. 3, lines 18-19) that the reduction in subtropical cloudiness in the models is robust? There needs to be a clear differentiation here between the consensus of the current generation of models (if this indeed even holds up when all the CMIP5 results are in) and a physical understanding that subtropical low cloud feedback is positive. That understanding does not exist at this time. More on this below. [Anthony Del Genio, USA] | Well spotted. This bullet and the main text have been carefully reworded to maintain a better distinction between responses supported by multiple lines of evidence, vs. those that occur consistently in GCMs but lack other support. |
| 7-419 | 7 | 15 | 41 | 15 | 44 | At least one of the LES models participating in the GASS CGILS intercomparison gets a negative cloud feedback for the stratocumulus regime because deepening of the boundary layer in the warmer climate leads to a physically thicker cloud and thus a higher liquid water path. This potential negative feedback mechanism is not mentioned at all. I'm not saying that I believe it, but this paragraph gives the impression that there is no longer any physically plausible way to imagine a negative low cloud feedback. [Anthony Del Genio, USA] | Again a good point, this possibility is now discussed. |
| 7-420 | 7 | 15 | 41 | | | Stephens (Feb 2010) GEWEX News (and refs within) note that models simulate low cloud with optical depths that are overestimated which raises the interesting possibility that the sensitivity of reflectivity changes to cloud water content (cloud water negative feedback) may be undestimated due to the non-linear relationship between cloud albedo and optical deapth. [Richard Allan, UK] | Interestingly other papers (Karlsson et al. 2008) make the opposite feedback speculation based on the same observation. Our position is that there is no clear link between biases of cloud amount and feedback, except in the special case pointed out by Fasullo and Trenberth where the observed phenomenon has 100% cloud cover and model changes go in a consistent direction. |

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| 7-421 | 7 | 15 | 42 | | | There's no need to introduce acronyms (WVMR) that aren't used later. [Robert Pincus, USA] | Text using this acronym (four times) was accidentally omitted from the submitted FOD. |
| 7-422 | 7 | 15 | 48 | | | change "skilful" to "skillful"? [Hailong Wang, USA] | OK |
| 7-423 | 7 | 15 | 51 | 15 | 51 | "negative feedback" is an error, and should be replaced by "positive feedback"! The anti-correlation means that at higher SSTs the clouds are thinner and less extensive, hence reducing their cooling effect on climate, i.e., a positive feedback. [Jón Egill Kristjánsson, Norway] | Thanks, this is corrected. |
| 7-424 | 7 | 15 | 51 | | 51 | negative should be changed to positive here because of the anti-correlation between low cloud cover/liquid water content and SST anomalies. [Kuan-Man Xu, USA] | Thanks, this is corrected. |
| 7-425 | 7 | 15 | 54 | | | The Section number 7.2.4.3.4 is not correct. [Minghuai Wang, United States of America] | corrected |
| 7-426 | 7 | 16 | 19 | 16 | 28 | So let me understand the logic here: A subset of models other than conventional GCMs (MMFs, LESs, LAMs) have produced negative low cloud feedbacks, but the authors choose to ignore these in favor of the low cloud feedbacks produced by conventional coarse-resolution GCMs because the coarse resolution models are in some unspecified way better tested? I don't want to say that I believe any of these models, but when I see an LES model in particular get a negative low cloud feedback, my reaction is that we'd better understand why it does that before we conclude that a GCM that doesn't even purport to resolve stratocumulus is more reliable. [Anthony Del Genio, USA] | Low-cloud feedbacks obtained from process models are not consistently more negative than GCMs, though they perhaps show a broader spread. The weaknesses of process-model studies are now better explained in the text, though obviously GCMs also have we |
| 7-427 | 7 | 16 | 22 | 16 | 22 | My understanding was that the superparameterization studies also often involved +SST experiments rather than coupled-ocean experiments (eg, Wyant 2006) and that this might lead to lower sensitivity estimates (Danabasoglu and Gent, 2008). [Marcus Sarofim, USA] | this is now noted. |
| 7-428 | 7 | 16 | 25 | 16 | 25 | Replace "high-resolutions" with "high-resolution". [Steven Ghan, USA] | Actually we have changed the terminology to "cloud resolving" (and explained what this does and doesn't mean). |
| 7-429 | 7 | 16 | 25 | 16 | 25 | resolutions singular [James Hudson, USA] | Actually we have changed the terminology to "cloud resolving" (and explained what this does and doesn't mean). |
| 7-430 | 7 | 16 | 32 | 16 | 40 | Accurate quantification of partitioning between ice and water (for constant cloud water content) in mixed-phase clouds is highly essential for better representation of clouds in the climate models. [Panuganti China Sattilingam Devara, India] | This issue is discussed to the extent possible based on the available peer-reviewed literature. |
| 7-431 | 7 | 16 | 32 | 16 | 40 | Old discussion of feedback loop of warming - cloud optical thickness - falling speed might be revisited for note. [Teruyuki Nakajima, Japan] | cloud optical depth feedback and cloud phase feedback are discussed. Is this a different feedback? |
| 7-432 | 7 | 16 | 32 | 16 | 40 | See the above comments. [Masaki Satoh, Japan] | no response |
| 7-433 | 7 | 16 | 35 | 16 | 36 | Comment on the line "As climate warms … did not change."<br><br>The shift from ice to liquid cloud would also increase the mass of cloud condensate because liquid cloud is more presistent than ice cloud due to the difference in fall velocity (liquid cloud's velocity smaller than ice cloud). This is another aspect of the negative cloud feedback described by Senior and Mitchell (1993), and may be worth mentioning in order to increase the completeness of scientific content. [Tomoo Ogura, Japan] | Thanks, change made |
| 7-434 | 7 | 16 | 42 | | | The problem with identifying and quantifying optical depth feedback in GCMs is the GCMS have marked water path biases that effectively lead to a minimization of the optical depth feedback by construction - I discuss this in GEWEX news article and the liquid water bias of CMIP5 models is documented in Jiang et al (submitted to JGR, and I can get this sent to LAs- I will be sending this to LAs of chapter 9 as well). [Graeme Stephens, USA] | this issue is now noted. |
| 7-435 | 7 | 16 | 49 | 16 | 49 | Insert "well" after "represented". [Steven Ghan, USA] | text has been changed |

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| 7-436 | 7 | 16 | 56 | | | Radar and lidar [Andrew Gettelman, USA] | fixed |
| 7-437 | 7 | 17 | 12 | | | It should be noted somewhere how the surface albedo feedbacks is in fact strongly modulated by cloud feedbacks - this has been appreciated for some time (even noted in my 1981 paper with Webster) - a more recent example of such a potential modlaltion is the study of Kay et al (2009; GRL, I think) showing how the clear skies and the resulting heating of the Arctic ocean were significant factors in the 2007 sea ice loss. [Graeme Stephens, USA] | This was discussed (and the Kay et al. paper cited) in the previous paragraph. We've reworded the first sentence to make this more obvious. |
| 7-438 | 7 | 17 | 20 | 17 | 20 | There is no section 7.2.4.3.7 (nor any division of 7.2.4.3 into further subsections). [Anthony Del Genio, USA] | corrected |
| 7-439 | 7 | 17 | 20 | | 20 | The referenced section cannot be found. [Kuan-Man Xu, USA] | corrected |
| 7-440 | 7 | 17 | 20 | | | the section number 7.2.4.3.7 is not correct. [Minghuai Wang, United States of America] | corrected |
| 7-441 | 7 | 17 | 23 | 17 | 23 | Ch7 and Ch8 have agreed on using 'rapid response' instead of 'fast adjustement' [Gunnar Myhre, Norway] | now using mutually agreed terminology |
| 7-442 | 7 | 17 | 35 | 7 | 40 | I think the term "fast adjustment" is misleading here. That term usually refers to times of weeks, as in adjusted radiative forcing. The SST not catching up to CO2 changes is on a scale of decades or longer. [Daniel Murphy, United States of America] | text reworded to clarify. There are subtleties here involving how exactly "rapid responses" are defined, and they do in principle include transient changes to SST patterns for a given mean surface temperature. |
| 7-443 | 7 | 17 | 39 | 17 | 40 | I think that there are earlier studies which made the point about CO2 increases acting to reduce global precipitation. For example, Mitchell et al 1987 and Allen and Ingram 1992. Refs: Mitchell, J.F.B., C.A. Wilson, and W.M. Cunningham, 1987: On CO2 climate sensitivity and model dependence of results. Q. J. R. Meteorol. Soc., 113, 293–322. Allen, M.R., and W.J. Ingram, 2002: Constraints on future changes in climate and the hydrologic cycle. Nature, 419, 224–232. [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | this is now discussed in a dedicated subsection (7.6.2). Allen and Ingram cited in 7.5.2 |
| 7-444 | 7 | 17 | 52 | 17 | 56 | I am not sure that Williams and Webb (2009) is relevant to this point. Also, Yokohata et al (2010) found such a relationship in the HadCM3 ensemble but not the MIROC ensemble. Perhaps citing Collins et al 2010 would help to support the point that the relationships seen in parameter perturbed ensembles don't appear as strongly in the multi-model ensemble. Ref: Collins M, Booth BBB, Bhaskaran B, Harris G, Murphy JM, Sexton DMH, Webb MJ (2010) A comparison of perturbed physics and multi-model ensembles: Model errors, feedbacks and forcings. Clim Dyn. [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | Thanks, change made |
| 7-445 | 7 | 17 | 52 | 18 | 1 | There is an important caveat to all of the observables suggested here: they arise from climate model calculations alone, with weak theoretical and/or observational basis. It’s not clear that observables derived in this way provide any real constraint at all (e.g. doi:10.1175/2011JCLI4193.1). [Robert Pincus, USA] | this is kind of the point of the whole section. The suggested paper is now cited. |
| 7-446 | 7 | 17 | 53 | 17 | 53 | What are these "nonstandard parameter settings", any example? [Chien Wang, United States of America] | Space limitations require that interested readers go to the cited studies for details. |
| 7-447 | 7 | 17 | 56 | | | the revised version of Gettelman et al 2011a is equivocal: the CMIP3 archive does show weak evidence of these relationships in the latest version: see if that will hold up in review. [Andrew Gettelman, USA] | text revised; main point will not change. |
| 7-448 | 7 | 17 | | | | I haven't noted any discussion of moid-level clouds in the sections on cloud simulation by climate models. Are these unimportant? If so this should be noted. [Larry Thomason, United States of America] | There was and still is a section on this (now 7.2.4.3.2) but there is not much published literature available. |
| 7-449 | 7 | 18 | 1 | 18 | 3 | Actually, a lot of what Trenberth and Fasullo are talking about is not a positive feedback, but rather the fact that a negative feedback that exists in the CMIP3 models is an artifact. [Anthony Del Genio, USA] | the mechanistic aspects of their explanation are discussed earlier (better, thanks to your earlier comment). Here it doesn't really matter, we are merely discussing statistical associations reported in the literature. In most cases they don't even p |
| 7-450 | 7 | 18 | 3 | | | Gettelman et al 2011a does argue for a physical mechanism for how cloud feedbacks in these regions are related to the spread in climate sensitivity, and how cloud microphysics (and water path) radiatively might be related to sensitivity [Andrew Gettelman, USA] | A number of mechanisms have been suggested and are noted. |

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| 7-451 | 7 | 18 | 4 | | | I think this should be HadGEM1 rather than HadCM3. [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | thanks |
| 7-452 | 7 | 18 | 10 | 18 | 10 | Change "connexions" to "connections"? [Steven Ghan, USA] | fixed |
| 7-453 | 7 | 18 | 10 | | | I think this is a reasonable assessment and nicely done. [Andrew Gettelman, USA] | Thanks |
| 7-454 | 7 | 18 | 12 | 18 | 25 | Employing a combination of satellite datasets, Zelinka and Hartmann (2011c) determine positive cloud SW feedbacks and negative LW cloud altitude feedbacks, in contrast to positive LW feedback in models (Zelinka and Hartmann, 2010) but demonstrating the physical basis relating to upper tropospheric detrainment level [Zelinka, M. D., and D. L. Hartmann (2011), The observed sensitivity of high clouds to mean surface temperature anomalies in the tropics, J. Geophys. Res., 116, D23103, doi:10.1029/2011JD016459]. [Richard Allan, UK] | Text wording has been adjusted. |
| 7-455 | 7 | 18 | 12 | | | "long term"; is this necessary? is there a "short term sensitivity"? suggest distinguish or strike. [Stephen E Schwartz, USA] | change made |
| 7-456 | 7 | 18 | 13 | 18 | 23 | To shorten I suggest deleting the second and last sentences. [Daniel Murphy, United States of America] | It is essential for the report to address peer-reviewed papers claiming to provide evidence for strong negative cloud feedback, so these two sentences were not deleted. |
| 7-457 | 7 | 18 | 25 | | | Good summary [Andrew Gettelman, USA] | Thanks |
| 7-458 | 7 | 18 | 28 | 18 | 29 | "consistently when applied to climate models", add "differen"t before "climate models"? [Chien Wang, United States of America] | Thanks, change made |
| 7-459 | 7 | 18 | 30 | 18 | 33 | It might also be useful to cite Williams, Ingram and Gregory (2008) in support of this point. Ref: Williams KD, Ingram WJ, Gregory JM (2008) Time variation of effective climate sensitivity in GCMs. J Climate 21 (19):5076–5090, DOI 10.1175/2008JCLI2371.1 [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | Thanks, this is a very useful paper. |
| 7-460 | 7 | 18 | 35 | | 35 | The referenced section cannot be found. [Kuan-Man Xu, USA] | fixed |
| 7-461 | 7 | 18 | 38 | | | Highlight that climate sensitivity and cloud feedback are likely state dependent. [Andrew Gettelman, USA] | This was noted but a bit more is now said, including links to Chapter 5. |
| 7-462 | 7 | 18 | 41 | | | I would propose you use a figure from Andrews et al., 2012 (Submitted to ??) who have quantified cloud feedbcks in CMIP5 SST experiemtns via the Gregory method - these show the range in cloud feedbacks again map one to one with the range in model sensitivity. Not sure fo the fate of the paper but it was submitted around November last year - Piers could check with Tim. [Graeme Stephens, USA] | we have this paper and are considering such a figure |
| 7-463 | 7 | 18 | 46 | 18 | 49 | Unless I missed it, this sentence is the only mention of the water vapor and lapse rate feedback in the entire document other than the Executive Summary. This is terrible. The discussion, such as it is, blows off the feedback as simply being determined by Clausius-Clapeyron to a first approximation. But it is just not that simple for the middle and upper troposphere, which do not directly experience the evaporative source of increased water vapor but rather depend on the transport by convection and the large-scale dynamics to determine its humidity. Since AR4 there has been a major thread in the literature about the temperature of last saturation (advection-condensation) conceptual model of what determines the distribution of water vapor inthe atmosphere. Surely this AR5 report deserves some discussion of what has gone on in that area and how it explains the water vapor feedback that the AR5 models get. Regarding the lapse rate feedback, how confident are we that we have gotten that right, in light of continuing uncertainty about how well models simulate upper vs. lower troposphere temperature trends over the 20th Century? Even Zelinka and Hartmann in their most recent paper show that the AR4 models' lapse rate anomalies above about 400 mb deviate from what is observed. Finally, one thing I have been shocked about is the almost complete absence of discussion of the perturbed parameter ensemble experiments and what they have contributed to the climate feedback discussion. I raise this issue here because the parameter that makes the most difference to the climate | The submitted draft unfortunately omitted two pages covering the water vapour feedback. This material has now been restored. The entrainment issue is mentioned but is mooted by the fact that the anomalous sensitivity reported by Sanderson et al occu |

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| | | | | | | sensitivity in that ensemble (e.g., Sanderson et al. 2010) is the convective entrainment, which affects the water vapor feedback. Admittedly, they required a large and unrealistic change to the entrainment to get the large sensitivity result that they did. But might the relatively good agreement among the AR4 models hide the fact that that generation of models systematically underestimated entrainment? Has anyone done an analysis yet of the subset of AR5 models that do have stronger entrainment to see whether their water vapor/lapse rate feedbacks differ from those that do not? [Anthony Del Genio, USA] | |
| 7-464 | 7 | 18 | 46 | 18 | 49 | This is not my area, but I think it might be useful to cite more work on water vapour feedback which has emerged since AR4. For example, Ingram (2010) and Ingram (2012) apply a new 'partly Simpsonian' approach to understanding the water vapour feedback and reasons for its differences between models. Refs: Ingram WJ (2010) A very simple model for the water vapour feedback on climate change. Quarterly Journal of the Royal Meterological Society, Volume 136, Issue 646, pages 30–40, January 2010 Part A, DOI: 10.1002/qj.546 and Ingram WJ (2012) A new way of quantifying GCM water vapour feedback Clim. Dyn. 2012 10.1007/s00382-012-1294-3 (In press) [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | The submitted draft unfortunately omitted two pages covering the water vapour feedback. These pages did indeed cite the Ingram papers, as does the SOD. |
| 7-465 | 7 | 18 | 47 | | 48 | "feedback factor"; better normalized feedback strength. Friedlingstein et al J Clim 06 (multi author paper; includes fung, joos, roeckner) refers to the quantity f (in the above; they call it g in the context of carbon cycle feedbacks) as the "gain". [Stephen E Schwartz, USA] | We now use dimensional feedback strengths. |
| 7-466 | 7 | 18 | 48 | 18 | 49 | This might seem overstated in that the *magnitude* is not just from simple physical arguments - these can't do more than suggest that, given the dominance of the natural greenhouse effect by water vapour, that the increases in absolute humidity are likely to give a substantial radiative feedback. The actual magnitude depends on the fraction of emission to space which is from water vapour, which we know reasonably well, but from detailed radiative transfer calculations, not simple physical arguments. Either expand the argument to explain that or add a reference to Ingram, W., 2010: A very simple model for the water vapour feedback on climate change. Q. J. R. Meteorol. Soc., 136, 30-40 (available at http://onlinelibrary.wiley.com/doi/10.1002/qj.546/abstract) which does so. [William Ingram, UK] | The submitted draft unfortunately omitted two pages covering the water vapour feedback. These pages did indeed cite the Ingram papers, as does the SOD. |
| 7-467 | 7 | 18 | 48 | | | I suggest adding "(e.g. Ingram, 2010)" which outlines some of the fundamental physics: Ingram, W. William Ingram A very simple model for the water vapour feedback on climate change Q. J. R. Meteorol. Soc. 2010 136 646 30--40 10.1002/qj.546 (the following paper by the same author is also relevant: W. J. Ingram A new way of quantifying GCM water vapour feedback Clim. Dyn. 2012 10.1007/s00382-012-1294-3 (in press) [Richard Allan, UK] | The submitted draft unfortunately omitted two pages covering the water vapour feedback. These pages did indeed cite the Ingram papers, as does the SOD. |
| 7-468 | 7 | 18 | 51 | 18 | 52 | I could not disagree more with this statement. Yes, there are individual positive feedbacks that seem pretty solid (cloud height, storm track shift). And there is not yet a negative cloud feedback that seems solid. But subtropical stratocumulus and cumulus are the 8000 pound gorilla in the room. In generations of GCMs before AR4, there were in fact models that achieved a negative cloud feedback in all likelihood, despite having the positive cloud height and storm track feedbacks that the current generation of models has. What is the difference? The negative low cloud feedback results have mostly disappeared (though according to Chapter 9 one of the AR5 models has a 2.08 degree climate sensitivity - is that model getting a negative cloud feedback?). One can argue that this is the result of more physically based boundary layer cloud representations such as Lock and Bretherton-Park. But do we actually have confidence in the low cloud feedback that these or any other models produce? If memory serves me, Lock's LES model gives a different sign low lcoud feedback in the CGILS intercomparison than his PBL parameterization does in the Hadley Centre GCM. From where I sit, I see no reason to rule out a negative - even a strong negative - low cloud feedback at this time. That will not change until we have a physical story to tell about why that feedback is not negative: The PBL deepening argument as a dominant factor will have to be refuted, for example, or at least shown to be confidently offset by an increase in cloud top entrainment of dry air, rising cloud base, reduction of liquid water content by drizzle, or something else. Right now, my sense of things is that all these mechanisms are still out there, competing in different ways even in different LES models to give different results. If the authors wish to point out that no GCMs get a negative overall cloud feedback because of the more well established positive components of the feedback, that is fine. But to suggest that there is "evidence" for a "strong positive" feedback is just going too far. [Anthony Del Genio, USA] | The combined use of "evidence" and "substantial" was indeed an inadvertent overstatement and the text has been reworded. |

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| 7-469 | 7 | 18 | 51 | 18 | 52 | As with the comment about the summary, this strong statement is not consistent with the evidence presented in the chapter. [Robert Pincus, USA] | The combined use of "evidence" and "substantial" was indeed an inadvertent overstatement and the text has been reworded. |
| 7-470 | 7 | 18 | 52 | | | Substantial ? I am not convinced and this chapter isnt convincing [Graeme Stephens, USA] | The combined use of "evidence" and "substantial" was indeed an inadvertent overstatement and the text has been reworded. |
| 7-471 | 7 | 18 | 55 | | | AR4 (Andrews et al., 2012). [Graeme Stephens, USA] | Reference added |
| 7-472 | 7 | 18 | 55 | | | A recently submitted study on the CMIP5 models by Andrews et al might be useful to cite in support of this point. Ref: Andrews T, Gregory JM, Webb MJ, Taylor KE (2012) Forcing, Feedbacks and Climate Sensitivity in CMIP5 Coupled Atmosphere-Ocean Climate Models. (Submitted to Geophysical Research Letters) [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | Reference added |
| 7-473 | 7 | 18 | 57 | | | Might clarify that this is mostly low and mid level cloud in the sub-tropics. [Andrew Gettelman, USA] | most recent studies (e.g. Vecchi and Soden 2011) show that clouds contribute to the spread more or less everywhere. |
| 7-474 | 7 | 19 | 2 | | | It might be good to mention that much of the understanding has come through better diagnostics and use of Radiative kernels to diagnose feedbacks and remove them from cloud radiative forcing changes (Soden et al 2008; Shell et al 2008) [Andrew Gettelman, USA] | agreed, more is now said about this. |
| 7-475 | 7 | 19 | 5 | 19 | 5 | Please insert "are expected" after "high clouds" [Jón Egill Kristjánsson, Norway] | Thanks, change made |
| 7-476 | 7 | 19 | 9 | 19 | 10 | The sentence "consistent with ….. more water vapour" is not easy to follow. For instance, what is meant by "upward, cloudy air fluxes"? Are the fluxes themselves cloudy? And, what is meant by "balance the hydrological cycle"? [Jón Egill Kristjánsson, Norway] | wording changed |
| 7-477 | 7 | 19 | 13 | | | Changing aerial extent of cloud regimes also matters here. [Andrew Gettelman, USA] | agreed |
| 7-478 | 7 | 19 | 18 | 19 | 20 | Might reword something like: "… particularly for low cloud amount -- available indications, both from observations and modeling, favour a positive net cloud feedback of uncertain magnitude, rather than a negative one." [Ralph Kahn, United States of America] | Thanks, this has been revised. |
| 7-479 | 7 | 19 | 22 | 19 | 32 | I really advise against use of the formal IPCC term "likely" at this stage. When there is a story that can be told about the fundamental physics that rules out a strong negative low cloud feedback, then it will be appropriate to use such language. If such a story exists, tell it. If not, don't take this step. [Anthony Del Genio, USA] | The reviewer is incorrect in stating that a "likely" designation "rules out" the alternative: "likely" only means 2/3 chance or greater. Our options are (1) not to make a likelihood statement at all, (2) say "about as likely as not," or (3) say "like |
| 7-480 | 7 | 19 | 22 | | | "feedback" --> "cloud feedback" [Richard Allan, UK] | wording changed |
| 7-481 | 7 | 19 | 23 | | | "…than a negative one although there is no robust observational evidence to confirm this." [Richard Allan, UK] | wording changed |
| 7-482 | 7 | 19 | 24 | | | "…from GCMs and interpretation through simple physical models." (for example Hartmann and Larson FAT hypothesis) [Richard Allan, UK] | this section is now rewritten. |
| 7-483 | 7 | 19 | 26 | 19 | 26 | Again, a minor rewording suggestion: "...strong enough to outweigh convincingly the known sources…" [Ralph Kahn, United States of America] | this section is now rewritten. |
| 7-484 | 7 | 19 | 27 | 19 | 31 | Karlsson et al. 2008 suggest that CMIP3 models tend to overestimate feedback by low level subtropical clouds. Karlsson, J., Svensson, G. and Rodhe, H. 2007. Cloud radiative forcing of subtropical low level clouds in global models. Climate Dynamics. http://www.springerlink.com/content/b4r17t0614104114/?p=3321f972ed6443a99724fe9ec0d9dd2e&pi=0 [Henning Rodhe, Sweden] | we are aware of this study, but do not believe that the suggestion constitutes evidence. In fact other authors have drawn the opposite conclusion from the same biases (Stephens 2010). |
| 7-485 | 7 | 19 | 46 | 19 | 46 | The title of 7.2.5 is somewhat unclear. [Chien Wang, United States of America] | Yes, we now have a new title. |

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| 7-486 | 7 | 19 | 46 | | | This section needs an overhaul - there should be beginning discussion about how global precipitation changes, how this is related to radiation and water vapor feedback and why preciptation increases do not track track water vapor increases in a warming climate. The explanations are given in my papers (Stephens and Ellis 2009, J Climate, and Stephens and Hu, ERL 2010) - this should be followed by the regional discussion - wet wetter dry drier ultimately leading to discussion about intensity and freqeuncy of precipitition. [Graeme Stephens, USA] | agreed, we now have created a new section on precipitation to address these concerns. |
| 7-487 | 7 | 19 | 48 | 20 | 55 | Absurd models with unconvincing results [VINCENT GRAY, NEW ZEALAND] | no action requested |
| 7-488 | 7 | 19 | | | | Section 7.2.4.8 can be linked to the next section 7.2.5 by considering the commonality between temperature response to radiative forcing and precipitation response to radiative forcing. Andrews et al. (2010) and Ming et al. (2010) propose a hydrological sensitivity parameter, which is related to climate sensitivity, ansd also is affected by fast adjustments and slow SST-dependent changes. Figure 4 of O'Gorman et al. (2012) summarises the components of hydrological response to SST-dependent changes relating to Plankian, water vapour, lapse rate, cloud and sensible heat and is comparable to Figure 7.5 of this chapter. [Richard Allan, UK] | agreed, we now have created a new section on precipitation to address these concerns. |
| 7-489 | 7 | 20 | 25 | 20 | 25 | "factors other than surface temperature", need to be more specific. [Chien Wang, United States of America] | This section has been extensively revised |
| 7-490 | 7 | 20 | 25 | 20 | 27 | Could insert a short discussion that precipitation is governed by latent heat exchange as well as water availability, and the heat exchange scales more slowly with temperature than Clausius-Clapeyron so changes in precipitation patterns are expected. [Daniel Murphy, United States of America] | This section has been extensively revised |
| 7-491 | 7 | 20 | 27 | 20 | 36 | Please include the findings of the study by Lenderink et al. (2011) on extreme hourly precipitation. Their study showed that the trends in hourly precipitation extremes show substantial increases over the last century for both De Bilt (the Netherlands) and Hong Kong. There is a very close resemblance between variations in dew point temperature and precipitation intensity with an inferred dependency of hourly precipitation extremes of 10 to 14% per degree for dew points below 23oC. References :<br>- Lenderink, G., H. Y. Mok, T. C. Lee, and G. J. van Oldenborgh (2011), Scaling and trends of hourly precipitation extremes in two different climate zones—Hong Kong and the Netherlands, Hydrol. Earth Syst. Sci. 15, 3033–3041, 2011 doi:10.5194/hess-15-3033-201 [Tsz-cheung Lee, Hong Kong] | the requested reference has been considered |
| 7-492 | 7 | 20 | 27 | 20 | 36 | Please include the findings of the study by Utsumi et al. (2011) on extreme daily precipitation intensity and changes in extreme precipitation on sub-hourly timescale. They reported that the changes in the extreme daily precipitation intensity with the daily surface air temperature vary in different latitudes. The analysis using sub-hourly timescale data in Japan revealed that the decrease in the extreme daily precipitation intensity at high temperatures was well explained by the decrease in the duration of the precipitation events, not by the decrease in the precipitation intensity of individual storms. When the timescale was changed from daily to 10-min, the rate of increase in the extreme precipitation intensity came closer to the Clausius–Clapeyron rate. This implies the potential of warming to intensify extreme precipitation on sub-hourly timescales. References :<br>- Utsumi, N., Seto, S., Kanae, S., Maeda, E. E., and Oki, T.: Does higher surface temperature intensify extreme precipitation?, Geophys. Res. Lett., 38, L16708, doi:10.1029/2011GL048426, 2011. [Tsz-cheung Lee, Hong Kong] | the requested reference has been considered |
| 7-493 | 7 | 20 | 28 | | | state that CC scaling is ~7%/C [Andrew Gettelman, USA] | Done |
| 7-494 | 7 | 20 | 40 | | | "Observed relationships are sensitive to time and space-scale (Haerter et al. 2010; Liu and Allan, 2012); the highest time and space scales are poorly represented by GCMs and yet are crucial for climate impacts." [Haerter, J. O., P. Berg, and S. Hagemann (2010), Heavy rain intensity distributions on varying time scales and at different temperatures, J. Geophys. Res., 115, D17102, doi:10.1029/2009JD013384.; Liu, C., and R. P. Allan (2012), Multisatellite observed responses of precipitation and its extremes to interannual climate variability, J. Geophys. Res., 117, D03101, doi:10.1029/2011JD016568.] [Richard Allan, UK] | the requested references have been considered |
| 7-495 | 7 | 20 | | | | Section 7.2.6: A link to discussion on the fast cloud adjustment to radiative forcing could be made here since this effect equally applies to model precipitation responses (Andrews et al. 2010; Ming et al. 2010; O'Gorman et al. 2012) [Ming Y, Ramaswamy V, Persad G (2010) Two opposing effects of absorbing aerosols on global- | a revised section does note these links. |

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| | | | | | | mean precipitation. Geophys Res Lett 37:L13701; O'Gorman, P. A., R. P. Allan, M. P. Byrne and M. Previdi (2012) Energetic constraints on precipitation under climate change, Surv. Geophys., in press DOI: 10.1007/s10712-011-9159-6.] [Richard Allan, UK] | |
| 7-496 | 7 | 21 | 1 | 22 | 3 | This section should be entitled "Anthropogenic influences on cloudiness" as "sources" pre-empts the answer to the net effect of anthropogenic influence [Richard Betts, United Kingdom of Great Britain & Northern Ireland] | The suggested title would be too broad, as it would include cloud feedbacks from climate change. The point is well taken, but on the other hand, title does not pre-empt, since it is based on the existing literature which does not give any reason to expect an anthropogenic sink of cloudiness. "Sources" best captures what is included in this part of the assessment. |
| 7-497 | 7 | 21 | 3 | 21 | 6 | Would it not be incorrect? As all hydrocarbon combustion could produce H2O as a by-product, the ever increasing burning of hydrocarbons (both as fossil fuel and combustible biomass) is expected to have a non-negligible contribution to radiative forcing, though it would be much smaller that the CRE. Some of it could be comparable to aerosol or contrail effects. I feel this should be discussed with some quantitative figures. [K KRISHNA MOORTHY, INDIA] | the statement is correct as it is. The emission of water vapour from hydrocarbon combustion is very small compared to the natural evaporation rate. Contrails only have an effect because aircraft trigger the condensation of ambient water vapour. Only irrigation or changes in land use can modify the water vapour flux in a significant way. This is very briefly discussed in the chapeau to the subsection. |
| 7-498 | 7 | 21 | 3 | 22 | 3 | The main anthropogenic effect on water vapour is the efforts to reduce evaporation by reservoirs, culverts, roads and .buildings. The rest conceals our basic ignorance of water vapour in the atmosphere and its distribution over small and longer periods. [VINCENT GRAY, NEW ZEALAND] | we discuss the impact of land use on atmospheric water vapour. |
| 7-499 | 7 | 21 | 8 | 21 | 49 | In section 7.2.6.1 more emphasis should be put on the regional distributions of the contrail and contrail-cirrus forcing, as regions with higher air-traffic are prone to a much higher forcing than regions with lower traffic. [Andrew Ferrone, Germany] | it is now stated that the RF is very heterogeneous. |
| 7-500 | 7 | 21 | 8 | | | There is a lot written here about a very samll effect - I would slash this section a lot [Graeme Stephens, USA] | the section has been shortened. |
| 7-501 | 7 | 21 | 10 | | | Suggest being consistent in use of 'contrail-induced' vs 'aviation-induced' (ln 24) and define 'cirrus sheets' with your choice between the former terms. [David Fahey, USA] | we chose to say "aviation-induced" cloudiness consistently. However the point is that contrails can evolve into something that is a cirrus cloud. |
| 7-502 | 7 | 21 | 10 | | | Suggest changing to 'Aviation jet engines emit hot, moist air along with particles which can lead to the formation of line-shaped, persistent contrails.….' [David Fahey, USA] | changed. |
| 7-503 | 7 | 21 | 12 | 21 | 13 | In addition to Haywood et al. (2009), you should also refer to Burkhardt and Kärcher (2011) regarding the positive radiative forcing from cirrus and contrails. [Borgar Aamaas, Norway] | Burkhardt and Kärcher (2011) is not an observational study (unless you are refeering to a different study than that references in the chapter). Burkhardt and Kärcher (2011) is discussed further down in the subsection. |
| 7-504 | 7 | 21 | 12 | | | The use of 'their' is ambiguous: is it contrails or aviation-induced cirrus of both? [David Fahey, USA] | TBC |
| 7-505 | 7 | 21 | 22 | | | Suggest that this recent work be cited since it is constrained by in situ observations which is rare. "Radiative transfer estimates using the in-situ measured contrail optical depth lead to a year-2005 estimate of line-shaped contrail radiative forcing of 15.9 mWm−2 with an uncertainty range of 11.1– 47.7 mWm−2" in Voigt, C., U. Schumann, P. Jessberger, T. Jurkat, A. Petzold, J.-F. Gayet, M. Krämer, T. Thornberry, and D. W. Fahey (2011), Extinction and optical depth of contrails, Geophys. Res. Lett., 38, L11806, doi:10.1029/2011GL047189. [David Fahey, USA] | reference included. |
| 7-506 | 7 | 21 | 28 | 21 | 28 | Change 'contributed to lessen' to 'lessened' [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | changed. |

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| 7-507 | 7 | 21 | 30 | | | Change to: '...0.020 Wm-2 for linear contrails for the reference…..' [David Fahey, USA] | changed. |
| 7-508 | 7 | 21 | 33 | 21 | 33 | Has "efficacy" been defined before this point? [Steven Ghan, USA] | Good point, this will be mentioned where adjustments are first introduced. |
| 7-509 | 7 | 21 | 33 | 21 | 36 | The impact of aviation contrails, in the presence of clear-sky and cirrus cloud conditions, on regional climate needs to be assessed. [Panuganti China Sattilingam Devara, India] | it has been assessed by Rap et al. (2010a) who found little impact if any on regional climate. |
| 7-510 | 7 | 21 | 33 | 21 | 36 | Is it right? Contrails, per say, may not contribute significantly, though its magnitude still remains uncertain. But, when several such small contributions add, for example with similar minor effects of water vapour from increased fuel use, brown carbon etc.. [K KRISHNA MOORTHY, INDIA] | the sentence is about contrails and it correct as such. |
| 7-511 | 7 | 21 | 38 | 21 | 49 | It seems to me that discussions of such forcing associated with contrail would better serve the purpose by emphaizing on the regional effects rather than global forcing and response. By the way, to be consistent with later writings and also easier for the reader should mWm^-2 be used in all the cases where forcing qunatity is significantly smaller than 1? [Chien Wang, United States of America] | it is now stated that the RF is very heterogeneous. TBC unit |
| 7-512 | 7 | 21 | 41 | 21 | 44 | Please specify the period for which the values of RF reported here have been considered [Andrew Ferrone, Germany] | year 2000 TBC |
| 7-513 | 7 | 21 | 42 | | | An upcoming study (Chen and Gettelman, to be submitted by spring) analyzed aviation effects on cirrus clouds. This treats linear cirrus, and aviation effects on natural cirrus (similar to Burkhardt and Karcher). Paper will probably indicate RF is also 0.03Wm-2, with some uncertainty (TBD) [Andrew Gettelman, USA] | please provide study |
| 7-514 | 7 | 21 | 46 | | | Change to: 'As the Kaercher and Burkhardt study is a major step….' [David Fahey, USA] | changed |
| 7-515 | 7 | 21 | 46 | | | Which study are you referring to? ("this study") Burkhardt & Karcher? [Andrew Gettelman, USA] | yes, sentence modified. |
| 7-516 | 7 | 21 | 51 | 22 | 3 | "Regional surface cooling (due to irrigation) was confirmed by a number of more recent regional and global studies." Please include a discussion of the results in Jacobson, M.Z., The short-term effects of agriculture on air pollution and climate in California, J. Geophys. Res., 113, D23101, doi:10.1029/2008JD010689, 2008. [Mark Z. Jacobson, U.S.A.] | references noted but it is not possible to cite all articles on irrigation here. The results are consistent with other studies. |
| 7-517 | 7 | 21 | 56 | | | Summarize effects and mechanisms a bit better. How do you get positive or negative RF values? Negative = increased cloudiness, positive = increased water vapour. [Andrew Gettelman, USA] | yes, water vapour exerts a positive RF as stated on line 53. Increased cloudiness could cause a negative AF. But the climate effect is dominated by evaporative cooling as stated on line 54. There is no space for further development on irrigation. |
| 7-518 | 7 | 22 | 4 | 22 | 4 | I suggest there should be a short section on the influence of CO2 physiological forcing on cloud cover (direct effects of CO2 on transpiration by plants). As mentioned in chapter 8 (page 31 lines 33-35) there is considerable evidence that CO2 exerts a significant effect on transpiration, and modelling studies such as the one by Doutriaux-Boucher et al (2009) GRL suggest that this reduces low-level cloud, enhancing the CO2 radiative forcing by about 10%. [Richard Betts, United Kingdom of Great Britain & Northern Ireland] | This is now mentioned among other rapid adjustments to CO2. |
| 7-519 | 7 | 22 | 5 | | | There have been recent claims of solar-activity modulated aerosol formation by cosmic rays affecting the clear-sky transmission of the atmosphere (W. Weber, Ann. Phys. (Berlin) 522, 372–381 (2010), doi:10.1002/andp.201000019; Hempelmann & Weber (2012), Solar Physics, in press, doi:10.1007/s11207-011-9905-4). These claims have been refuted (G. Feulner, Atmos. Chem. Phys. 11, 3291–3301 (2011), doi:10.5194/acp-11-3291-2011). This could be discussed in this chapter. [Georg Feulner, Potsdam] | a better location for this material if relevant would be in Chapter 2 (brightening / dimming) or in Chapter 8 (discussion of the solar forcing). |
| 7-520 | 7 | 22 | 9 | 22 | 10 | The limitation to anthropogenic changes of the aerosol jumps much too short. The topic of the IPCC is "Climate Change", which could be natural or anthropogenic or both. Otherwise the IPCC should be renamed to IPACC. [Ruprecht Jaenicke, Germany] | This chapter discusses both natural and anthropogenic aerosol but necessarily focusses on the anthropogenic component |
| 7-521 | 7 | 22 | 9 | 22 | 38 | You ignore completely the very different role of aerosols between day and night. By day they cool the earth by blocking sunshine and so warm the atmosphere. By night they reduce cooling of the surface by returning part of the earth's radiation. These diffeent roles cannot be satisfactorily averaged, Aerosols also provide nuclei for | This is not ignored, models and observations account for these diurnal effects. However, the focus of the report is long-term climate change so they need not |

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| | | | | | | cloud formation, mostly by day. [VINCENT GRAY, NEW ZEALAND] | be explictly addressed here |
| 7-522 | 7 | 22 | 9 | | | Strictly speaking, it should rather be "aerosol particles"  [Henning Rodhe, Sweden] | We have edited to explicitly define "particles" at start but then use aerosols throughout |
| 7-523 | 7 | 22 | 17 | 22 | 17 | Change The to This [James Hudson, USA] | typo corrected |
| 7-524 | 7 | 22 | 23 | 22 | 23 | Should "remote observation" be "remote sensing data"? [Chien Wang, United  States of America] | Introductory text deleted |
| 7-525 | 7 | 22 | 25 | 22 | 25 | "in-situ measurement and remote sensing" reads much better than "in-situ and remote observations". [Chien Wang, United  States of America] | Introductory text deleted |
| 7-526 | 7 | 22 | 30 | | | Has anything fundamental on aerosols been learned? Mostly this is moving more detailed aerosol representations into climate models. [Andrew Gettelman, USA] | Text revised to highlight progress |
| 7-527 | 7 | 22 | 32 | | | emissions and removal [Henning Rodhe, Sweden] | emissions are a more important source of uncetainty than removal processes, so text is retained |
| 7-528 | 7 | 22 | 40 | 24 | 44 | <Aerosol Sources and Processes>   I think the emission estimates of aerosols from each source should be given here.   In this current manuscript, we can find only the uncertainty span of the emission estimates of aerosols from each source: for example, "The range of estimates for the global dust emission span a factor of about 5."  "The range of the current estimates of global DMS emission spans a factor of about 2."  "..., the recent estimates being mostly within a factor of 3." (for the aerosols from BVOCs)<br>There should be some description on secondary aerosols derived from reactive halocarbons in this section. [Yoko Yokouchi, Japan] | the table providing aerosol source estimates were revised to include more components. |
| 7-529 | 7 | 22 | 40 | 24 | 46 | Section 7.3.2. Quantitative estimates of aerosol source strength should be given. The paragraphs on the different aerosol types end up stating things like "range of estimates spans a factor of 5"  but don't give any values. If the authors don't want to commit themselves, they could at least cite the recent review by Andreae & Rosenfeld, Earth Science Reviews, 2008, 13-41. [Meinrat O. Andreae, Germany] | the table providing aerosol source estimates were revised to include more components. |
| 7-530 | 7 | 22 | 40 | | | Section 7.3.2: More detailed information on the sources and the relative amount of species from the same source will be helpful.  [Shigeki KOBAYASHI, Japan] | the table providing aerosol source estimates were revised to include more components. |
| 7-531 | 7 | 22 | 40 | | | Section 7.3.2: would recommend adding a few references on the progress in atmospheric remote sensing of important aerosol and precursor species. While emission inventory is becoming increasingly available for different regions, they often lag behind the vibrant economy of many developing regions. Satellite observations, while still have large uncertainties, have been shown to allow for validation and timely update of emission inventories of SO2, NOx, as well as biogenic VOC. Examples include:  [Can Li, United  States of America] | Remote sensing as a tool to constrain emission estimates is mentioned in the revised text. |
| 7-532 | 7 | 22 | 40 | | | Zhang, Q., D. G. Streets, and K. He (2009), Satellite observations of recent power plant construction in Inner Mongolia, China, Geophys. Res. Lett., 36, L15809, doi:10.1029/2009GL038984. [Can Li, United  States of America] | Remote sensing as a tool to constrain emission estimates is mentioned in the revised text. We have added one citation on this issue. |
| 7-533 | 7 | 22 | 40 | | | Li, C., Q. Zhang, N. A. Krotkov, D. G. Streets, K. He, S.-C. Tsay, and J. F. Gleason (2010), Recent large reduction in sulfur dioxide emissions from Chinese power plants observed by the Ozone Monitoring Instrument, Geophys. Res. Lett., 37, L08807, doi:10.1029/2010GL042594. [Can Li, United  States of America] | Remote sensing as a tool to constrain emission estimates is mentioned in the revised text. We have added one citation on this issue. |
| 7-534 | 7 | 22 | 42 | 22 | 42 | Delete "distinctively"; the concept of semivolatile emissions mixes these somewhat. [Daniel Murphy, United States of America] | English corrected |
| 7-535 | 7 | 22 | 42 | 24 | 44 | Section 7.3.2 Aerosol sources: Primary biological particles (PBAP) are missing, although mentioned in table 7.1. The relevance for cloud formationa and prcipitation of supermicron PBAP particles is highlighted in Pöschl et al., Rainforest Aerosols as Biogenic Nuclei of Clouds and Precipitation in the Amazon, Science 329, 1513 (2010) [Johannes Schneider, Germany] | consider citing |
| 7-536 | 7 | 22 | 49 | | | Replace "shape" with morphology [Henning Rodhe, Sweden] | uncesessary, "shape" is simpler |

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| 7-537 | 7 | 23 | 1 | 23 | 4 | Main chemical constituents. Does "chemical" mean that biological constituents are not included? In any circumstance, biological partciles are a main constituent and should not be omitted. [Ruprecht Jaenicke, Germany] | reworded as "main constituents, biological constituents are a subgroup of organic aerosol" |
| 7-538 | 7 | 23 | 1 | 23 | 15 | The concept of coarse and fine modes should be introduced before the flux estimates [Daniel Murphy, United States of America] | agreed, text rearranged |
| 7-539 | 7 | 23 | 2 | 23 | 2 | Replace "species" with "salts", as inorganic and organic species covers all species. BC and mineral dust are both inorganic. [Steven Ghan, USA] | species is written for an inexpert readership. "species" retained |
| 7-540 | 7 | 23 | 2 | | | The word "species" belongs to biology. In chemistry it is compounds or constituent [Henning Rodhe, Sweden] | species is written for an inexpert readership. "species" retained |
| 7-541 | 7 | 23 | 5 | 23 | 5 | A lot of sulphate is produced directly (primary) or at least the conversion is so quick that it might as well be primary for all observational purposes. [James Hudson, USA] | Reworded as "...formed dominantly in the atmosphere" |
| 7-542 | 7 | 23 | 8 | | 9 | "SOA is to larger extent of natural origin". I am surprised at the confidence of this statement. Certainly SOA is enhanced in air influenced by anthro emissions (Kroll and Seinfeld 08; Hallquist et al 09). Based on aircraft measurements of urban-influenced air over New England, de Gouw et al. (2005) found that POM was highly correlated with secondary anthropogenic gas phase species suggesting that the POM was derived from secondary anthropogenic sources and that the formation took one day or more. Even if the SOA is modern (as indicated by 14C measurements, the conversion can be greatly enhanced by the active photochemistry induced by anthropogenic nitrogen oxides (Kroll and Seinfeld 08; Hallquist et al 09).<br><br>de Gouw, J., et al., 2005: Budget of organic carbon in a polluted atmosphere: Results from the New England Air Quality Study in 2002. Journal of Geophysical Research, 110, D16305, doi:10.1029/2004JD005623. [Stephen E Schwartz, USA] | The text on SOA formation and its connection with natural and anthropogenic sources was revised. |
| 7-543 | 7 | 23 | 8 | | | POA over oceans likely natural. Facchini, M.C. et al., 2008. Primary submicron marine aerosol dominated by insoluble organic colloids and aggregates. Geophysical Research Letters, 35(17): L17814; Bigg, E.K. and Leck, C., 2008. The composition of fragments of bubbles bursting at the ocean surface. Journal of Geophysical Research, 113(D11209): doi:10.1029/2007JD009078. [Henning Rodhe, Sweden] | POA over ocean is included in current sea spray source functions. The text was modified. |
| 7-544 | 7 | 23 | 9 | 23 | 10 | "Despite earlier recognition of their climatic importance (Adams et al., 2001), it is only recent that nitrate aerosols are being considered in a wider set of models." Concurrently with Adams et al., the direct radiative forcing of nitrate aerosols was reported in Jacobson, M.Z., Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001, published in January 2001. [Mark Z. Jacobson, U.S.A.] | older citation used here as an example. Extra citation not necessary |
| 7-545 | 7 | 23 | 10 | 23 | 10 | recent -> recently [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | typo corrected |
| 7-546 | 7 | 23 | 10 | | | replace "recent" with "recently". [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | typo corrected |
| 7-547 | 7 | 23 | 10 | | | Change to "…it is only recently that nitrate…" [Patricia Quinn, US] | typo corrected |
| 7-548 | 7 | 23 | 11 | | | Table 1 does not include emissions of aerosols, only emissions important for aerosol formation. Why not include emissions of aerosols (sea salt, dust) and natural gases important for aerosol formation (DMS, isoprene). [Timothy Bates, USA] | the emission table has been revised to include more components |
| 7-549 | 7 | 23 | 11 | | | "Emissions of anthropogenic aerosols and…" [Warren Richard Leaitch, Canada] | typo corrected |
| 7-550 | 7 | 23 | 21 | | | Replace "shape" by "morphology" [Henning Rodhe, Sweden] | uncesessary, "shape" is simpler |
| 7-551 | 7 | 23 | 25 | 23 | 25 | It is a general remark, several references are not the first ones exploring the topics. It will be preferable to cite as (e.g. names, date) [Patrick CHAZETTE, France] | The text was shortened and number of citations reduced. |
| 7-552 | 7 | 23 | 25 | 23 | 25 | More correct to say "by bubble bursting induced mostly by breaking waves"; there are a few situations such as the Arctic sea ice where bubbles without breaking waves seem to matter. [Daniel Murphy, United States of | the text reworded as suggested |

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| | | | | | | America] | |
| 7-553 | 7 | 23 | 25 | | 35 | "Since AR4, substantial progress has been made in constraining the total mass, number size distribution and chemical composition of emitted sea salt particles (Evan et al., 2008; Fuentes et al., 2011; Keene et al., 2007; Ovadnevaite et al., 2011)."<br><br>This sentence is overly optimistic and certainly does not reflect even the most cursory read of deLeeuw (2011), the authorship of which includes leading investigators on the subject from multiple institutions, which concludes (emphasis added):<br><br>Despite the many gains in understanding in recent years, the uncertainty in the SSA production flux remains sufficiently great that present knowledge of this quantity cannot strongly constrain the representation of emissions of SSA in chemical transport models or climate models that include aerosols. As a consequence, it is not yet possible to improve the modeling of these emissions much beyond the state of affairs represented in Figure 1, which shows a nearly 2 orders of magnitude spread in current estimates of global annual SSA emissions. It is also clear that this situation cannot be resolved by demonstration of the ability to generate reasonable concentration fields with one or another source function, given the demonstrated ability of such greatly varying emissions to yield concentration fields that compare reasonably with observations.<br><br>Further, the statement is not supported by the references given.<br><br>Evan 08 (which is coauthored by Myhre, contributing author of this Chapter and which should therefore be well known to the chapter authors) mentions sea salt exactly once, as follows:<br><br>Our background value [of aerosol optical depth] of 0.1 represents average levels of sulfates and sea salt (including carbonaceous aerosols) in this region, in agreement with simulations from a global aerosol-chemistry transport model [Myhre et al., 2007].<br><br>This sentence lends absolutely no support to the above quoted statement.<br><br>Figure 4 of deLeeuw explicitly compares the experimental results of Fuentes and Keene, showing differences in the several experiments of Fuentes of up to an order of magnitude, and differences between Fuentes and Keene of as much as three orders of magnitude.<br><br>The case study of Ovadnevaite presents concentration and composition measurements under conditions of high biological activity, but hardly can be viewed as constraining the total mass, number size distribution and chemical composition of emitted sea salt particles. [Stephen E Schwartz, USA] | the text on sea spray emissions was rewritten considering these points and comments by others. |
| 7-554 | 7 | 23 | 25 | | | The terminology "sea salt" (sometimes spelled with hyphen, sometimes without) is outdated and incorrect. "Sea spray" aerosol should be used instead. Later in the chapter, the text states correctly, that the submicron fraction of the sea spray aerosol consists to a great extent of organic matter. Since quite a number of years we know that the formation mechanism of sea spray aerosol, bubble bursting at the sea surface, produces particles that grade in composition from almost pure salt in the largest sizes to almost pure organic matter in the smallest sizes. It is thus incorrect to call this material "sea salt". THIS NEEDS TO BE CORRECTED IN THE ENTIRE CHAPTER! [Meinrat O. Andreae, Germany] | the term say spray, including sea salt and associated organics, is used in revised text |
| 7-555 | 7 | 23 | 30 | 23 | 30 | Not so new. To cite myself, Middlebrook et al., JGR, 103, 16475, 1998 showed the majority of submicron sea-salt particles contained organics. [Daniel Murphy, United States of America] | the text has been reworded. |
| 7-556 | 7 | 23 | 30 | 23 | 33 | Avoid using sea-salt particles to be common to both those made up of sea salt (NaCl) and of organics. Call them primary marine nascent particles. [Henning Rodhe, Sweden] | The term sea spray will be used in the revised text. |
| 7-557 | 7 | 23 | 30 | 23 | 35 | The 3 sentences "A major new observation has been the frequent presence of organic material…emitted sea-salt particles translate into a large uncertainty on the natural level of CCN in the marine atmosphere." may best be revised to: "The presence of organic particles with dry diameters in the 10-100 nm range has been observed at several marine locations (Leck and Bigg, GRL 32,2005; Bigg and Leck, J. Geophysical Res. 113, | the text on say spray emissions, including sea salt and associated organics, has been revised. |

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| | | | | | | D11209, 2008; Hawkins and Russell, Advances in Meteorology, doi:10.1155/2010/612132, 2010). They are, generally, not associated with sea salt (Bigg, Environ. Chem. 4, 2007; Leck and Bigg, Tellus 60B, 2008). These organic primary particles, given their size and chemical character, are likely to be a substantial CCN source to the marine atmosphere (Bigg, Env. Chem. 4, 2007). Given that few sea salt particles <200 nm in diameter have been found in the marine boundary layer, only a very small minority of sea salt particles act as CCN. CCN of organic particle origin are likely "the best place to look" for a major source of marine atmosphere CCN. Uncertainties in the chemical composition of sea-derived particles that are small enough to become CCN translates into large uncertainties regarding the natural level of marine atmosphere CCN. [Herman Sievering, United States of America] | |
| 7-558 | 7 | 23 | 30 | | | When discussing POA or sea salt, there should be some mention of bacteria and related gels that have been identified in many marine locations globally (see Leck and Bigg, GRL 32, 2005). Additionally Bigg (Env. Chem., 4, 155-161, 2007) suggests that these biologically derived organic primary aerosols are active as CCN. [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | The text on say spray emissions, including sea salt and associated organics, has been revised.The exact nature of this sea spray POA goes is too detailed information to be discussed here. We have discussed separately primary biological aerosol particles in the revised text. |
| 7-559 | 7 | 23 | 31 | | | Bigg, E.K. and Leck, C., 2008. The composition of fragments of bubbles bursting at the ocean surface. Journal of Geophysical Research, 113(D11209): doi:10.1029/2007JD009078. [Henning Rodhe, Sweden] | individual papers on the nature of POA associated with sea spray go in too detail to be discussed here. |
| 7-560 | 7 | 23 | 32 | | | "significant uncertainty". "Significant" is one of those weasel words the use of which should raise a red flag. Does it mean "statistically significant", which would imply just a slight difference that can be established by some test of significance? Or do the authors mean "substantial"? The reader has no idea. So the authors are at best being careless. A quantitative statement is required. DeLeeuw stated that<br><br>"The best estimate for the production flux of SSA particles with r80 > 1 μ m [has] uncertainty a multiplicative factor of ×/÷ (times/ divided by) 4 – 5. "<br><br>For smaller particles the uncertainty in production flux is greater, with recent studies showing differences of 2 or more orders of magnitude.<br><br>The uncertainty here translates directly into uncertainty in the Twomey effect of indirect radiative forcing, the magnitude of which depends strongly on the concentration of CCN absent the anthropogenic perturbation. The effect of this uncertainty is directly reflected in GCM calculations for which formulations have dealt with the issue very artificially, by constraining the minimum concentration of CCN. See page 7-48 lines 33-35. By assuming a given lower limit to cloud drop concentration it is possible to change the forcing substantially.Text should indicate what approaches are taken to constrain this quantity and assess present situation. [Stephen E Schwartz, USA] | We fully agree with this critics. The text on sea spray emissions was rewritten based on this and other comments. |
| 7-561 | 7 | 23 | 34 | 23 | 34 | I don't agree that sinks are generally better understood. See Table 5.5 and discussion on page 302 of AR3, where there were factor of 2 differences in sulfate deposition terms. And because of acid rain research sulfate is the best understood for deposition. [Daniel Murphy, United States of America] | This comment refers to the text one page later. We agree with the comments and have revised the text accordingly. |
| 7-562 | 7 | 23 | 34 | 23 | 35 | The CCN concentrations over the oceans cannot be constrained from space observations with presently available sensors. There are proposals to deploy sensors that can determine CCN concentrations from space. [Meinrat O. Andreae, Germany] | We agee. There is no conflict between our text and the fact mentioned here. |
| 7-563 | 7 | 23 | 37 | 23 | 37 | Typo: "… follwing creep and saltation of larger…" [Ralph Kahn, United States of America] | the typo was corrected. |
| 7-564 | 7 | 23 | 37 | 23 | 43 | Is dust defined as mineral particles only? Dust is a very soft (woolly) expression (Friedlander, S.K. (2000): Smoke, Dust, and Haze. Oxford University Press, 407 p) and covers not only mineral particles. The source strength of mineral particles depends very much on the largest particles taking into consideration (Jaenicke, R. (2007): Is Atmospheric Aerosol an Aerosol? - A Look at Sources and Variability. Faraday Discussions 137, 235-243). Sea salt particle production (line 25-35) are discussed at length and organic material is mentioned. However, the large source of primary biological particles is not mentioned. A comprehensive review is now in print (Viviane R. Després, J. Alex Huffman, Susannah M. Burrows, Corinna Hoose, Aleksandr S. Safatov, | The text on dust emissions was modified. PBAP are explicitly discussed in the revised text and citation to Despres et al. (2012) was added. |

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| | | | | | | Galina Buryak, Janine Fröhlich-Nowoisky, Wolfgang Elbert, Meinrat O. Andreae, Ulrich Pöschl, and Ruprecht Jaenicke (2012): Primary Biological Aerosol Particles in the Atmosphere: A Review. Tellus B, 57 p) and is available on request. Please let me know. [Ruprecht Jaenicke, Germany] | |
| 7-565 | 7 | 23 | 37 | 23 | 43 | Discuss the role of anthropogenic activities, construction of roads, buildings, other infra-structure, urbanization which are all on the rise in the recent past in several developing countries, especially in Asia. In general, most construction activities precede by demolition of some existing structures or large scale changes to the land cover, all of which generate dust with a wide spectral distribution than those generated naturally. Fine dust is increasingly becoming a significant fraction to the aerosol load [K KRISHNA MOORTHY, INDIA] | Anthropogenic contribution to dust is mentioned in the revised text. Further discussion on this issue is too detailed to be included here. |
| 7-566 | 7 | 23 | 37 | 23 | 43 | Should the soil dust from land use and land change be discussed here? [Chien Wang, United States of America] | Anthropogenic contribution to dust is mentioned in the revised text. |
| 7-567 | 7 | 23 | 40 | 23 | 42 | The following paper is also relevant for dust emissions schemes and comparisons to observations and should be added to the examples provided: Stanelle, T., Vogel, B., Vogel, H., Bäumer, D., and Kottmeier, C.: Feedback between dust particles and atmospheric processes over West Africa during dust episodes in March 2006 and June 2007, Atmos. Chem. Phys., 10, 10771-10788, doi:10.5194/acp-10-10771-2010, 2010. [Andrew Ferrone, Germany] | The revised text does not explicitly discuss dust emission schemes. |
| 7-568 | 7 | 23 | 41 | 23 | 41 | Change "observations by remote sensing" to "remote sensing data"? [Chien Wang, United States of America] | This sentence has been removed from the revised text. |
| 7-569 | 7 | 23 | 41 | 23 | 42 | You might also want to reference here: Ginoux, P., D. Garbuzov, and N. C. Hsu (2010), Identification of anthropogenic and natural dust sources using Moderate Resolution Imaging Spectroradiometer (MODIS) Deep Blue level 2 data, J. Geophys. Res., 115, D05204, doi:10.1029/2009JD012398. [Ralph Kahn, United States of America] | The revised text does not discuss dust observatoins in such details as suggested here. |
| 7-570 | 7 | 23 | 43 | 23 | 43 | Replace "span" with "spans". [Steven Ghan, USA] | The typo was corrected. |
| 7-571 | 7 | 23 | 45 | 24 | 2 | This paragraph reads more like a literature review than assessment. [Daniel Murphy, United States of America] | The text was considerably shortened and tightened´. |
| 7-572 | 7 | 23 | 54 | 23 | 54 | Replace "span" with "spans". [Steven Ghan, USA] | This sentence has been removed from the revised text. |
| 7-573 | 7 | 23 | 54 | 23 | 55 | rather than "gas-phase chemistry" and "cloud processing" it would be good to use the terms homogeneous and heterogeneous processes. Homogeneous = gas-phase and nucleation (NPF), heterogeneous = gas with aerosol (liquid/solid). [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | This sentence has been removed from the revised text. |
| 7-574 | 7 | 23 | 55 | 24 | 2 | A recent review of observations and model simulations of the impact of DMS emissions on marine CCN concentrations presents evidence that DMS does not control marine CCN concentrations. See Quinn and Bates, Nature, vol. 480, 51 - 56, 2011. [Patricia Quinn, US] | The text was revised and the reference added. |
| 7-575 | 7 | 23 | 56 | | | I am not sure we can continue to say that "DMS has a major influence on marine CCN concentrations". See Quinn and Bates (Nature, 480, December 2011 doi: 10.1038/nature10580) for an excellent review on DMS, CCN and the "CLAW" Hypothesis. Several other papers indicate that heterogeneous processes, where gas reacts with pre-exisiting aerosol is effective at removing DMS(g) from the atmosphere, limiting the contribution of DMS to CCN. [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | The reference was added and text modified accordingly. |
| 7-576 | 7 | 23 | | 24 | | Emissions, especially of primary particles. The discussion is almost entirely on primary natural particles. Recent studies (Spracklen, Merikanto, Chang give budgets of CCN or accumulation mode particles attributed to primary and secondary formation, finding the fraction of CCN or accumulation mode aerosol particles highly dependent on assumptions regarding primary emissions. Chang notes that a factor of 2 in mode radius of emitted particles corresponds to a factor of 8 in number and that primary anthropogenic emissions are generally not characterized to size and hence that such a range in mode radius is not unreasonable. Spracklen and Merikanto, examining the consequences of a smaller range of primary emissions, likewise note strong sensitivity to assumptions on primary emissions. | This is important point. The importance of uncertainties in primary particle emissions, especially in their size distribution, is mentioned in the revised text in paragraph discussing NPF. Relevant references have been added. |

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| | | | | | | Uncertainties in number concentrations of emissions translate directly into uncertainties in concentrations and in turn into uncertainties in forcing. It does not appear that the consequences of these uncertainties are adequately recognized in the discussion.<br><br>What seems to be required at minimum is a quantitative assessment of emissions across models, although this might not be enough to assess uncertainties, as many models use emissions from common sources. But I am not sure much more can be expected in the present assessment. Still it would seem that some quantitative assessment need be made as to whether the present understanding of emissions of primary particles and precursor gases is adequate to meet the requirements of representing aerosol processes and ultimately forcing in climate models; of how accurate such emissions need to be, globally, regionally, as function of secular time.<br><br>Chang L.-S., Schwartz S. E., McGraw R. and Lewis E. R. Sensitivity of aerosol properties to new particle formation mechanism and to primary emissions in a continental scale chemical transport model. J. Geophys. Res., 114, D07203 (2009); doi:10.1029/2008JD011019.<br><br>Spracklen DV et al  (2010) Explaining global surface aerosol number concentrations in terms of primary emissions and particle formation Atmos. Chem. Phys., 10, 4775–4793, 2010 www.atmos-chem-phys.net/10/4775/2010/ doi:10.5194/acp-10-4775-2010<br><br>Merikanto J et al (2009), Impact of nucleation on global CCN. Atmos. Chem. Phys., 9, 8601–8616, 2009 www.atmos-chem-phys.net/9/8601/2009/ [Stephen E Schwartz, USA] | |
| 7-577 | 7 | 24 | 1 | 24 | 2 | add "Langley, L., W. R. Leaitch, U. Lohmann, N. C. Shantz, and D. R. Worsnop Contributions from DMS and ship emissions to CCN observed over the summertime North Pacific, Atmos. Chem. Phys., 10, 1287-1314, 2010" to references.<br>[Warren Richard Leaitch, Canada] | The discussion on DMS was considerably shortened and therefore this reference was not added. |
| 7-578 | 7 | 24 | 1 | | | Add Leck, C. and K. Bigg, 2007, A modified aerosol–cloud–climate feedback hypothesis. Environmental Chemistry, 4,400–403,  doi:10.1071/EN07061. [Henning Rodhe, Sweden] | climate feedbacks related to aerosols are discussed in a separate section. |
| 7-579 | 7 | 24 | 2 | 24 | 2 | Insert<br>Faloona, I., Conley, S.A., Blomquist, B., Clarke, A.D., Kapustin, V., Howell, S., Lenschow, D.H., Bandy, A.R.: Sulfur dioxide in the tropical marine boundary layer: dry deposition and heterogeneous oxidation observed during the Pacific Atmospheric Sulfur Experiment. J. Atmos. Chem. (2010). doi:10.1007/s10874-010-9155-Gray, B. A., Wang, Y., Gu, D., Bandy, A., Mauldin, L., Clarke, A., Alexander, B., Davis, D.D.:  Sources, transport, and sinks of SO2 over the equatorial Pacific during the Pacific Atmospheric Sulfur Experiment.  J. Atmos. Chem. (2010). doi10.1007/s10874-010-9177-7 [James Hudson, USA] | The discussion on DMS was considerably shortened and therefore these references were not added. |
| 7-580 | 7 | 24 | 2 | | | Include Quinn & Bates, 2011, Nature 480, 51-56. [Timothy Bates, USA] | The suggested reference was added. |
| 7-581 | 7 | 24 | 4 | 24 | 4 | A reference to this statement would be useful [Gunnar Myhre, Norway] | The text was modified. |
| 7-582 | 7 | 24 | 6 | 24 | 9 | Two particle production processes are discussed, primary and secondary particles. If Secondary Organic Particles (SOA) are organic compounds, consequentely Primary Organic Partciles (POA) are Primary Biological Aerosol Particles (PBAP). I don't believ that as droplet expelled POA are playing a major role. Then, atmospheric POA are not dominated by anthropogenic sources. POA coarse mode is mentioned in Tab 7.2, but I don't see any further discussion of the subject. Anyhow, coarse mode is only mentioned in Tab 7.2, but not in the body of the text. [Ruprecht Jaenicke, Germany] | PBAP are now being discussed in this section |
| 7-583 | 7 | 24 | 12 | 24 | 14 | Spracklen et al. (2011) claim a much larger range than a factor of 3 [Gunnar Myhre, Norway] | Text was modified and the uncertainty range was increased accordingly |
| 7-584 | 7 | 24 | 16 | 24 | 22 | I'm thinking that it might be appropriate to include the following reference where you discuss recent progress in understanding SOA processes: Goldstein, A. H., et al. (2009), Biogenic carbon and anthropogenic pollutants combine to form a cooling haze over the southeastern United States, Proc. Natl. Acad. Sci. U. S. A., 106(22), | The issue raised by the reviewer is discussed in a broader sence in the revised text. |

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| | | | | | | 8835–8840, doi:10.1073/pnas.0904128106. [Ralph Kahn, United States of America] | |
| 7-585 | 7 | 24 | 16 | 24 | 32 | NPF is strongly influenced by the presence of exisiting surface area (aerosol). Heterogeneous proceses are energetically more favourable than homogeneous processes (NPF). See Pirjola et al., J. Aerosol Sci., 30, 1079-1094, 1999 and Cainey and Harvey, GRL, 29, 1128, 2002. This means that in most of the MBL heterogeneous processes are favoured, with homogeneous processes (NPF) occuring in the Free Troposphere (see also Quinn and Bates, 2011). This is covered a little better in the section on p25, lines 7-16. [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | We agree but there is no room for a detailed dicussion on all the factors affecting NPF. |
| 7-586 | 7 | 24 | 16 | 24 | 32 | A good piece indeed. The new particle formation from precursors is an emerging process, important to be considered and the authors' efforts are appreciated. However, its discussion is incomplete. Secondary VOCs over dense vegetation is a primary source; DMS over ocean another. However, over high-altitude mountain regions, NPF occurs quite significantly. There are recent reports from the Himalayan regions, where new particle formation occurs at altitudes as high a 4.5 km near mid-troposphere, and this has strong solar and meteorological control. Compared to those formed closer to the surface from precursor gases, these particles would have a higher chemical lifetime in the atmosphere and would be significant in modulating the cloud (including cirrus) radiative properties. The magnitudes, though yet remain quite uncertain, the processes and the uncertainties are worth mentioning here. [K KRISHNA MOORTHY, INDIA] | The text has been revised based on this and other comments. |
| 7-587 | 7 | 24 | 18 | 24 | 18 | Correct the Reference "Kulmala and Kerminen 2008" to "Kulmala and Kerminen, 2008" [Panuganti China Sattilingam Devara, India] | The format was corrected. |
| 7-588 | 7 | 24 | 18 | | | Add Leck, C., and E.K. Bigg, 2010, New particle formation of marine biological origin, Aerosol Science and Technology, 44, 570–577. [Henning Rodhe, Sweden] | there is no room to include individual studies on NPF unless they bring up something really new. |
| 7-589 | 7 | 24 | 23 | | | Should Meskhidze and Nene (Science, 314, 1419-1423, 2006) be included here for their paper on Isoprene? [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | there is no room to include individual studies on NPF unless they bring up something really new. |
| 7-590 | 7 | 24 | 25 | | | statement 'Ion-induced nucleation is very likely to contribute to NPF throughout the atmosphere' is in contradiction to page 51, l 48: The CLOUD experiment indicates that GCR-induced ionization enhances water–sulphuric acid<br>49 nucleation in the middle and upper troposphere, but is very unlikely to give a significant contribution to<br>50 nucleation taking place in the continental boundary layer. [Urs Baltensperger, Switzerland] | There is no real contradiction here. However, the text was modified to avoid confusion and misunderstanding. |
| 7-591 | 7 | 24 | 26 | 24 | 26 | Correct the Reference "Yu 2010" to "Yu, 2010" [Panuganti China Sattilingam Devara, India] | The format of the reference was corrected. |
| 7-592 | 7 | 24 | 27 | 24 | 28 | The reason why NPF does not affect much mass and direct RF should be given. I suggest "Because of the small size of newly-formed particles, …" [Nicolas Bellouin, United Kingdom] | This sentence has been removed from the revised text. |
| 7-593 | 7 | 24 | 30 | 24 | 31 | Wang and Penner (2009) represent the first such study that examine how NPF can affect global CCN and further affect the estimate of aerosol indirect forcing. (Wang, M., and Penner, J. E.: Aerosol indirect forcing in a global model with particle nucleation, Atmos. Chem. Phys., 9, 239-260, 2009.) [Minghuai Wang, United States of America] | This paper is cited in the revised text. |
| 7-594 | 7 | 24 | 31 | 24 | 32 | Pierce et al., 2011; Nucleation and condensational growth to CCN sizes during a sustained pristine biogenic SOA event in a forested mountain valley, Atmos. Chem. Phys. Discuss., 11, 28499-28544. [Warren Richard Leaitch, Canada] | The paper is a bit too narrow (single example case) to be included here. |
| 7-595 | 7 | 24 | 34 | 24 | 35 | This sentence is confusing. Aerosol sinks are better understood so there has been less progress in understanding them? [Patricia Quinn, US] | We agree. The paragraph was rewritten and this statement was removed. |
| 7-596 | 7 | 24 | 34 | 24 | 43 | It is said that aerosol sinks are better understood that sources, but also that wet deposition remains a key source of uncertainty. This is contradictory. Wet deposition is indeed a key source of uncertainty. [Twan Van Noije, Netherlands] | We agree. The paragraph was rewritten and this statement was removed. |
| 7-597 | 7 | 24 | 34 | | 35 | "Aerosol sinks are generally better understood than aerosol sources, and there has been correspondingly less progress in understanding and modeling since AR4." | this paragraph was rewritten. The lifetime issue is briefly discussed |

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| | | | | | | This is reading more like a progress report and less like an assessment. What is needed is a sense of the magnitude of uncertainty, ultimately, of forcing that results from uncertainty in various processes, here aerosol sinks.<br><br>Textor (ACP 06) presented the residence times (days) for aerosol components in the 15 models that particpated in the AEROCOM study, as follows: median (range).<br><br>Sulfate 4.1 (2.6-5.4)<br>BC 6.5 (5.3-15)<br>POM 6.2 (4.3-11)<br>Dust 4.0 (1.3-7)<br>Sea salt 0.4 (0.03-1.1)<br><br>What is required is an assessment as to the present state of the understanding (one infers not much changed from then) and whether this is good enough, if not what is required, what is the uncertainty in aerosol forcing that results from the uncertainty in residence times (removal rates) reflected in the present models. [Stephen E Schwartz, USA] | |
| 7-598 | 7 | 24 | 34 | | 44 | This whole para is just a statement of fairly well known information, instead of a quantitative assessment of what is known, what is unknown, how the uncertainties propagate into forcing or other aerosol influences on climate and climate change. [Stephen E Schwartz, USA] | The paragraph was fully revised. |
| 7-599 | 7 | 24 | 35 | 24 | 35 | depend -> depend on [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | The typo was corrected. |
| 7-600 | 7 | 24 | 35 | 24 | 35 | Insert on after depend [James Hudson, USA] | The typo was corrected. |
| 7-601 | 7 | 24 | 35 | | | Change to "…which depend on the…" [Patricia Quinn, US] | The typo was corrected. |
| 7-602 | 7 | 24 | 36 | 24 | 36 | should be "depend on the…" [Anthony Del Genio, USA] | The typo was corrected. |
| 7-603 | 7 | 24 | 37 | 24 | 37 | Correct the Reference "Feng 2008" to "Feng, 2008" [Panuganti China Sattilingam Devara, India] | The format was corrected. |
| 7-604 | 7 | 24 | 38 | | | Change to "…material reduces the effectiveness…" [Patricia Quinn, US] | text reworded |
| 7-605 | 7 | 24 | 40 | 24 | 41 | "For insoluble … depends also strongly on…mixing with soluble species", I assume that this is only referring to the nucleation scavenging, because for impaction scavening the only thing matters is the size of particles when precipitating drops are in place. [Chien Wang, United States of America] | text clarified |
| 7-606 | 7 | 24 | 41 | 24 | 42 | The statement that wet deposition remains a key source of uncertainty in aerosol models is kind of contradictory to the first sentence of this paragraph saying that aerosol sinks are generally better understood than sources. [Johannes Schneider, Germany] | This paragraph was fully revised. |
| 7-607 | 7 | 24 | 42 | | | remainS [Warren Richard Leaitch, Canada] | The typo was corrected. |
| 7-608 | 7 | 24 | 42 | | | Change to "…remains a key source…." [Patricia Quinn, US] | The typo was corrected. |
| 7-609 | 7 | 24 | 46 | 24 | 46 | Much of section 7.3.3 reads more as a review of recent literature than assessment. A really hard thing to go back and trim references that are not essential for assessment but it will make it a better section. [Daniel Murphy, United States of America] | agreed text should be shortenend |
| 7-610 | 7 | 24 | 46 | 28 | 18 | Although this section is an improvement over the ZOD, it still provides too much detail. The IPCC reports are not review articles; they are summaries of what people think is important for climate change. Much of the complexity that is discussed in these pages has not been shown to be important to climate change at all. Perhaps some subset of these things will be found to be important in the future, and at that time it will be appropriate to include them in an IPCC report. But unless the authors can point to published estimates of the CLIMATE importance of the things they are talking about, they should stick to the things that we already know we need to do better on to shrink the error bar in direct or indirect forcing. I will provide a few specific | agreed text should be shortenend |

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| | | | | | | examples below, but the comment applies to the section in general. Also, see my upcoming comment on Figure 7.8. [Anthony Del Genio, USA] | |
| 7-611 | 7 | 24 | 46 | 28 | 36 | I'm surprised that section 7.3.3 does not include a section on progress and gaps in understanding aerosol optical properties [JOHN OGREN, USA] | consider with above |
| 7-612 | 7 | 24 | 46 | | | This section should be shortened quite a bit. It is too much detail relative to the treatment of clouds, and focuses on processes more detailed than most cliamate models. [Andrew Gettelman, USA] | agreed text should be shortenend |
| 7-613 | 7 | 24 | 46 | | | Section 7.3.3: Here I would like to see an assessment of the refractive indices reported in the literature for the various aerosol types, including their wavelength dependences. [Twan Van Noije, Netherlands] | consider with above |
| 7-614 | 7 | 24 | 51 | 24 | 51 | "cloud and ice nuclei" is not well worded. Better: "CCN and IN", since these acronyms have already been defined and used. [Jón Egill Kristjánsson, Norway] | agreed, text reworded |
| 7-615 | 7 | 24 | | | | Section 7.3.3: Should include a subsection on the vertical distribution of aerosols (abundance, extinction, Sd, BC etc) and the recent advances made in their understanding using air borne, balloon-borne and other profiling as well as from high altitude and remote mountain sites where measurements are possible from altitudes similar to free tropospheric conditions. These assume increased significance over regions where abundant solar radiation is available and strong aerosol layers, especially absorbing type, occur above low clouds and in regions where cirrus form. These are very recent developments, most of which have taken place since the AR4, and are from data- sparse and potentially aerosol-laden regions, and have raised issues such as "do BC build their own homes?' and 'self-lofting of BC to stratosphere'. Not including these would definitely hamper the great effort in AR5. [K KRISHNA MOORTHY, INDIA] | not in this section |
| 7-616 | 7 | 25 | 5 | 26 | 10 | BC is the only component for which transport is mentioned. All aerosols can be transported with implications for climate and health outcomes, including absorbing over reflective surfaces and snow/ice deposition. Suggest either mentioning transport for all components, or describing why it's discussed for BC. In additoin, the literature on long-range transport of BC is richer than is implied here (and is not just limited to emissions from biomass burning), and has important implications for snow and ice deposition, among other climate and health outcomes. For example, see Arctic Modeling and Assessment Programme (AMAP), The Impact of Black Carbon on Arctic Climate, 2011. [Susan Anenberg, USA] | agreed, all species should potentially be discussed |
| 7-617 | 7 | 25 | 5 | 26 | 10 | Why are sulfate and nitrate not covered in this section, as they are also climate relevant? [Susan Anenberg, USA] | agreed, all species should potentially be discussed |
| 7-618 | 7 | 25 | 7 | 25 | 16 | Losses and deposition (wet and dry) should be briefly mentioned in this section (covered in more detail p24 lines 34-44). [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | repetition should be avioded, but could refer forward? |
| 7-619 | 7 | 25 | 12 | 25 | 13 | Aerosol models are increasingly calculating the gas-aerosol partitioning by assuming thermodynamic equilibrium…" Please include Jacobson, M. Z., Studying The effects of calcium and magnesium on size-distributed nitrate and ammonium with EQUISOLV II, Atmos. Environ., 33, 3635-3649, 1999 [Mark Z. Jacobson, U.S.A.] | consider citing |
| 7-620 | 7 | 25 | 13 | 25 | 13 | "…or by solving gas-particle mass transfer dynamically." Please include Jacobson, M.Z., A solution to the problem of nonequilibrium acid/base gas-particle transfer at long time step, Aerosol Sci. Technol, 39, 92-103, 2005 [Mark Z. Jacobson, U.S.A.] | consider citing |
| 7-621 | 7 | 25 | 13 | 25 | 16 | So the equilibrium assumption is not always valid even under typical conditions. What implications does this have for the global direct radiative forcing that IPCC cares about? Is there published evidence that non-equilibrium conditions would have a first-order global radiative effect and thus that the later estimates of direct RF are greatly underestimated? If not, it doesn't belong in this document. [Anthony Del Genio, USA] | consider removing text? |
| 7-622 | 7 | 25 | 13 | 26 | 14 | "climate and global models" to "climate and global aerosol models"? [Chien Wang, United States of America] | agreed, text reworded |
| 7-623 | 7 | 25 | 13 | | 16 | What would be the consequence of the invalid assumption of "inorganic semi-volatile species being in equilibrum"? Is that important for the climate effects of aerosols? It would mainly delay the condensation of gases for a short time, or? [Michael Schulz, Norway] | consider removing text? |

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| 7-624 | 7 | 25 | 13 | | 16 | This needs a reference: Although a number of climate and global models assume thermodynamic equilibrium for semi-volatile inorganic species (or do not include semi-volatile inorganic species), the equilibrium assumption is not always valid even under typical atmospheric conditions. [Larry Thomason, United States of America] | consider removing text? |
| 7-625 | 7 | 25 | 18 | 25 | 19 | You should specify that the "combustion" producing BC is "incomplete combustion." [Borgar Aamaas, Norway] | agreed, text reworded |
| 7-626 | 7 | 25 | 18 | 25 | 25 | It would be helpful to describe both EC and BC and their differences. Or if this is done earlier in the report, reference where this comparison is made. [Susan Anenberg, USA] | could consider brief statement |
| 7-627 | 7 | 25 | 18 | 25 | 25 | A strict definition of BC is lacking, and this has serious ramifications for methods that purport to measure "BC". Definitions of BC used in emissions inventories are not the same as the definitions of BC used for observations. Many observations of "BC" are really measurements of light absorption. The discussion referred to in this comment appears to suggest that BC is defined as what an SP2 measures. Without a strict definition, "BC" is just a qualitative term that is not useful in a quantitative context. Clarification of the definitions of "BC" used in emissions inventories, modelling studies, and observational studies is required. [JOHN OGREN, USA] | we should try to claify this |
| 7-628 | 7 | 25 | 18 | | 19 | Fuels' gives the impressive of limiting the source to fossil fuels and similar (and not wood, grass lands, etc.). Clarify this usage to eliminate any misinterpretation. [Larry Thomason, United States of America] | agreed text clairifed |
| 7-629 | 7 | 25 | 20 | 25 | 22 | This sentence is an example of the level of detail that is not appropriate in an IPCC report. It should be deleted. [Anthony Del Genio, USA] | consider deleting |
| 7-630 | 7 | 25 | 25 | | | This report says the SP2 enables accurate measurements of BC cores of 50-80 nm diameter, but the lower limit of the SP2 is 150 nm, and values below 150 nm are obtained by assuming a lognormal distribution for the BC particles [according to Schwarz et al., 2006]. Perhaps the instrument has been improved and there is a new lower limit, but a citation indicating this improvement needs to be included in the report. [Gregory Schuster, USA] | too much detail |
| 7-631 | 7 | 25 | 27 | 25 | 29 | "Condensation of gas-phase species on BC and coagulation with other particles alter the mixing state of BC ()." Please include Jacobson, M. Z., Strong radiative heating due to the mixing state of black carbon in atmospheric aerosols, Nature, 409, 695-697, 2001 [Mark Z. Jacobson, U.S.A.] | consider citing |
| 7-632 | 7 | 25 | 27 | 25 | 33 | Also recent laboratory studies have shown that coating of soot particles with inorganics or organics substances increases hygrospcopicity. (e.g., Henning, S., et al. (2010), Soluble mass, hygroscopic growth, and droplet activation of coated soot particles during LACIS Experiment in November (LExNo), J. Geophys. Res., 115, D11206, doi:10.1029/2009JD012626) [Johannes Schneider, Germany] | consider citing |
| 7-633 | 7 | 25 | 28 | | | Add Coz, E., and C. Leck, 2010, Morphology and State of Mixture of Atmospheric Soot-like Aggregates during the Winter Season over Southern Asia – a quantitative approach. Tellus B, 63 (1): 107-116. [Henning Rodhe, Sweden] | consider citing |
| 7-634 | 7 | 25 | 29 | 25 | 30 | "The resulting BC-containing particles become more hydrophilic..." The discrete size-resolved evolution of BC from externally-mixed size distribution to internally-mixed size distribution and the resulting cloud activation on each size distribution was first simulated in Jacobson, M.Z., Effects of absorption by soot inclusions within clouds and precipitation on global climate, J. Phys. Chem., 110, 6860-6873, 2006 [Mark Z. Jacobson, U.S.A.] | Agreed.reference included |
| 7-635 | 7 | 25 | 30 | 25 | 30 | In case of coated by inorganic matters, "become hygroscopic" would be more adequte than "become more hydrophilic". Also, regarding the lifetime and atmospheric loading, Kim et al. 2008 (JGR, 113, D16309) provided a much more comprehensive global model analysis with a mixing-dependent aerosol model coupled to a GCM. [Chien Wang, United States of America] | Agreed |
| 7-636 | 7 | 25 | 31 | 25 | 33 | Change to "In addition, based on modeling and laboratory studies, internal mixing can enhance…." [Patricia Quinn, US] | Agreed. Text modified |
| 7-637 | 7 | 25 | 35 | 25 | 38 | I would like to point out that such long-range transport of the BC to the Arctic have been both observed and modelled, not just "observed" as written here. You could possibly refer to some other modelling studies? What | Text related to long range transport removed and hence not relevant |

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| | | | | | | about refering to station measurements in the Arctic, which indicates long-range transport of plumes (such as from Ny-Ålesund, Barrow, Alert). [Borgar Aamaas, Norway] | |
| 7-638 | 7 | 25 | 35 | 25 | 38 | This paragraph gives a wrong impression. Long-range transport of BC is commonplace and has been observed often and for a long time (e.g., Andreae, Science, 1983, 1148-1151). Long-range transport of BC is at least as effective as for all other submicron aerosols. There is really no reason why it should be treated separately from the other constituents, especially as the BC subjected to LRT would mostly be in internal mixture with other components like OA and sulfate. [Meinrat O. Andreae, Germany] | Text related to long range transport removed and hence not relevant |
| 7-639 | 7 | 25 | 35 | 25 | 38 | Should also cite "Perturbation of the European free troposphere aerosol by North American forest fire plumes during the ICARTT-ITOP experiment in summer 2004, A. Petzold, B. Weinzierl, H. Huntrieser, A. Stohl, E. Real, J. Cozic, M. Fiebig, J. Hendricks, A. Lauer, K. Law, A. Roiger, H. Schlager, and E. Weingartner, Atmos. Chem. Phys., 7, 5105-5127, 2007" [JOHN OGREN, USA] | Text related to long range transport removed and hence not relevant |
| 7-640 | 7 | 25 | 35 | 25 | 38 | Granat et al.2010 show that BC containing particles from India can retain their hygrophobic properties even after several days of transport over the Indian Ocean. Granat, L., Engström, J.E., Praveen, S. and Rodhe, H. 2010, Light absorbing material (soot) in rainwater and in aerosol particles in the Maldives, J. Geophys. Res. 115, D16307, doi: 10.1029/2009JD013768,2010 See also: Coz, E., and C. Leck, 2010, Morphology and State of Mixture of Atmospheric Soot-like Aggregates during the Winter Season over Southern Asia – a quantitative approach. Tellus B, 63 (1): 107-116. [Henning Rodhe, Sweden] | Text related to long range transport removed and hence not relevant |
| 7-641 | 7 | 25 | 35 | 25 | 38 | Quennehen et al (2011) report from arctic airborne measurements that "pollution from North America contained only small amounts of BC mass concentrations and was mainly composed of sulphate (particularly dominating anthropogenic air mass) and organics during the summer measurement period within POLARCAT. No significant amounts of BC mass were transported to Greenland from the North American continent, at least not during the summer observation period. Hence, there was a low potential of BC deposition (dry and wet) from North American pollution plumes on the Greenland surface ice/snow and therefore, limited impact on the surface albedo" (Quennehen et al., Atmos. Chem. Phys., 11, 10947–10963, 2011) [Johannes Schneider, Germany] | Text related to long range transport removed and hence not relevant |
| 7-642 | 7 | 25 | 35 | 25 | 38 | This paragraph seems to be out of place. [Hailong Wang, USA] | Agreed. Paragraph removed |
| 7-643 | 7 | 25 | 38 | 25 | 38 | Replace "reduce" with "reduces". [Steven Ghan, USA] | Paragraph on long range transport removed and hence not relevant |
| 7-644 | 7 | 25 | 40 | 25 | 52 | It would help this disucssion if anthropogenic SOA and biogenic SOA were distinguished [Warren Richard Leaitch, Canada] | Will consider iincluding in SOD |
| 7-645 | 7 | 25 | 50 | 25 | 52 | "However, it has been found that a certain amount of POA may evaporate, followed by gas-phase photochemical oxidation of vapors, to produce even less volatile SOA (Robinson et al, 2007)." A 2005 high-resolution 3-D modeling study compared with road traffic data found that evaporation of POA causes particles to shrink, thereby increasing their coagulation rates, causing those particles to shift to larger size, eliminating much of the sub-nucleation mode. However, because of the dilution of the evaporated POA, the POA did not recondense readily. This result explains much of observed evolution of the aerosol size distribution within the first tens of minutes after emissions (Jacobson, M.Z., D.B. Kittelson, and W.F. Watts, Enhanced coagulation due to evaporation and its effect on nanoparticle evolution, Environmental Science and Technology, 39, 9486-9492, 2005) [Mark Z. Jacobson, U.S.A.] | Text related to this has been removed and hence not relevant |
| 7-646 | 7 | 25 | 52 | 25 | 52 | It is not so clear in Figure 7.7 [Patrick CHAZETTE, France] | The Figure has been redrawn abd improved in several aspects. It is not clear from the comment however what isn't clear in the figure. |
| 7-647 | 7 | 25 | 54 | 25 | 55 | I'm not sure that other sources of brown carbon such as liquid phase oligimerization can be excluded. [Daniel Murphy, United States of America] | Will consider iincluding in SOD |
| 7-648 | 7 | 25 | 54 | 25 | 57 | "Some of OA is light aborbing…The absorption properties of BrC can be attributed to water soluble organic compounds and humic like substances…" Please clarify that light-absorbing BrC also contains "nitrated aromatics, benzaldehydes, benzoic acids, aromatic polycarboxylic acids, phenols, and polyciclic aromatic | Text modified |

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| | | | | | | hydrocarbons" (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 Z. Jacobson, U.S.A.] | |
| 7-649 | 7 | 25 | 55 | 25 | 56 | Correct the References "Andreae and Gelencser 2006" and "Graber and Rudich 2006" to "Andreae and Gelencser, 2006" and "Graber and Rudich, 2006" [Panuganti China Sattilingam Devara, India] | OK. Thanks |
| 7-650 | 7 | 26 | 1 | 26 | 10 | "There is a large range in the complexity with which OA are represented in global aerosol models..." Please clarifiy that the radiative forcing due to light-absorbing weak, medium, and strongly-absorbing organic aerosols was first calculated in Jacobson, M.Z., Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001 (see abstract). [Mark Z. Jacobson, U.S.A.] | Reference included |
| 7-651 | 7 | 26 | 7 | 26 | 10 | Treatment of many things in GCMs - anything that happens on scales less than 100 km - is very crude. That's what parameterizations are by definition. The question is what level of complexity is needed to produce a reasonable estimate of the regional and global climate effects of these processes. What published estimates can you cite that document the climate effects of SOAs, aging processes, etc.? That's the issue for IPCC. In this case I think there are things out there that can be cited, maybe one of Nenes' papers, for example. [Anthony Del Genio, USA] | Text modified and reference included |
| 7-652 | 7 | 26 | 14 | 26 | 21 | Please note that most single particle measurements actually suggest the co-existance of external and internal mixtures, e.g., Hara et al., 2003; Schwarz et al., 2008; Twohy et al., 2008. [Chien Wang, United States of America] | Agreed |
| 7-653 | 7 | 26 | 14 | 26 | 33 | What this section needs is a reference to a paper that demonstrates how mixing assumptions affect the climate and/or a climate change prediction. If you are going to criticize modal approaches, then you need evidence that such approaches matter for the climate. [Anthony Del Genio, USA] | Reference included |
| 7-654 | 7 | 26 | 15 | 26 | 15 | Remove comma after "black carbon". [Steven Ghan, USA] | OK. Thanks |
| 7-655 | 7 | 26 | 15 | 26 | 16 | The sentence needs reformulation. Did it mean to say "Generally, in biomass burning aerosol, organic compounds and black carbon were frequently found to be internally mixed with…" [Johannes Schneider, Germany] | Yes, sentence revised |
| 7-656 | 7 | 26 | 17 | 26 | 17 | Correct the Reference "Pratt and Prather 2010" to "Pratt and Prather, 2010" [Panuganti China Sattilingam Devara, India] | OK. Thanks |
| 7-657 | 7 | 26 | 17 | 26 | 18 | more references possible: e.g., Dall'Osto et al., Atmos. Chem. Phys., 9, 3709–3720, 2009; or Adachi and Buseck, Atmos. Chem. Phys., 8, 6469-6481, 2008 [Johannes Schneider, Germany] | OK. Included. |
| 7-658 | 7 | 26 | 23 | 26 | 26 | A common form of mixing that is assumed by models or that is observed in the atmosphere? I think the former. [Patricia Quinn, US] | We mean, 'observed in the atmosphere' |
| 7-659 | 7 | 26 | 26 | | | A reference is needed for BC on dust. [Warren Richard Leaitch, Canada] | Text removed and hence not relevant |
| 7-660 | 7 | 26 | 27 | 26 | 27 | insert "absorbing" before internal mixture; mixing state doesn't matter much for pure scattering particles [Daniel Murphy, United States of America] | Agreed |
| 7-661 | 7 | 26 | 30 | 26 | 33 | Global aerosol models need to be examined also for external mixing (coating with foreign substances) state. [Panuganti China Sattilingam Devara, India] | Agreed. |
| 7-662 | 7 | 26 | 30 | 26 | 33 | Retain only this summary in section [Andrew Gettelman, USA] | Text modified |
| 7-663 | 7 | 26 | 30 | 26 | 33 | This statement might be "increasingly treating aerosols as internal and external mixtures. The climate modeling community still uses partially externally mixed and internally mixed aerosol models, in which we can use the internal/external mixing ratio as a tuning parameter to make the simple GCM aerosol model to be adjusted to what we observe. Also there is a need to have external mixtures to represent dust, sea salt and other aerosols near the source. There are a long history of internal mixture modeling other than the reference sited. [Teruyuki Nakajima, Japan] | agreed, needs rewording |
| 7-664 | 7 | 26 | 30 | 26 | 33 | Majority of global aerosol models and aerosol-climate models only include either all external or all internal | agreed, needs rewording |

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| | | | | | | mixtures, i.e., all aerosol species are aligned into the same size distribution (either sectional or mode, in latter case a few modes separated based on size) including the work of cited reference here. The so-called internal mixing effect in these models mostly only occurs in calculation of radiation, commonly by using the volume weighting of reflective index. This is not working in most case for core-shell model (e.g., Bond and Bergstrom, 2006). The two works (if not the only ones) known to include both internal and external mixtures in the model and allow the transforming between external mixture to different types of internal mixtures are Kim et al. 2008 (JGR, 113, D16309), and Bauer et al., 2008 (ACP, 8, 6003-6008), both also coupled interactively with GCM and performed climate response simulations. These are important papers representing the progress in process-based aerosol modeling. [Chien Wang, United States of America] | |
| 7-665 | 7 | 26 | 31 | 26 | 31 | The paper by Seland et al (2008) discussed throughly the effects of mixing state between BC and light-scattering aerosols, i.a. by comparing with Stier et al (2006).<br>Seland, Ø., T. Iversen, A. Kirkevåg, T. Storelvmo. (2008) Aerosol-climate interactions in the CAM-Oslo atmospheric GCM and investigation of associated basic shortcomings Tellus 60A, 459-491. DOI: 10.1111/j.1600-0870.2008.00318.x [Trond Iversen, United Kingdom of Great Britain & Northern Ireland] | agreed, needs rewording |
| 7-666 | 7 | 26 | 31 | 26 | 33 | "Commonly-used modal approaches do not allow to represent discrete variation…" Please state that discrete size-resolved models do allow such variation (e.g., Jacobson, M. Z., Strong radiative heating due to the mixing state of black carbon in atmospheric aerosols, Nature, 409, 695-697, 2001) [Mark Z. Jacobson, U.S.A.] | OK. Thanks |
| 7-667 | 7 | 26 | 31 | 26 | 33 | Change to "…do not allow for the representation of discreate variations…." [Patricia Quinn, US] | Text modified |
| 7-668 | 7 | 26 | 31 | | | Comparison of model and observations on primary versus secondary number concentrations: C.L. Reddington, K.S. Carslaw, D.V. Spracklen, M.G. Frontoso, L. Collins, J. Merikanto, A. Minikin, T. Hamburger, H. Coe, M. Kulmala, P. Aalto, H. Flentje, C. Plass-Dülmer, W. Birmili, A. Wiedensohler, B. Wehner, T. Tuch, A. Sonntag, C.D. O'Dowd, S.G. Jennings, R. Dupuy, U. Baltensperger, E. Weingartner, H.C. Hansson, P. Tunved, P. Laj, K. Sellegri, J. Boulon, J.P. Putaud, C. Gruening, E. Swietlicki, P. Roldin, J.S. Henzing, M. Moerman, N. Mihalopoulos, G. Kouvarakis, V. Zdímal, N. Zíková, A. Marinoni, P. Bonasoni, R. Duchi, Primary versus secondary contributions to particle number concentrations in the European boundary layer, Atmos. Chem. Phys. 11, 23, 12007-12036. [Urs Baltensperger, Switzerland] | Will include a representative reference |
| 7-669 | 7 | 26 | 32 | 26 | 32 | to represent -> representation of [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | Text modified |
| 7-670 | 7 | 26 | 35 | 27 | 4 | This section describes methods for determining size distribution without actually giving details on what typical distributions look like and why they're important. Could a graph on a typical size distribution be included with specific information on which sizes are climatically important? [Susan Anenberg, USA] | consider this |
| 7-671 | 7 | 26 | 35 | 27 | 4 | To shorten I suggest deleting this section. There are a number of minor problems with the way the literature on this subject is addressed. They could be fixed, but it would make the section a lot longer. Then I asked myself how essential the section is to the assessment. [Daniel Murphy, United States of America] | consider deleting |
| 7-672 | 7 | 26 | 35 | | | Throughout the section, it needs to be stated that aerosol size distribution in this case refers to the aerosol number size distribution. [Patricia Quinn, US] | maybe unnecessary\ |
| 7-673 | 7 | 26 | 37 | | | I think it would be worth to report on the following synthesizing work: Asmi, A., et al: Number size distributions and seasonality of submicron particles in Europe 2008–2009, Atmos. Chem. Phys., 11, 5505-5538, doi:10.5194/acp-11-5505-2011, [Michael Schulz, Norway] | reference included. |
| 7-674 | 7 | 26 | 38 | 26 | 38 | Replace ground-based measurements by ground-based and airborne measurments.<br><br>The reference for pollution aerosols may be Chazette P., H. Randriamiarisoa, J. Sanak, P. Couvert and C. Flamant, Optical properties of urban aerosol from airborne and ground-based in situ measurements performed during the ESQUIF program, J. Geophys. Res, Vol. 110, No. D2, D0220610.1029/2004JD004810, 2005.<br><br>The aerosol size distribution is always measured in the frame of the important campaigns dedicated to atmospheric aerosol: INDOEX, ESQUIF, ACE-ASIA, AMMA, and recently MEGAPOLI, etc. [Patrick CHAZETTE, France] | considered but not included |

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| 7-675 | 7 | 26 | 38 | 26 | 38 | Change aerosols to atmospheric particles.  Aerosol includes the medium. [James Hudson, USA] | unnecessary |
| 7-676 | 7 | 26 | 38 | 26 | 39 | I think 'ground-based' should be 'aircraft-based in situ'. It's probably not worth just citing Haywood et al, 2011. Either cite a bunch of measurement campaigns or none at all. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | consider  with above |
| 7-677 | 7 | 26 | 38 | 26 | | Add a  reference the end of the 1st sentence "estimating the spectral optical and CCN properties of aerosols (Jeong et al (2005)", or add the following "Jeong et al. (2005) showed that for the same satellite-measured radiance, the assumption of two different aerosol size distributions as used in the MODIS and GACP leds considerable (>50%) discrepancies in retrieved aerosol optical depth".   Reference: Jeong, M.-J., Z. Li, D. A. Chu, and S.-C. Tsay (2005), Quality and compatibility analyses of global aerosol products derived from the advanced very high resolution radiometer and Moderate Resolution Imaging Spectroradiometer, J. Geophys. Res., 110, D10S09, doi:10.1029/2004JD004648. [Zhanqing Li, USA] | reference included, but not in this place. |
| 7-678 | 7 | 26 | 39 | 26 | 39 | There are certainly numerous field campaigns that have measured size distributions. The citation of only one paper is not appropriate here. I suggest either to perform a literature search and name at least 10 publications, or leave this as a general statements withour any citation. [Johannes Schneider, Germany] | consider  with above |
| 7-679 | 7 | 26 | 40 | 26 | 40 | should be "only a few…" [Anthony Del Genio, USA] | OK |
| 7-680 | 7 | 26 | 40 | | | Add Dubovik et al. [2002] size distribution climatology [Gregory Schuster, USA] | other references by Dubovik included |
| 7-681 | 7 | 26 | 41 | 26 | 41 | I suggest adding Osborne and Haywood, 2005 after Heintzenberg et al (2000): Osborne, S.R., and Haywood, J.M., Aircraft observations of the physical and optical properties of major aerosol types, Atmospheric Research, 73, 173-201, 2005.  [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | section has been rewritten. |
| 7-682 | 7 | 26 | 41 | 26 | 45 | It should be mentioned here that airborne sunphotometry allows the retrieval of size distributions for individual aerosol layers which allows a very close match of the sample volume when validated against in-situ aircraft measurements. In fact a more rigorous comparison than comparing a column averaged size distribution with in-situ measurments from an aircraft that can only sample part of that column. Reference: Kuzmanoski M., M. Box, B. Schmid, G. P. Box, J. Wang, P. Russell, H. Jonsson, J. Seinfeld. Aerosol properties computed from aircraft-based observations during the ACE-Asia campaign: 1. Aerosol size distributions retrieved from optical thickness measurements. Aerosol Science and Technology, 41:202-216, 2007. [Beat Schmid, USA] | consider  with above |
| 7-683 | 7 | 26 | 41 | | | There is a lot of new information on size distributions, e.g. A. Asmi, A. Wiedensohler, P. Laj, A.M. Fjaeraa, K. Sellegri, W. Birmili, E. Weingartner, U. Baltensperger, V. Zdimal, N. Zikova, J.P. Putaud, A. Marinoni, P. Tunved, H.C. Hansson, M. Fiebig, N. Kivekäs, H. Lihavainen, E. Asmi, V. Ulevicius, P.P. Aalto, E. Swietlicki, A. Kristensson, N. Mihalopoulos, N. Kalivitis, I. Kalapov, G. Kiss, G. de Leeuw, B. Henzing, R.M. Harrison, D. Beddows, C. O'Dowd, S.G. Jennings, H. Flentje, K. Weinhold, F. Meinhardt, L. Ries, M. Kulmala, Number size distributions and seasonality of submicron particles in Europe 2008-2009, Atmos. Chem. Phys., 11, 5505-5538, 2011. [Urs Baltensperger, Switzerland] | consider  with above |
| 7-684 | 7 | 26 | 42 | 26 | 42 | "sun-photometer" will be misleading for the AERONET isntrumentation; Usually it is called as "sun-sky photometer" as stated in the original papers. [Teruyuki Nakajima, Japan] | we have sticked to the most common name. |
| 7-685 | 7 | 26 | 44 | 26 | 44 | I would add Osborne et al (2011) after Smirnov et al (2011): Osborne, S. R., A.J. Baran, B.T. Johnson, J.M. Haywood, E. Hesse, and S. Newman, Short-wave and long-wave radiative properties of Saharan dust aerosol. Quarterly Journal of the Royal Meteorological Society,  doi: 10.1002/qj.771, 137, 1149-1167, 2011. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | consider  with above |
| 7-686 | 7 | 26 | 46 | 26 | 47 | You should also discuss the vast amount of measurements of Kulmala and coworkers (Banana shape distribution evolution). [Ruprecht Jaenicke, Germany] | consider  with above |
| 7-687 | 7 | 26 | 49 | 26 | 49 | Main refrence for POLDER: Leroy M., J.L. Deuzé, F.M. Bréon, O. Hautecoeur, M. Herman, J.C. Buriez, D. Tanré, S. Bouffiés, P. Chazette, et J.L. Roujean, Retrieval of atmospheric properties and surface bidirectional reflectances over land from POLDER/ADEOS, J. Geophy. Res., 102, 17023 17037, 1997. [Patrick CHAZETTE, France] | considered but more recent reference included. |

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| 7-688 | 7 | 26 | 49 | 26 | 49 | should be "also provide…" [Anthony Del Genio, USA] | OK |
| 7-689 | 7 | 26 | 49 | 26 | 49 | provides should be singular [James Hudson, USA] | OK |
| 7-690 | 7 | 26 | 49 | 26 | 49 | The correct spelling includes the ring over the "A", i.e., Ångström [JOHN OGREN, USA] | OK |
| 7-691 | 7 | 26 | 49 | 26 | 49 | MISR should be listed. MiSR has the best chance to retrieve particle size information from sensors currently flying, and it does as well over land as over ocean. [Lorraine Remer, USA] | MISR is included now |
| 7-692 | 7 | 26 | 49 | 26 | 52 | It should be noted that satellite-derived Angstrom exponents or fine mode AOD are imperfect optical proxies for the size distribution, as they also depend on factors other than particle size (e.g. refractive index). [Nicolas Bellouin, United Kingdom] | agreed, needs clarification |
| 7-693 | 7 | 26 | 49 | 26 | 52 | Satellite-based aerosol retrievals rely on assumptions about microphysical and optical properties of aerosols. Ranges of uncertaintin aerosol size parameters due to such assumptions were investigated in details by Jeong et al., 2005 (Jeong, M.-J., Z. Li, D.A. Chu, and S.-C. Tsay (2005), Quality and compatibility analyses of global aerosol products derived from the advanced very high resolution radiometer and moderate resolution imaging spectroradiometer, J. Geophys. Res., 110, D10S09, doi:10.1029/2004JD004648.). [Myeong-Jae Jeong, Republic of Korea] | agreed, needs clarification |
| 7-694 | 7 | 26 | 49 | 26 | 52 | MODIS retrieves ANG over ocean, and *assumes* it over land, based on an AERONET climatology (Levy et al., 2010). The operational POLDER product does not constrain coarse-mode particle properties at all over land, but the measurements contain the information needed to do so (Dubovik, O., M. Herman, A. Holdak, T. Lapyonok, D. Tanré, J. L. Deuzé, F. Ducos, A. Sinyuk, and A. Lopatin, "Statistically optimized inversion algorithm for enhanced retrieval of aerosol properties from spectral multi-angle polarimetric satellite observations", Atmos. Meas. Tech., 4, 975-1018, 2011). MISR constrains ANG over land and water under favorable retrieval conditions (see: Kahn et al., JGR 2010) [Ralph Kahn, United States of America] | agreed, needs clarification |
| 7-695 | 7 | 26 | 50 | 26 | 50 | Fine mode fraction in MODIS and POLDER are defined for the model mode, not a strict cut-off at d=1.0. The mode r_eff are in the range of 0.1 to 0.25 microns, but the tails of these modes extend beyond the r=0.5 micron limit expressed here. Likewise the coarse mode from the model has a tail that extends to smaller radii than the 0.5 micron cutoff. [Lorraine Remer, USA] | agreed, needs clarification |
| 7-696 | 7 | 26 | 51 | 26 | 52 | Levy et al. (ACP, 2010) now concludes that MODIS size parameters have NO validity over land and are in the process of being removed from the data archive. Levy's statement is stronger than the one written here. [Lorraine Remer, USA] | agreed, needs clarification |
| 7-697 | 7 | 26 | 54 | 26 | 57 | This is also okay to retain [Andrew Gettelman, USA] | agreed text retained |
| 7-698 | 7 | 26 | 54 | 26 | 57 | "Measurements have been used to evaluate the ability of the new generation of global aerosol models to simulate aerosol number concentration and the aerosol size distribution." Please clarify that the first global study to simulate in time both the number and mass size distribution with discrete size and composition resolution was Jacobson, M. Z., Strong radiative heating due to the mixing state of black carbon in atmospheric aerosols, Nature, 409, 695-697, 2001. The algorithms used in that study were described in detail in Jacobson, M. Z., Analysis of aerosol interactions with numerical techniques for solving coagulation, nucleation, condensation, dissolution, and reversible chemistry among multiple size distributions, J. Geophys. Res., 107 (D19), 4366, doi:10.1029/ 2001JD002044, 2002. A comparison of a size distribution from that model with high-resolution freeway data is given in Jacobson, M.Z., D.B. Kittelson, and W.F. Watts, Enhanced coagulation due to evaporation and its effect on nanoparticle evolution, Environmental Science and Technology, 39, 9486-9492, 2005. [Mark Z. Jacobson, U.S.A.] | the chapter is not about who did what first. |
| 7-699 | 7 | 26 | 54 | 26 | 57 | It should include Kim et al. 2008 and Ekman et al., Sub-micrometer Aerosol Particles in the Upper Troposphere/Lowermost Stratosphere as Measured by CARIBIC and Modeled Using the MIT-CAM3 Global Climate Model (JGR, revision submitted). [Chien Wang, United States of America] | references incomplete |
| 7-700 | 7 | 27 | 4 | 27 | 4 | add 'primary' after inventories. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | done |

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| 7-701 | 7 | 27 | 6 | 28 | 36 | Maybe section 7.3.3.4 and 7.3.3.5 could be merged into 7.4.3 and 7.4.4, respectively. Especially 7.3.3.5 and 7.4..4 have some overlap. [Gunnar Myhre, Norway] | Agreed, Sections now deleted and partly merged into section 7.4 |
| 7-702 | 7 | 27 | 6 | | | reduce substantially: mostly just the first paragraph of this section is necessary [Andrew Gettelman, USA] | Agreed, Sections now deleted and partly merged into section 7.4 |
| 7-703 | 7 | 27 | 8 | 27 | 15 | McFiggans et al. (2006, ACP) review would be a good reference here [Daniel Murphy, United States of America] | Section now deleted |
| 7-704 | 7 | 27 | 9 | 27 | 9 | cite the review: Andreae, M. O., and Rosenfeld, D., Aerosol-cloud-precipitation interactions. Part 1. The nature and sources of cloud-active aerosols: Earth Science Reviews, 89, 13-41, 2008. [Meinrat O. Andreae, Germany] | Section now deleted |
| 7-705 | 7 | 27 | 10 | 27 | 14 | These two sentences are incorrect. Rephrase: "The variability in the size of the CCN was found to be more important as determinants of the variability of their CCN activity than the variability of their chemical composition at one continental location, as larger particles are more readily activated than smaller particles because they contain more soluble material (Dusek et al., 2006). However, the chemical composition influences the soluble fraction of the aerosol particles and compositional variability may be important in other locations." [Meinrat O. Andreae, Germany] | Section now deleted |
| 7-706 | 7 | 27 | 10 | | | Remove "some" [Henning Rodhe, Sweden] | Section now deleted |
| 7-707 | 7 | 27 | 11 | 27 | 14 | The fact that larger particles activate more readily than smaller particles was not discovered by Dusek et al. Chemical composition is most certainly important in other locations (e.g., Hudson and Da 1996; Hudson 2007). The Dusek et al. (2006) work was the special case of very polluted air with very little variability of hygroscopicity or chemistry. Even later papers by Dusek et al. show the importance of hygroscopicity/chemistry; e.g., Dusek, U., G.P. Frank, J. Curtius, F. Drewnick, J. Schneider, A. Kurten, D. Rose, M.O. Andreae, S. Borrmann, and U. Poschl, 2010: Enhanced organic mass fraction and decreased hygroscopicity of cloud condensation nuclei (CCN during new particle formation events). Geophys. Res. Lett., 37, L03804, doi: 10.29/2009GL040930. [James Hudson, USA] | Section now deleted |
| 7-708 | 7 | 27 | 13 | 27 | 14 | Change to "…the chemical composition influences the aerosol size distribution and can be an important in some locations" citing Quinn et al., Atmos. Chem. Phys., vol. 8, 1029 - 1042, 2008. [Patricia Quinn, US] | Section now deleted |
| 7-709 | 7 | 27 | 14 | 27 | 14 | Remove k, it is not usefull [Patrick CHAZETTE, France] | Section now deleted |
| 7-710 | 7 | 27 | 15 | 28 | 36 | Too much review detail all through the end of 7.3.3.4 and 7.3.3.5 [Daniel Murphy, United States of America] | Agreed, Section now deleted |
| 7-711 | 7 | 27 | 17 | 27 | 26 | Too much detail again, suggest this paragraph be eliminated. [Andrew Gettelman, USA] | Agreed, Section now deleted |
| 7-712 | 7 | 27 | 17 | 27 | 26 | There have been some studies relating kappa to the organic fraction of the aerosol particles, reporting a linear relationship: Gunthe et al., Atmos. Chem. Phys., 9, 7551–7575, 2009; Dusek et al, Geophys. Res. Lett., 37, L03804, doi:10.1029/2009GL040930; Gunthe et al., Atmos. Chem. Phys., 11, 11023–11039, 2011 [Johannes Schneider, Germany] | Section now deleted |
| 7-713 | 7 | 27 | 18 | | | Add references:<br>Gunthe, S. S., King, S. M., Rose, D., Chen, Q., Roldin, P., Farmer, D. K., Jimenez, J. L., Artaxo, P., Andreae, M. O., Martin, S. T., and Pöschl, U., Cloud condensation nuclei in pristine tropical rainforest air of Amazonia: size-resolved measurements and modeling of atmospheric aerosol composition and CCN activity: Atmos. Chem. Phys., 9, 7551–7575, 2009.<br>Gunthe, S. S., Rose, D., Su, H., Garland, R. M., Achtert, P., Nowak, A., Wiedensohler, A., Kuwata, M., Takegawa, N., Kondo, Y., Hu, M., Shao, M., Zhu, T., Andreae, M. O., and Pöschl, U., Cloud condensation nuclei (CCN) from fresh and aged air pollution in the megacity region of Beijing: Atmos. Chem. Phys., 11, 11,023–11,039, doi:10.5194/acp-11-11023-2011, 2011. [Meinrat O. Andreae, Germany] | Section now deleted |
| 7-714 | 7 | 27 | 18 | | | add to references"Chang, R.Y.-W., P.S.K. Liu, W.R. Leaitch and J.P.D. Abbatt, 2007: Comparison Between Measured and Predicted CCN Concentrations in a Semi-Rural Environment: Focus on the Organic Aerosol | Section now deleted |

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| | | | | | | Fraction, Atmos. Envrion., 41, 8172-8182." [Warren Richard Leaitch, Canada] | |
| 7-715 | 7 | 27 | 22 | | | add to references"Shantz, N.C., R.Y.-W. Chang, J.G. Slowik, J.P.D. Abbatt and W.R. Leaitch, 2010: Slower CCN growth kinetics of anthropogenic aerosol compared to biogenic aerosol observed at a rural site. Slower CCN growth kinetics of anthropogenic aerosol compared to biogenic aerosol observed at a rural site, Atmos. Chem. Phys.,10, 299-312." [Warren Richard Leaitch, Canada] | Section now deleted |
| 7-716 | 7 | 27 | 22 | | | Add Hede, T., L. T., Xin , Y. Tu , C. Leck , and H. Ågren, 2011, HULIS in nano aerosol clusters; investigations of surface tension and aggregate formation using Molecular Dynamics simulations. Atmos. Chem. Phys., 11(13): 6549-6557. [Henning Rodhe, Sweden] | Section now deleted |
| 7-717 | 7 | 27 | 23 | 27 | 23 | Replace k by hygroscopicity parameter [Patrick CHAZETTE, France] | Section now deleted |
| 7-718 | 7 | 27 | 25 | 27 | 25 | Insert Kim et al. 2011 Kim, J.H., S.S. Yum, S. Shim, S.-C. Yoon, J.G. Hudson, J. Park, and S.-J. Lee, 2011: On aerosol hygroscopicity, cloud condensation nuclei (CCN) spectra and critical supersaturation measured at two remote islands of Korea between 2006 and 2009. Atmos. Chem. Phys., 11, 12627–12645, doi:10.5194/acp-11-12627-2011. [James Hudson, USA] | Section now deleted |
| 7-719 | 7 | 27 | 28 | 7 | 31 | The statement of "generally good agreement" or "good agreement" should be supported quantitatively. [Chien Wang, United States of America] | Section now deleted |
| 7-720 | 7 | 27 | 28 | 27 | 31 | This report is supposed to be an Assessment, so it would be appropriate here to assess the sufficiency of the observational data set to test global CCN models. [JOHN OGREN, USA] | Section now deleted |
| 7-721 | 7 | 27 | 31 | | | Juranyi et al (2011)demonstrated the ability to predict CCN concentrations from size distribution and average chemical composition: Z. Jurányi, M. Gysel, E. Weingartner, N. Bukowiecki, L. Kammermann, U. Baltensperger, A 17 month climatology of the cloud condensation nuclei number concentration at the high alpine site Jungfraujoch, J. Geophys. Res., 116, D10204, doi:10.1029/2010JD015199, 2011. [Urs Baltensperger, Switzerland] | Section now deleted |
| 7-722 | 7 | 27 | 35 | 28 | 36 | Not clear why presented; How has what has been learned improved understanding of climate change: what are consequences of uncertainties? [Stephen E Schwartz, USA] | Agreed, Section now deleted |
| 7-723 | 7 | 27 | 37 | 27 | 41 | "Four heterogeneous ice nucleation modes are distinguished in the literature." Another method of drop freezing not discussed in the report is "evaporative freezing" (Section 4.10 and the abstract of Jacobson, M. Z., Development of mixed-phase clouds from multiple aerosol size distributions and the effect of the clouds on aerosol removal, J. Geophys. Res., 108 (D8), 4245, doi:10.1029/2002JD002691, 2003) [Mark Z. Jacobson, U.S.A.] | section shortened, especially too much detail about the various freezing modes and materials. |
| 7-724 | 7 | 27 | 52 | 27 | 52 | should be "significantly warmer…" [Anthony Del Genio, USA] | Corrected |
| 7-725 | 7 | 27 | 52 | | | replace "significant" with "signficantly" [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | Corrected |
| 7-726 | 7 | 27 | 54 | 27 | 54 | Insert "only" after "ice". [Steven Ghan, USA] | Done |
| 7-727 | 7 | 27 | 55 | 27 | 55 | "not important …. and …." is ambiguous. Do you mean that it is not important when both these conditions are satisfied (temperature above -30C and relative humidity below water saturation)? Or, is something else meant? It is not clear. [Jón Egill Kristjánsson, Norway] | Sentence deleted |
| 7-728 | 7 | 27 | 56 | 28 | 2 | Most likely the dust has been processed in the atmosphere, thereby acting as CCN and first become a water droplet with a soot core, which then, at lower temperatures, initiates immersion freezing. [Johannes Schneider, Germany] | Taken into account |
| 7-729 | 7 | 27 | 56 | | | THere are other studies that show no nucleation by soot over homogenous nucleation however (DeMott et al 2009): DeMott, P. J., M. D. Petters, A. J. Prenni, C. M. Carrico, S. M. Kreidenweis, J. L. Collett Jr., and H. Moosmu ̈ller (2009), Ice nucleation behavior of biomass combustion particles at cirrus temperatures, J. Geophys. Res., 114, D16205, doi:10.1029/2009JD012036. [Andrew Gettelman, USA] | Paragraph revised |

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| 7-730 | 7 | 28 | 2 | 28 | 2 | There is another hypothesis to explain the difference, and that is that the "soot" used in the laboratory tests had different ice-nucleating properties than the "BC" observed in the field experiments. [JOHN OGREN, USA] | Paragraph revised |
| 7-731 | 7 | 28 | 4 | 28 | 7 | What evidence is there that these substances, if accounted for in climate models, would be important to the climate? If there is evidence that they are an important fraction of the IN population, then cite the relevant paper that shows that. If not, it is premature to include them in an IPCC report. [Anthony Del Genio, USA] | Paragraph shortened and climate relevance emphasized. |
| 7-732 | 7 | 28 | 4 | 28 | 8 | Paragraph is not necessary [Andrew Gettelman, USA] | Agreed, paragraph now deleted |
| 7-733 | 7 | 28 | 7 | 28 | 8 | Prenni et al. report on ice nucleus counter data measured at ground level in the Amazonian rian forest. Here it is not surprising that biological particles are present. The Pratt et al. study reports on data from real mixed phase clouds, but their data set is restricted to a very short time period and a very limited number of analyzed particles. It is questionable of those results are globally significant. Another study from mixed phase clouds with a similar technique has detectioed a much smaller fraction of bioparticles (Kamphus et al., Atmos. Chem.Phys., 10, 8077-8095, 2010.) [Johannes Schneider, Germany] | Agreed, paragraph now deleted |
| 7-734 | 7 | 28 | 10 | 28 | 18 | Shorten this paragraph. Not all necessary [Andrew Gettelman, USA] | Agreed, paragraph shortened and climate relevance emphasized. |
| 7-735 | 7 | 28 | 12 | 28 | 18 | Reitz et al., Niedermeier et al, and Sullivan et al. have shown that the IN ability of mineral dust particles after treatment with sulfuric acid decreases irreversibly (Reitz et al.,Atmos. Chem. Phys., 11, 7839-7858, 2011; Niedermeier et al, Atmos. Chem. Phys., 11, 11131-11144, 2011; Sullivan et al., Atmos. Chem. Phys., 10, 11471-11487, 2010. [Johannes Schneider, Germany] | Paragraph now shortened as requested above |
| 7-736 | 7 | 28 | 15 | 28 | 18 | Sentence starting with "IN have been…" needs to be edited for grammar. [Patricia Quinn, US] | Sentence deleted |
| 7-737 | 7 | 28 | 17 | 28 | 17 | "of BC particles …" is not clear at all. What does "of" refer to? [Jón Egill Kristjánsson, Norway] | Sentence deleted |
| 7-738 | 7 | 28 | 24 | 28 | 24 | Correct the Reference "Kanji and Abbatt 2006" to "Kanji and Abbatt, 2006" [Panuganti China Sattilingam Devara, India] | will be corrected |
| 7-739 | 7 | 28 | 25 | 28 | 25 | Correct the References "Knopf and Koop 2006" and "Kulkarni and Dobbie 2010" to "Knopf and Koop, 2006" and "Kulkarni and Dobbie, 2010" [Panuganti China Sattilingam Devara, India] | will be corrected |
| 7-740 | 7 | 28 | 27 | 28 | 27 | Correct the Reference "Schaller and Fukuta 1979" to "Schaller and Fukuta, 1979" [Panuganti China Sattilingam Devara, India] | will be corrected |
| 7-741 | 7 | 28 | 38 | 29 | 27 | It has been known for many years that PM10 mass concentration is not particularly relevant to climate, because (a) CCN only represent a small fraction of the PM10 mass, and (b) the mass scattering efficiency of fine particles (and hence their direct forcing efficiency) is much greater than coarse particles. Since this is an Assessment Report, it would be appropriate here to assess the suitability of PM10 mass data to represent aerosol effects on climate. [JOHN OGREN, USA] | All the data assessed here are from particles with diameter smaller than 10 micron, which already including the information of fine aerosols. The data of sulfate, nitrate, ammonium product, most of EC and OC here are all from the fine aerosol particles. Right, the indirect RF of aerosol is mainly relevant with fine aerosol, but the direct RF is both related with fine and coarse particles. Even for the coarse mineral aerosol, CCN activation capacity of mineral aerosol will be enhanced through heterogeneous chemical reaction with polluted gases. This is a very hot topic, especially in Asian polluted areas, in aerosol indirect climate effect research. |
| 7-742 | 7 | 28 | 38 | | | "Global Aerosol Distributions" to distinguish from size distributions. [Warren Richard Leaitch, Canada] | Good suggestion. |
| 7-743 | 7 | 28 | 38 | | | Section 7.3.4: would recommend adding a figure of global aerosol distribution from spaceborne or AERONET observations. The two existing figures are both from in situ measurements and can hardly provide representative global coverage on a consistent basis. Figure 7.9 cites a number of studies, but given the highly variable aerosol loading and composition, particularly in urban environment, their representativeness is questionable. For some areas, the cited studies are more than 10 years apart, during which period air quality | The global and regional trends of aerosol measurment from spaceborne or AERONET are already in Chapter 2. The in situ measurements here were really not in the same period, but what we want to do, and what we can do is always a question that need to be |

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| | | | | | | controlling measures and change in economy can strong influence measurement results. In addition, while surface in situ measurements can provide data for model validation, they not necessarily represent the whole atmospheric column, which is more directly related to the aerosol radiative forcing. [Can Li, United States of America] | balanced. Something is better than nothing. The comparisions of aerosol chemistry in different parts of the world is valuable. |
| 7-744 | 7 | 28 | 42 | | | "predictions." Careful; are these predictions, projections, post-dictions? or simply model calculations? [Stephen E Schwartz, USA] | Agreed, projections would be fine. |
| 7-745 | 7 | 28 | 46 | 28 | 47 | global and local networks mainy dedicated to air quality survey. [Patrick CHAZETTE, France] | These kind of network can also be used to validate the output of climate model. |
| 7-746 | 7 | 28 | 48 | 28 | 48 | If the in-situ data are not from long-term measurement, the term "survey" or alike should be used instead of "climatology". [Chien Wang, United States of America] | Agreed, text has been changed to "Survey". |
| 7-747 | 7 | 28 | 50 | 28 | 50 | The statement 'Mineral aerosol is the largest aerosol component ….' needs refining. I suggest 'Over land regions, mineral dust aerosol contributes the most to PM10 mass …' Alternatively this section on the mean aerosol mass contribution to PM10 (does this really mean PM10 or sub-micron aerosols?) could be replaced with something very much more generic along the lines of 'The varability in the composition of aerosols around the globe is tremendously variable (e.g. Jiminez et al., 2009, Fig 7.9) the variability being caused by the proximity to a wide variety of local sources, and the relatively short atmospheric lifetime of tropospheric aerosols.' [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | Yes, we have revised the wording about the mineral aerosol according to the suggestion. We also modified the word regarding the varibility in the composition of aerosols. |
| 7-748 | 7 | 28 | 50 | 28 | 57 | The text on lines 50-51 is very confusing, and partly contradicts with what is said on lines 52 and 57. First of all, terms like "largest aerosol component" and "aerosol fraction" are used, without specifying whether a fraction of mass or number are meant. Secondly, how can it be that "Mineral aerosol is the largest aerosol component in most areas"? Certainly not in "rural U.S. and South America" (lines 51-52). And, certainly not "at oceanic sites" (line 57). And, what is meant by "higher concentrations in Urban ..."? Higher than what? [Jón Egill Kristjánsson, Norway] | Yes, the text has been revised, especially for re-wording of mineral aerosl and aerosol mass. |
| 7-749 | 7 | 28 | 51 | 28 | 51 | This is the first time the "PM" notation is used in the chapter, and may need to be defined. [Nicolas Bellouin, United Kingdom] | We changed the word of PM to "aerosol" |
| 7-750 | 7 | 28 | 51 | 28 | 51 | Urban lower case [James Hudson, USA] | Yes, changed. |
| 7-751 | 7 | 28 | 51 | 28 | 51 | Insert for after accounting [James Hudson, USA] | Changed |
| 7-752 | 7 | 28 | 51 | 28 | 52 | "Aerosol fraction in rural U.S. and South America are composed mainly (i.e., ~20%) of OC…" is incorrect and poorly worded. Rephrase: "In the rural U.S. and South America, OC contributes the largest fraction to the atmospheric aerosol (i.e., ~20%), while in other areas of the world the OC fraction ranks second or third with a mean of ~16%." [Meinrat O. Andreae, Germany] | Accepted for this wording |
| 7-753 | 7 | 28 | 51 | | | "PM10". Not clear why the focus on PM10; certainly not important for indirect effects, and a small contributor (usually) to DRE and the more so for secular forcing; PM10 is dominated by large particles; influenced by dust, sea spray. Not very climate relevant. Suggest replace table with one that has a lower cut-off, 2.5 µm or better even 1 µm diameter. [Stephen E Schwartz, USA] | Again. All the data cited in the figure are from particles with diameter smaller than 10 micron, which including the information of fine aerosols. |
| 7-754 | 7 | 28 | 57 | 29 | 2 | There is also the paper by Dentener et al. called "Nitrogen and sulfur deposition on regional and global scales: A multimodel evaluation" (Global Biogeochemical Cycles, 20, GB4003, 2006). [Twan Van Noije, Netherlands] | Here is the in-situ measurement comparision, not for modelling results, and also for just aerosol particle, not for specific compoent, like nitrogen and sulfur. |
| 7-755 | 7 | 28 | 57 | | | Sea salt is dominant in terms of both mass and in controlling light scattering over ocean regions not heavily impacted by continental aerosol sources (Quinn and Coffman, J. Geophys. Res., vol. 104, 21327 - 21342, 1999). [Patricia Quinn, US] | We have added a plot for sea slat comparision. |
| 7-756 | 7 | 29 | 4 | 7 | 23 | This section combines two quite different lines of argument. The first three reasons given for positive cloud feedbacks are supported by multiple lines of evidence, e.g. simple theories, observations, detailed process modeling, and climate model simulations. But the argument for positive low cloud feedbacks starting on line 13 is, essentially, "all the models do it." The tenuousness of this reasoning is noted on lines 15 and 18 but this | Agreed, the revised text will address these concerns. |

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| | | | | | | uncertainty is not reflected in the assertions that “a positive overall cloud feedback is more likely.” That robust negative feedbacks have not yet been identified is certainly not evidence that none exist. These summary statements are bold; they should be unambiguously support by the evidence presented in the chapter. [Robert Pincus, USA] | |
| 7-757 | 7 | 29 | 5 | 29 | 5 | About Figure 7.9: i) in line 6 it is said: "… six major aerosols...". Hence, in the inner part of the figure, several types of aerosols which are represented with molecular symbols (ie, NO4,2+), need to be named with their names; ii) in the representation of the aerosol concentration, explain the difference between the rectangles and the error bars (some of the rectangles have error bars and others not); iii) in the world map, some names are incorrectly placed: US is indicated in the middle of United States and Canada, EUROPE over Europe but also over Middle East, S.E-E Asia includes Japan (please verify in this last case if the concentration data corresponds also to Japan, besides other S.E-E countries). [Rubén D Piacentini, Argentina] | i) Using molecular symbols is the commonly used way denoting aerosol species;<br>ii) It is not plot with error bar, it is box plot displaying batches of set of data. Because of insufficient data from some areas, such as in rural Africa, the upper and lower hinges (quartiles) can not be displayed.<br>iii) Agreed. We replaced US, EUROPE and S.E-E Asia in the map |
| 7-758 | 7 | 29 | 5 | 29 | 5 | The caption should say that the "annual, seasonal or monthly mean mass concentrations" are for surface measurements, not column measurements. [Lorraine Remer, USA] | Agreed, we have changed to "surface measurement. |
| 7-759 | 7 | 29 | 5 | 29 | 27 | Again, these are likely just averaged data over only one year period. [Chien Wang, United States of America] | All these data are very rare. |
| 7-760 | 7 | 29 | 8 | 29 | 8 | Correct the Reference "Malm and Schichtel 2004" to "Malm and Schichtel, 2004" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |
| 7-761 | 7 | 29 | 9 | 29 | 9 | Delete the Reference "Malm and Schichtel 2004" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |
| 7-762 | 7 | 29 | 10 | 29 | 10 | Correct the Reference "Mariani and Mello 2007" to "Mariani and Mello, 2007" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |
| 7-763 | 7 | 29 | 17 | 29 | 17 | Correct the References "Harrison 2008" and "Mkoma 2008" to "Harrison, 2008" and "Mkoma, 2008" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |
| 7-764 | 7 | 29 | 18 | | | Post AR4 period witnessed substantial enhancement of several regional aerosol networks (eg., ARFINET over India, PHOTONS over France and Africa, and several over China and Japan). These and Campaigns (such as ICARB) carried out post AR4 period over south and south-east Asia have resulted in more than 100 peer reviewed journal articles and a wealth of information on spatial gradients, spatial heterogeneity in radiative forcing and modulations by natural processes such as planetary waves. Not including this in the report, leaves a lot of important information not accounted for, and would be a deficiency of this report. [K KRISHNA MOORTHY, INDIA] | We now highlight the large number of aerosol field campaign in various regions of the world. We also summarize information on aerosol chemical composition in various regions of the world. Because of length limitation it is not possible though to provide a comprehensive description of aerosol properties. |
| 7-765 | 7 | 29 | 20 | 29 | 20 | Correct the Reference "Rastogi and Sarin 2005" to "Rastogi and Sarin, 2005" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |
| 7-766 | 7 | 29 | 21 | 29 | 21 | Correct the References "Zhang et al., 2008;Zhang et al., 2011a" as "Zhang et al., 2008; 2011a" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |
| 7-767 | 7 | 29 | 23 | 29 | 23 | Correct the Reference "Xiao and Liu 2004" as Xiao and Liu, 2004" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |
| 7-768 | 7 | 29 | 24 | 29 | 24 | Correct the Reference "Lee and Kang 2001" to "Lee and Kang, 2001" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |
| 7-769 | 7 | 29 | 25 | 29 | 25 | Correct the Reference "Khare and Baruah 2010" to "Khare and Baruah, 2010" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |
| 7-770 | 7 | 29 | 26 | 29 | 26 | Delete the repeated Reference "Rastogi and Sarin 2005" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |
| 7-771 | 7 | 29 | 27 | 29 | 27 | Correct the Reference " Wang and Shooter 2001" to " Wang and Shooter, 2001" [Panuganti China Sattilingam Devara, India] | That would be changed finally using different reference format of Endnote |

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| 7-772 | 7 | 29 | 29 | 29 | 30 | You might mention the climatological results based on AERONET, e.g., Eck TF et al (2010) Climatological aspects of the optical properties of fine/coarse mode aerosol mixtures. J Geophys Res 115:D19205. doi:10.1029/2010JD014002;, and Eck TF, Holben BN, Reid JS, Dubovik O, Smirnov A, O'Neill NT, Slutsker I, Kinne S (1999) Wavelength dependence of the optical depth of biomass burning, urban, and desert dust aerosols. J Geophys Res 104:31333–31349 [Ralph Kahn, United States of America] | we could consider this |
| 7-773 | 7 | 29 | 29 | 29 | 30 | It should be mentioned here that airborne sunphotometry has been used all over the world for 25 years to measure column AOD when flying low over the surface and to measure the vertical profiles of AOD. A possible reference: Russell P. B., J. M. Livingston, J. Redemann, B. Schmid, S. A. Ramirez, J. Eilers, R. Kahn, A. Chu, L. Remer, P. K. Quinn, M. J. Rood, W. Wang (2007). Multi-grid-cell validation of satellite aerosol property retrievals in INTEX/ITCT/ICARTT 2004, J. Geophys. Res., 112, D12S09, doi:10.1029/2006JD007606. [Beat Schmid, USA] | we could consider this |
| 7-774 | 7 | 29 | 29 | 29 | 35 | Satellite aerosol retrieval of great importance applied to MODIS and SeaWiFS sensors (i.e., retrievals over bright surfaces like deserts) is missing here. The following works should be cited: 1) Hsu N.C., S.C. Tsay, M.D. King, and J.R. Herman (2004), Aerosol properties over bright-reflecting source regions IEEE Trans. Geosci. Remote Sens., 42, 557-569.; 2) Hsu N.C., S.C. Tsay, M.D. King, and J.R. Herman (2006), Deep blue retrievals of Asian aerosol properties during ACE-Asia IEEE Trans. Geosci. Remote Sens., 44, 3180-3195, 10.1029/2005JD006549. [Myeong-Jae Jeong, Republic of Korea] | we could consider this |
| 7-775 | 7 | 29 | 29 | 29 | 37 | This paragraph can be improved. The difference between direct measurements by ground-based sunphotometers and inversion from satellite instrument observations should be clearly stated. It would also be useful to summarise the different satellite techniques: multi-spectral, multi-angle, and polarisation. [Nicolas Bellouin, United Kingdom] | we should try to claify this |
| 7-776 | 7 | 29 | 29 | 29 | 37 | The effort is only partial. On the one hand, Aeronet does not have adequate regional coverage to provide accurate regional representation, while there are several regional aerosol networks, which have contributed considerably in last decade to improve our understanding of region-specific diversities in aerosol properties and effects. Not including them in the literature and the assessment would leave a deep dent in the report [K KRISHNA MOORTHY, INDIA] | Figure 7.12 shows regional variations. Given the large aerosol variability, it is not possible to assess all measurements. |
| 7-777 | 7 | 29 | 29 | 29 | 37 | OMI is an aerosol-dedicated instrument and it should be listed here also. [Lorraine Remer, USA] | we do not mean to include all instruments |
| 7-778 | 7 | 29 | 31 | 29 | 33 | This sentence has no verb. [Steven Ghan, USA] | Corrected. |
| 7-779 | 7 | 29 | 32 | 29 | 32 | It would be better to cite the updated version of Kahn et al.(2005) here, which is: Kahn, R.A., B.J. Gaitley, M.J. Garay, D.J. Diner, T. Eck, A. Smirnov, and B.N. Holben, 2010. Multiangle Imaging SpectroRadiometer global aerosol product assessment by comparison with the Aerosol Robotic Network. J. Geophys. Res. 115, D23209, doi: 10.1029/2010JD014601. [Ralph Kahn, United States of America] | agreed |
| 7-780 | 7 | 29 | 34 | 29 | 37 | Large differneces between AERONET and satellite retrievals and associated uncertainties are reviewed in detail in the following paper: Li, Z., X. Zhao, R. Kahn, M. Mishchenko, L. Remer, K.-H. Lee, M.Wang, I. Laszlo, T. Nakajima, and H. Maring, 2009, Uncertainties in satellite remote sensing of aerosols and impact on monitoring its long-term trend: a review and perspective, Ann. Geophys., 27, 1–16. [Myeong-Jae Jeong, Republic of Korea] | we could consider this |
| 7-781 | 7 | 29 | 35 | 29 | 35 | measurements singular [James Hudson, USA] | OK |
| 7-782 | 7 | 29 | 35 | | | "some skill". Not sure if this is good news, bad news. How much skill? How much skill is required? what is the measure of skill? See comment on implications of error in AOD on forcing at chapter 7, page 32, line 36. [Stephen E Schwartz, USA] | we cannot go into detail here. The point is that there are still large differences in climatologies. |
| 7-783 | 7 | 29 | 37 | 29 | | Add "Li et al. 2009;" before Kokhanovsky et al., 2010. reference: Li, Z., X. Zhao, R. Kahn, M. Mishchenko, L. Remer, K.-H. Lee, M.Wang, I. Laszlo, T. Nakajima, and H. Maring, 2009, Uncertainties in satellite remote sensing of aerosols and impact on monitoring its long-term trend: a review and perspective, Ann. Geophys., 27, 1–16. [Zhanqing Li, USA] | agreed, needs clarification |

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| 7-784 | 7 | 29 | 39 | 29 | 39 | The vertical profile is important. The authors might like to consider inclusion of the DABEX (Dust and Biomass Burning Experiment) (Johnson et al., 2008): Johnson, B. T., Heese, B., McFarlane, S. A., Chazette, P., Jones, A. & Bellouin, N. (2008). Vertical distribution and radiative effects of mineral dust and biomass burning aerosol over West Africa during DABEX. Journal of Geophysical Research, 113, D00C12. This study showed the layering structure of biomass burning aerosol and mineral dust over W Africa - the biomass burning aerosol overlying the mineral dust. The state-of-the-science GCMs appear able to reasonably represent this layering. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | need to consider this |
| 7-785 | 7 | 29 | 39 | 29 | 49 | There have been several aircraft and high altitude balloon campaigns carried over India during post AR4 period. However, there is no mention about this. [K KRISHNA MOORTHY, INDIA] | consider with 7-784 |
| 7-786 | 7 | 29 | 40 | 29 | | Add", EAST-AIRE (Li et al. 2007a, Dickerson et al. 2007)" before "ARCTAS". References: Dickerson, R. R., C. Li, Z. Li, L. T. Marufu, J. W. Stehr, B. McClure, N. Krotkov, H. Chen, P. Wang, X. Xia, X. Ban, F. Gong, J. Yuan, and J. Yang (2007), Aircraft observations of dust and pollutants over northeast China: Insight into the meteorological mechanisms of transport, J. Geophys. Res., 112, D24S90, doi:10.1029/2007JD008999 [Zhanqing Li, USA] | we cannot add field campaigns here. |
| 7-787 | 7 | 29 | 41 | 29 | 41 | The first work to retrieved aerosol optical properties (Saharan dusts) from spaceborne lidar (LITE mission) is in Berthier, S., Chazette, P., Couvert, P., Pelon, J., Dulac, F., Thieuleux, F., Moulin, C., and Pain T., (2006), Desert dust aerosol columnar properties over ocean and continental Africa from Lidar in-Space Technology Experiment (LITE) and Meteosat synergy, J. Geophys. Res., 111, D21202, doi:10.1029/2005JD006999. It has to be added. [Patrick CHAZETTE, France] | we cannot discuss all instruments. |
| 7-788 | 7 | 29 | 42 | 29 | 47 | Koch et al. [2009] also showed that the modeled column BC exceeds my column BC retrieval [Schuster et al., 2005], which should be of interest. [Gregory Schuster, USA] | noted |
| 7-789 | 7 | 29 | 45 | 29 | 47 | AERONET retrievals of AAOD are uncertain, and are biased towards polluted regions (AOD must be high to get a Level 2 retrieval). Please provide an Assessment of the utility of AERONET AAOD retrievals to test global aerosol models. [JOHN OGREN, USA] | uncertainty on AAOD is mentioned |
| 7-790 | 7 | 29 | 45 | 29 | 49 | "Koch et al. (2009b) also used AERONET retrievals of AAOD to show that most AeroCom models underestimate AAOD in many regions." Jacobson, M.Z., Investigating cloud absorption effects: Global absorption properties of black carbon, tar balls, and soil dust in clouds and aerosols, J. Geophys. Res., doi:10.1029/2011JD017218, in press, 2012, http://www.agu.org/pubs/crossref/pip/2011JD017218.shtml found that GATOR-GCMOM predicted AAODs between those of AERONET and OMI when compared at all AERONET stations where OMI data were available. That paper also shows that GATOR-GCMOM vertical profiles of BC match HIPPO data well. [Mark Z. Jacobson, U.S.A.] | noted, but we cannot discuss individual models. AeroCom II models used for illustration. |
| 7-791 | 7 | 29 | 52 | 29 | 54 | About Figure 7.10: i) indicate after the word "measured" the corresponding colour (red) and after the words "aerosol models" (black). Please check because the red curves have error bars which are normally used in relation to measured values; ii) indicate what the grey band means and with which of the curves it is associated (it seems to be related to the black curve). [Rubén D Piacentini, Argentina] | figure has been modified to show individual models. |
| 7-792 | 7 | 29 | 56 | 29 | 59 | The following paper also provides a detailed comparision of aerosol measurements with model output and should be added to the list of examples: Knote, C., Brunner, D., Vogel, H., Allan, J., Asmi, A., Äijälä, M., Carbone, S., van der Gon, H. D., Jimenez, J. L., Kiendler-Scharr, A., Mohr, C., Poulain, L., Prévôt, A. S. H., Swietlicki, E., and Vogel, B.: Towards an online-coupled chemistry-climate model: evaluation of trace gases and aerosols in COSMO-ART, Geosci. Model Dev., 4, 1077-1102, doi:10.5194/gmd-4-1077-2011 [Andrew Ferrone, Germany] | paper considered. |
| 7-793 | 7 | 29 | 57 | 29 | 57 | Also cite Koffi et al. (2011) here. [Steven Ghan, USA] | paper cited. |
| 7-794 | 7 | 29 | 58 | 29 | 58 | I think it worthes mentioning the new Modal Aerosol Module (MAM) (Liu et al., 2011) developed for the NCAR CAM5, whcih is used in IPCC AR5. In the paper there are extensive evaluations of model simulations with measurements. Liu, X., R. C. Easter, S. J. Ghan, R. Zaveri, P. Rasch, X. Shi, J.-F. Lamarque, A. Gettelman, H. Morrison, F. Vitt, A. Conley, S. Park, R. Neale, C. Hannay, A. Ekman, P. Hess, N. Mahowald, W. Collins, M. Iacono, C. Bretherton, and M. Flanner, Toward a Minimal Representation of Aerosol Direct and Indirect | paper cited. |

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| | | | | | | Effects: Model Description and Evaluation. Geoscientific Model Development Discussion, 4, 3485-3598, doi:10.5194/gmdd-4-3485-2011, 2011. [Xiaohong Liu, United States of America] | |
| 7-795 | 7 | 29 | 59 | 29 | 59 | Models will always be imperfect, however I think validation studies show that they are fit for the purpose of studying the aerosol impacts on climate. [Nicolas Bellouin, United Kingdom] | need to consider this and clarify text |
| 7-796 | 7 | 29 | | | | Fig. 7.9: I suggest to change the color scheme as follows: SO4 (red), NO3 (blue), NH4 (yellow), OC (green) and EC (grey) [Johannes Schneider, Germany] | That would be new but too much work and need different software to realize |
| 7-797 | 7 | 29 | | | | Fig 7.10: need to describe which pro le is measured, which is modeled, and what the grey areas mean. [Gregory Schuster, USA] | figure has been modified to show individual models. |
| 7-798 | 7 | 30 | 1 | 30 | 2 | I am not trying to pad the citations and reference list, but aerosol data assimilation is simply incomplete without mentioning Zhang et al. (2008). This is a land mark paper and major scientific accomplishment. J.Zhang,and J. S. Reid, D. Westphal, N. Baker, and E. Hyer, A System for Operational Aerosol Optical Depth Data Assimilation over Global Oceans, J. Geophys. Res., 113, D10208, doi:10.1029/2007JD009065, 2008. [Lorraine Remer, USA] | agreed |
| 7-799 | 7 | 30 | 1 | 30 | 3 | Data assimilation studies seem only of limited relevance in a climate assessment as they can only very indirectly help to reduce model deficiencies for current climate and are of no use for future climate projections. [Andrew Ferrone, Germany] | consdier with 7-800 |
| 7-800 | 7 | 30 | 1 | 30 | 4 | There is a paper that calculates the RF using assimilated aerosol data: Yumimoto and Takemura (2011, GRL, 38, L21802, doi:10.1029/2011GL049258). [Teruyuki Nakajima, Japan] | consdier with 7-799 |
| 7-801 | 7 | 30 | 1 | | | The distributions cannot be improved, only the modeling of the distributions, or the accuracy of the modeling. [Meinrat O. Andreae, Germany] | noted. |
| 7-802 | 7 | 30 | 2 | 30 | 3 | This needs to be said in more places than just this place! This is the key unknown of the aerosol effects. We do not understand the natural sources. We probably know more about the anthropogenic sources than the natural sources. The understanding of aerosol forcing will not be complete until these two sources are assessed. This point needs to be stressed throughout the document. [James Hudson, USA] | need to clarify this point |
| 7-803 | 7 | 30 | 2 | 30 | 4 | Good comment about pre-industrial constraint. [Daniel Murphy, United States of America] | enhance this with 7-802 |
| 7-804 | 7 | 30 | 3 | 30 | 3 | continous -> continual [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | noted |
| 7-805 | 7 | 30 | 4 | | | add reference: Andreae, M. O., Aerosols before pollution: Science, 315, 50-51, 2007. [Meinrat O. Andreae, Germany] | paper considered but not included. |
| 7-806 | 7 | 30 | 6 | 34 | 44 | Very different from green house gases, the aerosols and correpondent radiation forcings have regional pattern, therefore I suggest besides putting numbers for the RFs of aerosols (direct, indirect, and by species), the known spatial pattern is something worth mention in this assessment report. [Xuemei Wang, China] | spatial pattern of AOD added to figure 7.10 |
| 7-807 | 7 | 30 | 6 | | | Section 7.3.5: The total (net) effect combining direct, semi-direct and indirect effects should be discussed somewhere in this section, hopefully by species. [Shigeki KOBAYASHI, Japan] | Such a level of details is beyond the scope of the report. Observations of optical depth are mentionned in section 7.3.4. AeroCom results are discussed in the context of direct forcing. |
| 7-808 | 7 | 30 | 6 | | | Section 7.3.5: In this section I would expect to see a detailed assessment of available observations of global aerosol optical depth and a step-by-step evaluation of optical depths, clear-sky DRE, all-sky DRE, and corresponding radiation efficiencies from the AeroCom phase-II and other global models. [Twan Van Noije, Netherlands] | a number of methods are used and getting into these details here is too technical |
| 7-809 | 7 | 30 | 10 | 30 | 10 | Please state how direct radiative forcing is calculated; namely one simulation, with two radiation calls each time stap, and differetntiate that from other types of forcings, which are calculated from two simulations. [Mark Z. Jacobson, U.S.A.] | DRE is not defined with respect to pre-industrial conditions, and is therefore not a forcing. It is the current contribution of the direct effect of all aerosols to the Earth energy budget, and in the IPCC context does not need to be discussed as much as the DARF. |

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| | | | | | | | Text clarified. |
| 7-810 | 7 | 30 | 10 | 30 | 11 | DRE looks defined as the radiative forcing for total aerosols including natural aerosols. There has been a long history about the definition of the radiative forcing, but the definition of DRE looks more ambuious than that of ARF. It might be better to say that DRE is DARF for total aerosols. [Teruyuki Nakajima, Japan] | it is ok to average over day and night in terms of radiative effects. |
| 7-811 | 7 | 30 | 10 | 37 | 14 | These figures ignore the very different role of aerosols between day and night, which cannot rationally be averaged into a single figure and ought to be measured separately [VINCENT GRAY, NEW ZEALAND] | Typo corrected |
| 7-812 | 7 | 30 | 13 | 30 | 14 | Correct the Reference "Satheesh and Moorthy 2005" to "Satheesh and Moorthy, 2005" [Panuganti China Sattilingam Devara, India] | The surface albedo dependence is illustrated in the figure and already mentioned in the text |
| 7-813 | 7 | 30 | 14 | 30 | 26 | Accurate determination of albedo of the underlying surface and location of aerosols in the cloudy environment are essential for better estimations of Direct Radiative Effect (DRE). Sensitivity of other parameters under different environments to the evaluation of DRE needs to be known. [Panuganti China Sattilingam Devara, India] | Caveat added |
| 7-814 | 7 | 30 | 18 | 30 | 12 | Important to state 'In cloud-free regions …' at the beginning of the sentence. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | agreed, text reworked as indicated but reference not used |
| 7-815 | 7 | 30 | 21 | 30 | 44 | The statement "DRE is largest in cloud free-conditions" is not correct. Be careful about using 'largest' - 'strongest' is sometimes easier to reconcile as largest means most positive. I would recommend changing the order of things around a little here: TOA Clear Sky DRE and what impacts it (ssa, asymmetry parameter, underlying surface), TOA Cloud-sky DRE and what impacts it - i.e. the cloud acts as an effective high surface reflectance thereby leading to positive forcing areas, Surface DRE, LW DRE, then measurement-based estimates of the DRE would seem a more logical structure. For the LW DRE, I would inlcude reference to Haywood et al., (2005): Haywood, J.M, Allan, R.P., Culverwell I., Slingo, A., Milton, S., Edwards. J.M., and Clerbaux, N., Can desert dust explain the outgoing longwave radiation anomaly over the Sahara during July 2003? J. Geophys. Res., 110, D05105, doi:10.1029/2004JD005232, 2005. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | agreed, text reworked |
| 7-816 | 7 | 30 | 22 | 30 | 22 | "above clouds" is correct, but surely the same applies above highly reflective surfaces, e.g., snow and ice. [Jón Egill Kristjánsson, Norway] | Surface DRE can be positive in marginal cases (low Sun elevation at high latitudes). However, agreed that level of detail is too great for the report. 'almost always' removed. |
| 7-817 | 7 | 30 | 23 | 30 | 23 | For an increase in aerosol amount the radiative effect for the direct aerosol effect will always be negative at the surface. 'almost' can therefore be deleted [Gunnar Myhre, Norway] | older citations not used here |
| 7-818 | 7 | 30 | 23 | | | many others determined that DRE can become positive above clouds, beginning with Chylek and Coakley [1974]. [Gregory Schuster, USA] | Figure was looked at. Nitrate and OM effects are much smaller, text reworded but statement generally stands. Reference used for stratospheric aerosols. |
| 7-819 | 7 | 30 | 24 | 30 | 26 | "In the longwave spectrum, the DRE is only significant for desert dust and sea salt." Please correct this statement. The longwave DRE is important for many more consttituents, including nitrate and organic matter, both of which are often present in coarse particles, as shown in Figure 4 of Jacobson, M.Z., Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001. [Mark Z. Jacobson, U.S.A.] | agreed, wording used |
| 7-820 | 7 | 30 | 24 | 30 | 26 | Might reword: "… but is only significant for coarse-mode aerosol such as desert dust (Reddy et al., 2005)." Although sea salt is generally coarse-mode dominated, it is usually concentrated very close to the surface, and has a small IR impact. The suggested revision does not preclude a sea salt impact, but does not emphasize it, I think appropriately. [Ralph Kahn, United States of America] | Text has been reworked but this is too much detail |
| 7-821 | 7 | 30 | 24 | 30 | 26 | Please explain the importance of the longwave versus the shortwave DRE for the various aerosol types in more detail. [Twan Van Noije, Netherlands] | Agreed text reworded and Jacobson cited |
| 7-822 | 7 | 30 | 25 | 30 | 26 | longwave is also important for sulfate in the stratosphere [Daniel Murphy, United States of America] | local study is not needed here - we are making generalised statements |

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| 7-823 | 7 | 30 | 26 | 30 | 26 | Add the following at the end of the paragraph. “The dust aerosol direct radiative forcing over the Taklimakan Desert in Northwestern China are 44.4, −41.9,and 86.3Wm−2, respectively, at the top of the atmosphere (TOA), surface, and in the atmosphere (Huang et al., 2009).” Reference: Huang J., Q. Fu, J. Su, Q. Tang, P. Minnis, Y. Hu, Y. Yi, and Q. Zhao, 2009: Taklimakan dust aerosol radiative heating derived from CALIPSO observations using the Fu-Liou radiation model with CERES constraints, Atmos. Chem. Phys., 9, 4011–4021. [Jianping Huang, China] | plenty of examples are given already and concentration is on more modern studies |
| 7-824 | 7 | 30 | 28 | 30 | 29 | Another very direct measurement-based estimate of the DRE that deserves mentioning has been presented by Redemann, J., P. Pilewskie, P. B. Russell, J. M. Livingston, S. Howard, B. Schmid, J. Pommier, W. Gore, J. Eilers, and M. Wendisch (2006), Airborne measurements of spectral direct aerosol radiative forcing in the Intercontinental chemical Transport Experiment/Intercontinental Transport and Chemical Transformation of anthropogenic pollution, 2004, J. Geophys. Res., 111, D14210, doi:10.1029/2005JD006812. [Beat Schmid, USA] | plenty of examples are given already and cover the methods used by the proposed references. |
| 7-825 | 7 | 30 | 28 | 30 | 31 | DRE has been assessed following experimental programs as INDOEX, ACE-ASIA or AMMA. It will be very interesting to give such an information. It is also interesting to cite references using measurements (ground-based, airborne and spaceborne observations) to retrieve DRE for different aerosol species:<br><br>INDOEX: Léon J.F., P. Chazette, J. Pelon, F. Dulac et H. Ramdriamarisoa, Aerosol direct radiative impact over the INDOEX area based on passive and active remote sensing, J. Geophys. Res., 107(D19), 2002.<br><br>AMMA: Raut, J.-C. and Chazette, P., (2008), Radiative budget in the presence of multi-layered aerosol structures in the frame of AMMA SOP-0, Atmos. Chem. Phys, 8, 6839–6864. [Patrick CHAZETTE, France] | plenty of examples are given already and concentration is on more modern studies |
| 7-826 | 7 | 30 | 29 | 29 | | Add "Li et al. (2007b)" before "Yu et al. (2006)". Add reference: Li, Z., X., Xia, M. Cribb, W. Mi, B. Holben, P. Wang, H. Chen, S.-C. Tsay, T.F. Eck, F. Zhao, E.G. Dutton, R.E. Dickerson (2007b), Aerosol optical properties and their radiative effects in northern China, J. Geophys. Res., 112, D22S01, doi:10.1029/2006JD007382. [Zhanqing Li, USA] | text clarified, agreement is within measurement uncertainties |
| 7-827 | 7 | 30 | 30 | | | "good agreement". How good? how good is good enough? How does the agreement compare with the required agreement? That sort of comparison is required for an Assessment. [Stephen E Schwartz, USA] | We could not find a reference for such a global dataset of observed optical properties and concentrations, and no recent studies using such datasets. Current practice is clearly the use of satellite data. |
| 7-828 | 7 | 30 | 31 | 30 | 32 | Chapter 7, page 7-30, lines 31 and 32. The statement “ Global observational estimates of the DRE rely on satellite remote sensing of aerosol properties and/or measurements of the Earth’s radiative budget…” is incorrect because it is incomplete. There is an entirely separate set of observations of light scattering and chemical composition that yields both scattering efficiency (m2/g) and average concentration by chemical species. This, with a model, is what was used to develop the DRE calculations that I did in 1991 (Charlson et al., 1991, Tellus) and that also was reorted in early editions of the IPCC assessments. It seem important to (a) report that approach and (b) assess its uncertainties compared to those done by satellite plus model. [Robert Charlson, USA] | Very relevant reference now cited. |
| 7-829 | 7 | 30 | 31 | 30 | 33 | I tried to summarize this collection of points in a recent paper. I don't want to be self-serving here, so only if you think it adds appropriately to this discussion, you might consider also citing: Kahn, R.A., 2011. Reducing the uncertainties in direct aerosol radiative forcing. Surveys in Geophysics, doi:10.1007/s10712-011-9153-z. [Ralph Kahn, United States of America] | Reworded to "cloud-free". Use implicitly justified by difficulties with aerosol remote sensing in cloudy sky, discussed later in the section. |
| 7-830 | 7 | 30 | 33 | 30 | 33 | Somewhere it should be explained what "clear-sky" DRE means, why it's used, and its limitations. [Susan Anenberg, USA] | agreed, replaced by Kahn, 2011 |
| 7-831 | 7 | 30 | 33 | 30 | 33 | Remove the reference to Haywood et al (2011) - it's not relevant here. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | agreed, text clarified but reference not used. |
| 7-832 | 7 | 30 | 33 | | | Over the ocean downwind of the continents the clear-sky top-of-atmosphere DRE can be appreciably higher (Bates et al., 2006, Atmos. Chem. Phys., 6, 1657-1732, ) [Timothy Bates, USA] | Typo corrected |

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| 7-833 | 7 | 30 | 34 | 30 | 35 | Correct the Reference "Loeb and Manalo-Smith 2005" to "Loeb and Manalo-Smith, 2005" [Panuganti China Sattilingam Devara, India] | Clarified. |
| 7-834 | 7 | 30 | 34 | | | Clear sky aerosol DRE over global ocean "-4 -- -6 W m-2". Global annual average? specify. [Stephen E Schwartz, USA] | Reference added |
| 7-835 | 7 | 30 | 39 | 30 | 39 | The TOMS-OMI uv aerosol retrievals also make it possible to retrieve aerosol over cloud. See Section 3 of: Torres, O., A. Tanskanen, B. Veihelmann, C. Ahn, R. Braak, P. K. Bhartia, P. Veefkind, and P. Levelt (2007), Aerosols and surface UV products from Ozone Monitoring Instrument observations: An overview, J. Geophys. Res., 112, D24S47, doi:10.1029/2007JD008809. [Ralph Kahn, United States of America] | Reference added |
| 7-836 | 7 | 30 | 39 | | | A more convincing approach is described by Graaf et al. ("Retrieval of the aerosol direct radiative effect over clouds from space-borne spectrometry") in a manuscript in press for J. Geophys. Res. [Johannes Quaas, Germany] | Agreed, text clarified. |
| 7-837 | 7 | 30 | 40 | 30 | 40 | After DRE 'at TOA' should bed added [Gunnar Myhre, Norway] | older citations not used here |
| 7-838 | 7 | 30 | 41 | 30 | 41 | I would recommend adding the Keil and Haywood (2003) reference after 'stratocumulus clouds'. Keil, A., and Haywood, J.M., Solar radiative forcing by biomass aerosol particles over marine clouds during SAFARI-2000. J. Geophys. Res., 8467, 108(D13), doi:10.1029/2002JD002315, 2003. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | Focus is on observational studies, however AeroCom II numbers could be used when they are available. |
| 7-839 | 7 | 30 | 41 | | | I believe that the AeroCom RF paper Schulz et al 2006 , Fig 6, can still be used to underline the model diversity (and uncertainty) in computing the cloudy sky forcing due to BC overlaying the low stratocumulus, especially off Namibia. [Michael Schulz, Norway] | older citations not used here |
| 7-840 | 7 | 30 | 42 | 30 | 44 | An assessment of the single scattering albedo is proposed by Randriamiarisoa et al; in the frame of INDOEX:<br><br>Randriamiarisoa H., P. Chazette, and G. Mégie, The columnar retrieved single scattering albedo from NO2 photolysis rate, Tellus B, 56, 118-127, 2004. [Patrick CHAZETTE, France] | Hygroscopy added in the list of parameters key to the DRE. However, uncertainty related to RH enhancement is included in uncertainty in AOD. RH effects on single-scattering albedo are small according to Nessler et al., 2005. |
| 7-841 | 7 | 30 | 44 | | | Missing: Uncertainty on scattering enhancement at elevated relative humidity of aeroosl particles (depending on size and chemistry also adds to uncertainty of direct radiative effects: e.g., P. Zieger, R. Fierz-Schmidhauser, M. Gysel, J. Ström, S. Henne, K.E. Yttri, U. Baltensperger, E. Weingartner, Effects of relative humidity on aerosol light scattering in the Arctic, Atmos. Chem. Phys., 10, 3875-3890, 2010; and/or R. Fierz-Schmidhauser, P. Zieger, A. Vaishya, C. Monahan, J. Bialek, C.D. O'Dowd, S.G. Jennings, U. Baltensperger, E. Weingartner, Light scattering enhancement factors in the marine boundary layer (Mace Head, Ireland), J. Geophys. Res., 115, doi:10.1029/2009JD013755, 2010, and/or P. Zieger, E. Weingartner, J. Henzing, M. Moerman, G. De Leeuw, J. Mikkilä, M. Ehn, T. Petäjä, K. Clémer, M. van Roozendael, S. Yilmaz, U. Friess, H. Irie, T. Wagner, R. Shaiganfar, S. Beirle, A. Apituley, K. Wilson, U. Baltensperger, Comparison of ambient aerosol extinction coefficients obtained from in-situ, MAX-DOAS and LIDAR measurements at Cabauw, Atmos. Chem. Phys. 11, 2603-2624, 2011. [Urs Baltensperger, Switzerland] | DRE global-averaged numbers already given in the text. Uncertainty numbers given and discussed in the direct forcing section. |
| 7-842 | 7 | 30 | 44 | | | State DRE numbers here? [Andrew Gettelman, USA] | could addd numbers |
| 7-843 | 7 | 30 | 46 | 31 | 39 | The semi-direct effect needs to be distinguished from other effects of aerosols on clouds. Specifically, Cloud Absorption Effects I and II, which are defined as the effects on cloud heating of absorbing inclusions in hydrometeor particles and of absorbing aerosol particles interstitially between hydrometeor particles at their actual relative humidity (RH), respectively. These are not part of the semidirect effect (Jacobson, M.Z., Investigating cloud absorption effects: Global absorption properties of black carbon, tar balls, and soil dust in clouds and aerosols, J. Geophys. Res., doi:10.1029/2011JD017218, in press, 2012, http://www.agu.org/pubs/crossref/pip/2011JD017218.shtml). [Mark Z. Jacobson, U.S.A.] | Reference added and the role of inclusions now discussed. WHERE IS THIS DISCUSSED? |
| 7-844 | 7 | 30 | 46 | 31 | 39 | This section should mainly consist of a review paper for the aerosol semi-direct effect of Koch and Del Genio (2010). [Toshihiko Takemura, Japan] | Review paper now cited but not exclusively used. There are more recent studies as well. |

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| 7-845 | 7 | 30 | 48 | 30 | 54 | The definition of the semi-direct effect is not accurate. The semi-direct effect is the "change in cloudiness due to the decrease in near-cloud relative humidity (RH) and increase in atmospheric stability caused by absorbing aerosol particles below, within, or above a cloud [Hansen, J., M. Sato, and R. Ruedy (1997) Radiative forcing and climate response, J. Geophys. Res., 102, 6831-6864.7; Ackerman, A. S., O.B. Toon, D.E. Stevens, A.J. Heymsfield, V. Ramanathan, and E.J. Welton (2000) Reduction of tropical cloudiness by soot, Science, 288, 1042-1047; Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, J. Geophys. Res., 107, (D19), 4410, doi:10.1029/ 2001JD001376, 2002). This definition is given in Section 3.7 of Jacobson (2002) along with definitions of 11 other effects of absorbing aerosols on clouds and climate. [Mark Z. Jacobson, U.S.A.] | a number of defintions exist and text has been edited to be as clear as possible. We define the semi-direct effect as the rapid adjustment associated with aerosol-radiation interactions. |
| 7-846 | 7 | 30 | 50 | 30 | 51 | the verb “associated to” is proposed to be changed into “associated with” [Saviz Sehat Kashani, Iran, Islamic Republic of] | sentence has been rephrased. Thanks. |
| 7-847 | 7 | 30 | 51 | 30 | 52 | It is recommended in the sentence: “it can be accounted for through the concept of AF introduced in Chapters 1” the word “through” be omitted. [Saviz Sehat Kashani, Iran, Islamic Republic of] | agreed, English edited |
| 7-848 | 7 | 30 | 51 | | | Can we define the abbreviation (AF) here? [Larry Thomason, United States of America] | Defined earlier, but repeated here for claity |
| 7-849 | 7 | 30 | 52 | 31 | 1 | On a regional basis, *observations* established the importance of the semi-direct effect some time ago, most significantly based on INDOEX and SAFARI-2000 field campagin measurements. E.g., Hobbs, P. V., P. Sinha, R. J. Yokelson, T. J. Christian, D. R. Blake, S. Gao, T. W. Kirchstetter, T. Novakov, and P. Pilewskie, Evolution of gases and particles from a savanna fire in South Africa, J. Geophys. Res., 108(D13), 8485, doi:10.1029/2002JD002352, 2003; Ramanathan, V., et al. (2001), The Indian Ocean Experiment: An integrated analysis of the climate forcing and effects of the Great Indo-Asian Haze, J. Geophys. Res., 106, 28,371– 28,398. [Ralph Kahn, United States of America] | Agreed, these were cited in AR$, here we concentrate on updates. The text says "Since AR4, additional studies…" |
| 7-850 | 7 | 30 | | | | adding astronomy effect on the aerosol radiative effects and synthesis effects may help to explain the mechanism of warmer in summer and cooler in winter [Bing Qiao, China] | This comment is not clear. Varying insolation with season is accounted for in models of aerosol radiative effects. |
| 7-851 | 7 | 30 | | | | Comment to section 7.3.5 and 7.4: A Figure showing the direct and indirect effects like Fig. 2.10 of the AR4 is needed here. [Johannes Schneider, Germany] | A new figure has been introduced in Section 7.1 to show this |
| 7-852 | 7 | 31 | 2 | 31 | 5 | The following paper also highlights the complicated relationship between direct and semi-direct effects of aerosols and should be added to the list of examples: Vogel, B., Vogel, H., Bäumer, D., Bangert, M., Lundgren, K., Rinke, R., and Stanelle, T.: The comprehensive model system COSMO-ART – Radiative impact of aerosol on the state of the atmosphere on the regional scale, Atmos. Chem. Phys., 9, 8661-8680, doi:10.5194/acp-9-8661-2009, 2009 [Andrew Ferrone, Germany] | enough examples are already given and the Koch and del Genio review is cited |
| 7-853 | 7 | 31 | 5 | 31 | 5 | Add citation for Ghan et al. (2011a) here [Steven Ghan, USA] | paper now cited |
| 7-854 | 7 | 31 | 5 | 31 | 5 | "Add the following at the end of the paragraph: “Analysis of satellite observations indicated that that dust aerosols warm the clouds and increase evaporation of cloud droplets, further reducing cloud water path via the so-called semi-direct effect. Such semi-direct effects play an important role in cloud development and act to exacerbate drought conditions over semi-arid areas of northwest China (Huang et al., 2006a, b, 2010).”<br><br>References:<br>1. Huang J., B. Lin, P. Minnis, T. Wang, X. Wang, Y. Hu, Y. Yi, and J. R. Ayers, 2006a: Satellite-based assessment of possible dust aerosols semi-direct effect on cloud water path over East Asia, Geophys. Res. Lett., 33, doi: 10.1029/2006GL026561.<br>2. Huang J., P. Minnis, B. Lin, T. Wang, Y. Yi, Y. Hu, S. Sun-Mack, and K. Ayers, 2006b: Possible influences of Asian dust aerosols on cloud properties and radiative forcing observed from MODIS and CERES, Geophys. Res. Lett., 33, L06824, doi: 10.1029/2005GL024724.<br>3. Huang J., P. Minnis, H. Yan, Y. Yi, B. Chen, L. Zhang, and J. K. Ayers, 2010: Dust aerosol effect on semi-arid climate over Northwest China detected from A-Train satellite measurements, Atmos. Chem. Phys., 10, 12465-12495.<br>" [Jianping Huang, China] | it is not possible to cite every paper and we focus on new papers since AR4. |

| Comment No | Chapter | From Page | From Line | To Page | To Line | Comment | Response |
|---|---|---|---|---|---|---|---|
| 7-855 | 7 | 31 | 7 | 31 | 8 | The effect also depends on the type of cloud - it is very different for stratocumulus than for deep convection, as discussed later. [Anthony Del Genio, USA] | yes we now say that the effect is different for different types of clouds. |
| 7-856 | 7 | 31 | 7 | 31 | 17 | Absorbing aerosols can have strong effects on regional hydrological cycle (Ramanathan et al. 2005, Lau and Kim 2006, Collier and Zhang 2008), not just clouds, by heating the atmosphere and cooling the surface. [Guang Zhang, United  States of America] | These are discusssed in the precipitation section 7.6. |
| 7-857 | 7 | 31 | 7 | 31 | 24 | McFarquhar and Wang (2006: Effects of aerosols on trade wind cumuli over the Indian Ocean: model simulations. Quart. J. Roy. Meteor. Soc., 132, 821-843) is one of the very early studies that found the impact of absorbing aerosols on atmospheric stability and cloud cover that is dependent on the vertical distribution of absorbing aerosols. I think it's fair to cite this study here. [Hailong Wang, USA] | modern studies are cited here, we also cite the review paper by Koch and del Genio (2010). |
| 7-858 | 7 | 31 | 7 | | | Consider including the following reference behind "Absorbing aerosol modifies atmospheric stability": Wendisch, M., O. Hellmuth, A. Ansmann, J. Heintzenberg, R. Engelmann, D. Althausen, H. Eichler, D. Müller, M. Hu, Y. Zhang, and J. Mao, 2008: Radiative and dynamic effects of absorbing aerosol particles over the Pearl River Delta, China. Atmos. Environm., 42, 6405-6416, doi:10.1016/j.atmosenv.2008.02.033. [Manfred Wendisch, Germany] | more general studies are referenced here. This one not used. |
| 7-859 | 7 | 31 | 10 | 31 | 12 | A one-dimensional ananysis might not be appropriate here; I know this complicates the picture, but as aerosols are by no means uniform globally or even regionally in most cases, horizontal advection might also be a factor actually on a range of length scales in this case.  [Ralph Kahn, United  States of America] | text removed |
| 7-860 | 7 | 31 | 15 | 31 | 17 | Sentence not clear. [Meinrat O. Andreae, Germany] | Text reworded |
| 7-861 | 7 | 31 | 19 | 31 | 24 | Need to add the semi-direct of aborbing aerosols on deep convective clouds. Suggest to add the following statement to th end of this paragraph, "For deep convective clouds, aerosol semi-direct effect leads to weaker convection, and decreased cloud cover, cloud optical depth and precipitation, resulting from a more stable atmosphere due to enhanced surface cooling and atmospheric heating (Fan et al. 2008)". Reference: Fan, J., R. Zhang, W. Tao, and K. Mohr (2008), Effects of aerosol optical properties on deep convective clouds and radiative forcing, J. Geophys. Res., D08209, doi:10.1029/2007JD009257. [Jiwen Fan, United  States of America] | Role of aerosol on deep convection is unresolved. We do say however that the semi-direct effect is different for different types of clouds. |
| 7-862 | 7 | 31 | 19 | 31 | 24 | On the other hand, positive correlation (i.e., increasing cloud cover with increasing AOD) between cloud cover and AOD was observed from ground-based remote sensing data (e.g., Jeong and Li, 2010; Jeong, M.-J. and Z. Li (2010), Separating real and apparent effects of cloud, humidity, and dynamics on aerosol optical thickness near cloud edges, J. Geophys. Res., 115, D00K32, doi:10.1029/2009JD013547.). While artifact due to cloud contamination cannot be completetly ruled out, aerosol humidification effect and particle formation near clouds, and aerosol indirect effects contribute to such positive correlation. [Myeong-Jae Jeong, Republic of Korea] | reference was not convincing enough to imply this from correlations, so not used within this context. Text changed but citation not used |
| 7-863 | 7 | 31 | 21 | 31 | 21 | This has been previous assessed fallowing INDOEX by Ramanathan, V., Crutzen, P. J., Lelieveld, J., Althausen, D., Anderson, J., Andreae, M. O., Cantrell,W., Cass, G. and Chung, C. E. 2001. The Indian Ocean Experiment: an integrated assessment of the climate forcing and effects of the great Indo-Asian haze. J. Geophys. Res.,106, 28 371–28 398. [Patrick CHAZETTE, France] | concentratin on later references post AR4 |
| 7-864 | 7 | 31 | 29 | 31 | 31 | Sentence not clear [Meinrat O. Andreae, Germany] | text rewritten to come earlier |
| 7-865 | 7 | 31 | 30 | 31 | 30 | Need to specify what mechanisms are discussed here. I suggest "radiative mechanisms". [Nicolas Bellouin, United Kingdom] | text reworded |
| 7-866 | 7 | 31 | 30 | 31 | 30 | Replace comma with semicolon [Steven Ghan, USA] | text reworded |
| 7-867 | 7 | 31 | 31 | 31 | 33 | Might be appropriate to cite here Bill Lau's and Ramanathan's papers about aerosol impacts on the Asian monsoonal circulation, e.g., Lau K M and Kim K M 2006 Observational relationships between aerosol and Asian monsoon rainfall, and circulation Geophys. Res. Lett 33 L21810; Ramanathan V, Ramana M V, Roberts G, Kim D, Corrigan C E, Chung C E andWinker D 2007 Warming trends in Asia amplified by brow cloud solar absorption Nature 448 575–8. [Ralph Kahn, United  States of America] | citation added to ramanathan as an example. Regional circulation changes are discussed in Chapter 14. |

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| 7-868 | 7 | 31 | 31 | 31 | 33 | It could cite Wang 2004 (JGR, 109, D03106) and 2007 (GRL, 34, L05709), Chung and Seinfeld 2005 (JGR, 110, D11102), among many others published later. [Chien Wang, United States of America] | text is already clear, extra citation are not needed |
| 7-869 | 7 | 31 | 32 | 31 | 32 | "effect" should be "affect" [Anthony Del Genio, USA] | Text changed. |
| 7-870 | 7 | 31 | 32 | 31 | 32 | "effect" should be "affect" [Jón Egill Kristjánsson, Norway] | Text changed. |
| 7-871 | 7 | 31 | 32 | | | effect --> affect [Minghuai Wang, United States of America] | Text changed. |
| 7-872 | 7 | 31 | 33 | | | Liu et al 2010 use a model with indirect aerosol effects for liquid clouds and show significant semi-direct effects that reduce summer convective precipitaiton over E. Asia in a GCM, and altered heating and the circulation of the simulated S. Asian Monsoon: Liu, X, X. Xie, Z. Y. Yin, C. Liu, A. Gettelman, A modeling study of the effects of aerosols on clouds and precipitation over East Asia, Theor. Appl. Climatol., 10.1007/s00704-011-0436-6, 2011 [Andrew Gettelman, USA] | referenced here in revised text |
| 7-873 | 7 | 31 | 35 | 31 | 39 | Our knowledge on the influence of aerosols (especially absorbing fraction), through heating rates and hence large-scale circulation, on monsoon precipitation is incomplete. As this effect is mostly region-specific, its impact on monsoon parameters such as onset, break and active spells via its ISOs (Intra Seasonal Oscillations) needs to be known to enrich the present level of understanding. [Panuganti China Sattilingam Devara, India] | agreed text claified |
| 7-874 | 7 | 31 | 41 | 31 | 41 | I understand the desire to combine cloud effects and aerosol effects into one chapter, but it seems odd that aerosol forcings should come before LLGHG forcings reported in Chapter 8. I'm not sure if there's a way around this. [Susan Anenberg, USA] | chapter outline already agreed on,so this isn't changed |
| 7-875 | 7 | 31 | 41 | 33 | 23 | I still feel that the final uncertainty estimates are too small and should be ore in line with Loeb and Su. The SSA is still highly uncertain, as is partioning aerosol above and between clouds. [Lorraine Remer, USA] | Generally agree. Uncertanity estimate has been revaluated |
| 7-876 | 7 | 31 | 43 | 31 | 46 | The definitions still confuse me, and rather than refer to other chapters, it would be good to restate them here. How is AF related to iAF? [Andrew Gettelman, USA] | now defined explicitly in introduction |
| 7-877 | 7 | 31 | 44 | 31 | 44 | Here and elsewhere replace 'adjustment' with 'rapid response' [Gunnar Myhre, Norway] | rapid adjustment is now used throughout report |
| 7-878 | 7 | 31 | 45 | | | RF? Please Define [Larry Thomason, United States of America] | Defined on first use |
| 7-879 | 7 | 31 | 51 | | | This clear definition of the radiative forcing timeframe is very important. However, when the iRF/iAF is assessed, a discussion is lacking on how the published values were extrapolated to obtain a common 1750 reference- and 2010 perturbed state. [Johannes Quaas, Germany] | Extrapolation is used, this is now explicitly commented on |
| 7-880 | 7 | 32 | 2 | | | AeroCom? Please Define [Larry Thomason, United States of America] | AEROCOM defined |
| 7-881 | 7 | 32 | 3 | 32 | 3 | Are these "observationally-based" actually "remote sensing" based? [Chien Wang, United States of America] | Generally they are remote-sensed based but other methods also exist. Text modified to be more explicit |
| 7-882 | 7 | 32 | 5 | | | extra evidence (was) from observations for the total forcing "and total and fine aerosol optical depth" . The evidence was coming more from the rather precise measurement of the bulk parameters total and fine mode aerosol optical, which constrains the total amount of aerosol. In general, it seems to me that a discussion of the enhanced reliability of total direct aerosol forcing (as opposed to speciated RF eg for BC) is missing in the chapter. [Michael Schulz, Norway] | paragraph now deleted to save space. Comment on this is now added though |
| 7-883 | 7 | 32 | 8 | 32 | 16 | This paragraph is a bit confusing because the first sentence says "Observationally-based estimates of the direct RF are not completely independent of global aerosol models" but the second sentence only mentions observations. Better to say "Observationally-based estimates of the direct RF are based mostly on observations but use supplemental information from global aerosol models." Then the following sentences fall into place. [Steven Ghan, USA] | agreed, paragraph now rewritten with sentiment of comment |
| 7-884 | 7 | 32 | 8 | 32 | 16 | However, in the method applied by Chung et al. (J. Geophys. Res., 110, D24207, 2005), which was called observation based in AR4, aerosol properties are based on a combination of AERONET measurements and a | " typically" has been added to text, but this is an old study so not cited |

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| | | | | | | global model and anthropogenic change in optical properties are accounted for. [Twan Van Noije, Netherlands] | |
| 7-885 | 7 | 32 | 9 | 32 | 9 | After "Myhre, 2009)" add: "Su et al (2008) found the dust aerosol significantly reduced the cloud cooling effect at TOA by estimated using combined satellite observations and Fu-Liou model simulation. The combination of dust indirect and semi-direct shortwave radiative forcing is 78.4% of the total dust effect, but direct effect is only 21.6%. Because both first and second indirect effects enhance cloud cooling, the aerosol-induced cloud warming is mainly the result of the semi-direct effect of dust." Reference: Su J., J. Huang, Q. Fu, P. Minnis, J. Ge, and J. Bi, 2008: Estimation of Asian dust aerosol effect on cloud radiation forcing using Fu-Liou radiative model and CERES measurements, Atmos. Chem. Phys., 8, 2763-2771. [Jianping Huang, China] | this reference is not relevant here . The focus is the direct effect |
| 7-886 | 7 | 32 | 9 | 32 | 11 | In situ aerosol measurements also paly an important role here, for example, in constraining the satellite retrievals (e.g., Chen, W-T, R. Kahn, D. Nelson, K. Yau, and J. Seinfeld, 2008. Sensitivity of multi-angle imaging to optical and microphysical properties of biomass burning aerosols, J. Geophys. Res. 113, D10203, doi:10.1029/2007JD009414). They provide considerably more microphysical detail than can be derived from any remote sensing, including AERONET. For example, AERONET obtains *column-effective* aerosol indices of refraction, even though there is often more than one aerosol mode in the column, typically having very different absorption properties and SSA. [Ralph Kahn, United States of America] | this is discussed in Section 7.3.3 |
| 7-887 | 7 | 32 | 22 | 32 | 22 | The "would" implies that something must be done in order to obtain an agreement, but the text does not specify what is to be done. [Nicolas Bellouin, United Kingdom] | text modified and reduced, comment no longer relevant |
| 7-888 | 7 | 32 | 24 | 32 | 25 | Correct the References "Bellouin et al., (2005), Bellouin et al., (2008)" to "Bellouin et al. (2005; 2008)" [Panuganti China Sattilingam Devara, India] | The 2005 reference now dropped |
| 7-889 | 7 | 32 | 24 | | | The "modelling based estimates within AR4" could be referenced also at some place with Schulz et al. 2006 to facilitate the reader going back to the results used in AR4. There are other "old" papers included in the review. [Michael Schulz, Norway] | agreed, Schulz et al. now cited once |
| 7-890 | 7 | 32 | 29 | 32 | 44 | Note that the assessment of the single scattering albedo from measurements is within an absolute uncertainty of ~0.03 (e.g. Randriamiarisoa et al., 2004). [Patrick CHAZETTE, France] | We expand on optical property uncertanity now in Section 7.3.4 |
| 7-891 | 7 | 32 | 30 | 32 | 30 | Not clear whom "their" refers to. How about "the"? [Jón Egill Kristjánsson, Norway] | text now deleted |
| 7-892 | 7 | 32 | 31 | 32 | 34 | How reliable is the +/-0.2 Wm-2 uncertainty given that the change from the "refined" Bellouin et al (2008) value is 0.35 Wm-2 (from -0.65 to 0.3 Wm-2)? As described, it appears the main improvement in the Myhre (2009) study was to use a model to adjust the Bellouin et al. (2008) estimate. Seems way too optimisitc. [Norman Loeb, United States of America] | agree with sentiment of comment. Uncertainty aspects now more carefully considered and explicitly discussed |
| 7-893 | 7 | 32 | 32 | | | Aerosol forcing "-0.3 ± 0.2 W m-2". For forcing efficiency of 30 W m-2 per AOD of a scattering, accumulation-mode aerosol (24-hr avg, clear sky fraction 0.4) this would correspond to a global average incremental AOD of 0.01. Given the uncertainties in determination of AOD from satellite at low optical depth, 0.03 to 0.05, (Comment below at chapter 7, page 32, line 36), the question arises as to how satellite measurements can come up with such an estimate. .<br><br>Pursuing this further, if clear-sky DRE over global ocean ( not a forcing; lets assume dominated by sea salt) is -4 to -6 W m-2 (page 30, line 34) then a forcing efficiency (clear sky only) of 85 W m-2 diurnal avg TOA flux change per AOD would imply AOD of 0.05 ± 0.01; in fact as the radius of sea salt aerosol particles is larger, its forcing efficiency would be less, so the AOD of the non-anthro aerosol would be even greater. So the question is over the ability of the satellite measurements to determine an incremental AOD of 0.04 ± 0.01 as implied by the above against such a background marine aerosol. This would seem to require discussion and Assessment. [Stephen E Schwartz, USA] | PF, agree with sentiment of comment. Uncertainty aspects now more carefully considered and explicitly discussed. Uncertanity estimate based on combination of model and satellite-based results. Only best estimate is now used and uncertainty estiamtes are more carefully presented |
| 7-894 | 7 | 32 | 33 | 32 | 33 | "the Oslo model" is not clear. What kind of a model is it? How about "the Oslo CTM2", or something like that? [Jón Egill Kristjánsson, Norway] | text now deleted |
| 7-895 | 7 | 32 | 34 | 32 | 34 | Again, "their estimate" is unclear. Whose estimate? [Jón Egill Kristjánsson, Norway] | text now deleted |

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| 7-896 | 7 | 32 | 36 | 32 | 46 | Too much detail in this paragraph. Just state number [Andrew Gettelman, USA] | text reduced in line with comment |
| 7-897 | 7 | 32 | 36 | 32 | 46 | There also seems to be some subjective parsing of the literature. For instance, Chapter 7 states that Loeb and Su [2010] \. . . likely over-estimated the single scattering albedo uncertainty." Yet the single scattering albedo uncertainty used in Loeb and Su [2010] is based upon the accuracy analysis of Dubovik et al. [2000]. AERONET represents a state of the art aerosol retrieval technique, and the single scattering albedo uncertainty provided by AERONET is comparable to other published studies from both ground and airborne measurements [ Ostr om and Noone, 2000; Russell et al., 2002; Chowdhary et al., 2005; Waquet et al., 2009]. Hence, one can not objectively reject the Loeb and Su [2010] report on the grounds that they over-estimated the single scattering albedo, since they chose mainstream values [at a minimum, the AR5 authors should cite a journal article indicating that the single scatter albedo is known more accurately than stated in Loeb and Su, 2010]. Hence, the direct aerosol radiative forcing uncertainty of 0.5{1.0 W m2 presented in Loeb and Su [2010] should be quoted in this AR5 report, even though it is disturbingly large. [Gregory Schuster, USA] | uncertanity estimate now more carefully considered and explicitly discussed. Both model and observation studies are used to constrain uncertainty |
| 7-898 | 7 | 32 | 36 | 32 | 57 | The +/-0.3 Wm-2 value is a measure of the spread amongst models, not uncertainty. Otherwise, if +/-0.3 Wm-2 were the actual uncertainty in aerosol RF, this would imply that model-based global mean SSA is more accurate than what could be realized from AERONET observations, if AERONET accuracy were available globally. The argument against the higher uncertainty proposed by Loeb and Su 2010 is almost entirely based upon model-to-model intercomparisons. The claim that Loeb and Su (2010) over-estimated the single scattering albedo uncertainty is equivalent to stating Dubovik et al (2000) overestimated their uncertainty since that is where the single scattering albedo uncertainty in Loeb and Su (2010) came from. [Norman Loeb, United States of America] | uncertanity estimate now more carefully considered and explicitly discussed. Both model and observation studies are used to constrain uncertainty |
| 7-899 | 7 | 32 | 36 | | 46 | Uncertainty in forcing attributed to uncertainty in aerosol optical depth (0.01) attributed to Loeb and Su is 0.2 W m-2. Rather larger uncertainties are found in forcing as inferred from radiation transfer calculations together with estimated uncertainties in input parameters (McComiskey et al 2008). For uncertainty in aerosol optical depth, single scattering albedo, and asymmetry parameter of 0.01, 0.03, and 0.05, typical of current best measurement practice, uncertainty in TOA forcing was 0.6 to 1.0 W m-2 at several representative locations (24-hour average at equinox, cloud-free sky), with comparable contributions from the several uncertainties (contributions also from uncertainty in surface reflectance). For global average cloudiness of 0.6, these estimates would need to be reduced by a factor of 0.4, viz, 0.24 to 0.4 W m-2.<br><br>These estimates (based on uncertainty in AOD of 0.01) would be considerably amplified (more than a factor of 3) using estimates of uncertainty in AOD from satellite measurements. For example Remer et al 2005 state one standard deviation of MODIS optical thickness retrievals fall within the predicted uncertainty of $\Delta\tau = \pm 0.03 \pm 0.05\tau$ over ocean and $\Delta\tau = \pm 0.05 \pm 0.15\tau$ over land. For MISR about 70% to 75% of MISR AOD retrievals fall within 0.05 or 20% X AOD of the paired validation data from the Aerosol Robotic Network (AERONET) (Kahn et al 2010).<br><br>Thus it would seem that the uncertainties presented here based on Loeb and Su are seriously underestimated.<br><br>McComiskey A., Schwartz S. E., Schmid B., Guan H., Lewis E. R., Ricchiazzi P., and Ogren J. A. Direct Aerosol Forcing: Calculation from Observables and Sensitivities to Inputs. J. Geophys. Res. 113, D09202 (2008); doi:10.1029/2007JD009170.<br><br>Remer, L. A., and Coauthors, 2005: The MODIS Aerosol Algorithm, Products, and Validation. J. Atmos. Sci., 62, 947–973. doi: http://dx.doi.org/10.1175/JAS3385.1<br><br>Kahn, R. A., B. J. Gaitley, M. J. Garay, D. J. Diner, T. F. Eck, A. Smirnov, and B. N. Holben (2010), Multiangle Imaging SpectroRadiometer global aerosol product assessment by comparison with the Aerosol Robotic Network, J. Geophys. Res., 115, D23209, doi:10.1029/2010JD014601. [Stephen E Schwartz, USA] | uncertanity estimate now more carefully considered and explicitly discussed. Both model and observation studies are used to constrain uncertainty. These references are now cited in section 7.3.4 |

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| 7-900 | 7 | 32 | 36 | | 46 | Additional concern over systematic error in AOD from satellite. Retrieval of AOD from both MISR and MODIS are from radiance measurements and thus dependent on assumptions about phase function and single scattering albedo with resulltant possible error of either sign. These measurements also need to subtract surface-leaving radiance that is scene dependent, even for oceans, with issues of glint, whitecap, and biological activity (chlorophyll, coccolithophores). Issues of cloud contamination: slight cloud contribution to radiance (eg from thin cirrus) would lead to substantial overestimate; overestimate of surface leaving radiancein the subtraction would result in underestimate of aerosol. Issues of enhanced scattering from light reflected off cloud sidewalls. This is of course well known in the remote sensing community (and even to the present authors, page 38 lines 23-33), but does not seem to inform the present discussion and reliance on satellite "measurements" of AOD. It would seem that these systematic errors also need to be discussed. [Stephen E Schwartz, USA] | Uncertanity estimate has been revaluated |
| 7-901 | 7 | 32 | 44 | 32 | 45 | "Our own" should be avoided. [Chien Wang, United States of America] | text changed |
| 7-902 | 7 | 32 | 44 | 32 | 45 | "Our own..." Does this first-person use mean WG1? If not, specific papers should be cited instead. [Guang Zhang, United States of America] | text changed |
| 7-903 | 7 | 32 | 44 | 32 | 57 | This "uncertainty analysis" isjn't really a quantitative Assessment, and does not present a convincing argument of why the Loeb and Su estimate of the DARF uncertainty is too high. The range of RF values from GCMs is not nearly as convincing an estimate of uncertainties as the quantitative perturbation analysis of Loeb and Su. [JOHN OGREN, USA] | uncertanity estimate now more carefully considered and explicitly discussed. Both model and observation studies are used to constrain uncertainty |
| 7-904 | 7 | 32 | 46 | 32 | 46 | The reason why the authors think the uncertainty in absorption has been overestimated must be justified. I suggest pointing out that the +0.06 uncertainty for the single-scattering albedo over ocean translates into a huge uncertainty in the co-albedo, which is unlikely to be real for transported, aged aerosols, as suggested by aircraft observations (e.g. SAFARI campaign). [Nicolas Bellouin, United Kingdom] | agreed, uncertanity estimate now more carefully considered and explicitly discussed. Both model and observation studies are used to constrain uncertainty |
| 7-905 | 7 | 32 | 46 | 32 | 46 | I absolutely agree that the Loeb and Su uncertainty of +/-0.06 in ssa over ocean is too large. To change the ssa uncertainty from 0.03 over land to 0.06 over ocean ignores the fact that if models get the ssa right over land, then this constrains the ssa over ocean as the anthropogenic aerosols are advected from land to the oceans .... [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | uncertanity estimate now more carefully considered and explicitly discussed. Both model and observation studies are used to constrain uncertainty |
| 7-906 | 7 | 32 | 48 | 32 | 57 | Also too much detail: add to above, state number and assess. [Andrew Gettelman, USA] | agreed, text reduced and a more explicit assessment made |
| 7-907 | 7 | 32 | 51 | 32 | 52 | The models included in Figure 7.11 form only a small subset of the models participating to AeroCom phase-II (see http://aerocom.met.no/participants.html). [Twan Van Noije, Netherlands] | figure is updated to include more models |
| 7-908 | 7 | 32 | 52 | 32 | 53 | About the sentence: "Most of the models have a maximum negative radiative forcing around 20–50°N, in the region with highest aerosol concentrations". In the corresponding Figure 7.11, the value of "Model mean" for 50°N is almost the same as that of 10°N, so please change the words: "around 20-50°N" by "around 10-50°N". [Rubén D Piacentini, Argentina] | text revised with new data |
| 7-909 | 7 | 32 | 54 | 32 | 54 | Same as above to avoid "We combine our" alike. Are these direct copies from someone's paper? [Chien Wang, United States of America] | text reworded, "we" no longer used |
| 7-910 | 7 | 32 | 54 | 32 | 57 | it is not clear how the authors arrive at an uncertainty of 0.3 W m2. Is this the value derived from AeroCom Phase II, and shown in Figure 7.12? Ideally, this analysis would encompass the entire range of reasonable values for all inputs (this would require an unreasonable number of computer runs and is undoubtedly not possible with the AeroCom models, but the folks at climateprediction.net have taken a good stab at this sort of thing). All Phase II AeroCom models use the same emissions inventories for the direct forcing experiment (according to the AeroCom webpage), so what happens to the forcing uncertainty if di erent acceptable inventories are used? In particular, the BC inventories are uncertain by a factor of two, and this uncertainty is not included in the Phase II experiment. How does doubling the BC emissions a ect the BC forcing? Also the Bond and Bergstrom [2006] paper provides a range of refractive indices for black carbon { how does the BC forcing change if the full range of BC | uncertanity estimate now more carefully considered and explicitly discussed. Both model and observation studies are used to constrain uncertainty. Optical properties more carefully considered |

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| | | | | | | refractive indices are tested? These concepts may have been included in Phase II by the AeroCom team and the 0.3 W m2 quoted uncertainty, but the details are not in Chapter 7 and there is no citation of this work for a reader to examine. [Gregory Schuster, USA] | |
| 7-911 | 7 | 32 | | | | replace "to represent" with "the representation" [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | text reworded |
| 7-912 | 7 | 33 | 3 | 33 | 4 | Figure 7.11 doesn't show CAM5 direct radiative forcing. [Steven Ghan, USA] | figure updated to include CAM5 |
| 7-913 | 7 | 33 | 6 | 33 | 7 | Takemura and Uchida (2011) also specifically estimate the radiative forcing of the semi-direct effect with a GCM.<br>Takemura, T., and T. Uchida, 2011: Global climate modeling of regional changes in cloud, precipitation, and radiation budget due to the aerosol semi-direct effect of black carbon. Scientific Online Letters on the Atmosphere (SOLA), 7, 181-184, doi:10.2151/sola.2011-046. [Toshihiko Takemura, Japan] | citation added |
| 7-914 | 7 | 33 | 6 | 33 | 23 | Merge with semi-direct effect discussion above. [Andrew Gettelman, USA] | text reduced |
| 7-915 | 7 | 33 | 6 | 33 | 23 | Plase clarify that none of the studies of the semi direct radiative forcing account for either Cloud Absorption Effects I or 2. CAE 1 was accounted for in Jacobson, M.Z., Effects of absorption by soot inclusions within clouds and precipitation on global climate, J. Phys. Chem., 110, 6860-6873, 2006 [Mark Z. Jacobson, U.S.A.] | citation added |
| 7-916 | 7 | 33 | 7 | 33 | 7 | Lohman et al. (2010) did not isolate the semi-direct effect of aerosols, which were lumped with the 1st and 2nd indirect effects in the models. The only semi-direct effect it isolated was the fast CO2 effect on clouds. Consider citing Ghan et al. (2011a), which isolated the semi-direct effect of aerosols from the indirect effects, and quantified both LW and SW semi-direct effects, [Steven Ghan, USA] | agreed, citation added |
| 7-917 | 7 | 33 | 8 | 33 | 9 | "cloud response … generally increased cloud cover over ocean" is not clear. Response to what? [Jón Egill Kristjánsson, Norway] | text reworded for clarity |
| 7-918 | 7 | 33 | 13 | 33 | 16 | Lohman et al. (2010) did not isolate the semi-direct effect of aerosols, which were lumped with the 1st and 2nd indirect effects in the models. The only semi-direct effect it isolated was the fast CO2 effect on clouds. Consider discussing Ghan et al. (2011a), which isolated the semi-direct effect of aerosols from the indirect effects, and quantified both LW and SW semi-direct effects. For example: "Ghan et al. (2011a) found positivie and negative global mean longwave and shortwave semi-direct effects of aerosol that in all cases were were smaller than 0.1 W m-2." [Steven Ghan, USA] | agreed, citation added and text reworded |
| 7-919 | 7 | 33 | 13 | | | Starting here and for the rest of this paragraph, please be clear about what's included in this AF. It seems to be the semi-direct contribution to AF only, rather than all cloud responses, correct? [Drew Shindell, USA] | Yes, text clarified |
| 7-920 | 7 | 33 | 17 | 33 | 17 | Please replace "both increase and" by "either increase or". [Jón Egill Kristjánsson, Norway] | text reworded for clarity |
| 7-921 | 7 | 33 | 20 | 33 | 22 | I feel the statement that the global magnitude of the semi-direct effect is small and not significantly different than zero is not well supported by the materials presented in the paragraph. CSIRO does produces a signicant one (-0.3 W/m2). [Minghuai Wang, United States of America] | a reevaluation is made and magnitude of semi direct effect better justified |
| 7-922 | 7 | 33 | 21 | 33 | 21 | Where does the number 0.1 W/m2 come from? It seems to have been pulled out of thin air just because different cloud types in different models get effects of different sign and magnitude. The authors seem to be confusing the fact that model results are all over the place with a conclusion that therefore the effect is small. [Anthony Del Genio, USA] | a reevaluation is made and magnitude of semi direct effect better justified |
| 7-923 | 7 | 33 | 23 | 33 | 34 | Which set of models are referred to here? AeroCom phase 1 or 2? Can't you provide a citation here? [Steven Ghan, USA] | text reworded and clarified, reference added |
| 7-924 | 7 | 33 | 25 | 34 | 19 | So after all these pages about the complexity and number of different aerosol species, the reader reaches Fig. 7.12 and concludes…all that really matters for the global climate is sulfate! (And maybe BC, a little.) What evidence are you going to present to the typical IPCC reader to justify all the emphasis in the community on OC and SOA especially - which in the figure have near zero global effect with a small error bar!? Maybe you need another figure showing global maps of direct effect contributions from different species to make the case | need to consider this |

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| | | | | | | that other aerosols matter at least to certain regions, if not much in the global mean. Fig. 7.9 does not do that because (a) it is only about mass concentrations and (b) it does not isolate anthropogenic contributions. [Anthony Del Genio, USA] | |
| 7-925 | 7 | 33 | 25 | | | 7.3.5.4 Aerosol Direct Radiative Forcing by Species: There is a greater problem with constraining modeling studies to the AERONET database that is worth mentioning, though: the AERONET sampling is biased towards high optical depths for the advanced products. That is, the advanced Level 2.0 AERONET retrieval products (like AAOT) require AOT(440) greater than 0.4, which biases climatologies that are based upon these products. This is especially problematic for extrinsic aerosol properties, to the extent that larger AOT indicates greater aerosol mass in the atmospheric column. It is also problematic at many `clean' sites that only reach values of AOT(440) during the humid months of summer. This bias a ects the Myhre [2009] study as well, as he used the AERONET Level 2.0 product for the single scatter albedo and asymmetry parameter. One can avoid the AOT(440)>0.4 criteria by using Level 1.5 AERONET products, but the accuracy decreases (i.e., single-scatter albedo uncertainty increases from 0.03 to 0.06, as in Loeb and Su [2010]). Figure 2 (on Sheet 1) is a slide that I presented at AEROCOM in 2010 (Oxford), and illustrates this issue. Here, I plot a histogram of AERONET daily averaged AOD interpolated to 550 nm (grey region) alongside histograms of several AeroCom models. Only days with Level 2.0 almucantar retrievals are included for both the models and the retrievals. (Some optical depths in the grey histogram are less than 0.4 because this is not the 0.44 m wavelength and because sometimes individual almucantar retrievals occur during high AOD periods on days when the average AOD is less than 0.4). Normally, model histograms compare quite well to the AERONET optical depths measurements, but the models shown in Figure 2 do not compare to the subsetted advanced products very well. Hence, this discrepancy must be taken into account when using AERONET advanced products for comparison or assimilation with models. This is at least part of the reason why the models are biased low of the AERONET AAOD [Sato et al., 2003; Koch et al., 2009] and my column BC retrieval [Koch et al., 2009], but biased high of the SP2 pro les in Koch et al. [2009] and Schwarz et al. [2010]. This is worth mentioning in the AR5 report. Figure 2 caption: Models normally reproduce the AERONET AOD measurements reasonably well, but the sampling bias associated with the advanced AERONET products causes a shift in the AERONET AOD histogram that is not captured by the models. [Gregory Schuster, USA] | text reworded and bond et al. study referenced, along with careful caveats |
| 7-926 | 7 | 33 | 27 | 34 | 5 | In Figure 7.9 you suggest that organics are a larger component of the atmospheric fine particle aerosol, but here the direct RF is about 3 times larger by sulphate. This needs to be explained. [Warren Richard Leaitch, Canada] | text reworded to briefly explain forcing derivation |
| 7-927 | 7 | 33 | 29 | 33 | 29 | This sentence is simply wrong. How can "biomass burning or BB" represent a separate "species"? Solution: replace "species" by some other term, e.g., "aerosol types" or something like that. [Jón Egill Kristjánsson, Norway] | text reworded to aviod species |
| 7-928 | 7 | 33 | 33 | 33 | 34 | The sulfate forcing of -0.3 W/m2 is virtually the same as that from Table 7 of Jacobson, M.Z., Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001 (-0.32 W/m2). Please state this. [Mark Z. Jacobson, U.S.A.] | noted but old reference no used here |
| 7-929 | 7 | 33 | 33 | 33 | 34 | Same as above. [Chien Wang, United States of America] | text reworded, "we" no longer used |
| 7-930 | 7 | 33 | 33 | | | It would be good to add more explanation why the sulfate RF changed from AR4. This is finally one of the significant changes in the direct aerosol RF. [Michael Schulz, Norway] | should do this - do we know why? |
| 7-931 | 7 | 33 | 36 | 33 | 38 | Similarly, the authors state that the Ramanathan and Carmichael [2008] report \likely overestimate the aerosol optical depth (of BC) as their results may be contaminated by dust." The authors do not say why the Ramanathan and Carmichael [2008] report may be contaminated by dust, but presumably they are concerned about contamination of the AERONET | We notified this and modified the text. |

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| | | | | | | AAODs that are used in that study. However, the presence of dust does not a ect the BC signal very much, as we showed in Schuster et al. [2005] (paragraphs 50-51). This is because black carbon is incredibly absorbing, and dust is not very absorbing at most of the AERONET wavelengths (i.e., red to near-infrared wavelengths). This can also be deduced from Figure 1 below (on sheet 1), which shows the imaginary refractive index of black carbon [Bond and Bergstrom, 2006] and dust mixtures of illite, kaolinite, feldspar or montmorillonite with 1{3% hematite [Egan and Hilgeman, 1979; Chen and Cahan, 1981; Sokolik and Toon, 1999; Balkanski et al., 2007]. The vertical magenta lines at the bottom of the gure indicate the AERONET retrieval wavelengths of 0.44, 0.67, 0.87, and 1.02 m. Note that the black carbon refractive index is two orders of magnitude greater than the dust refractive index at 3 of the 4 AERONET wavelengths. This is why mixtures with dust hosts produce retrieved BC concentrations that di er from retrievals with water hosts by only 15% [Schuster et al., 2005]. At any rate, the huge di erence between the Ramanathan and Carmichael [2008] BC forcing value of 0.9 W m2 and the AR5 value of 0.4 W m2 has not been explained in this report, and I highly doubt that dust is the main cause of this di erence. Figure 1 caption: The imaginary refractive index for black carbon is two orders of magnitude greater than dust at most of the AERONET retrieval wavelengths. [Gregory Schuster, USA] | |
| 7-932 | 7 | 33 | 36 | 33 | 49 | The range of BC from fossil fuel starting at 0 is unphysical. This needs to be corrected,Not all model results should be included, particularly when the models are not physically based. The report ignores the most rigorous and physically based radiative forcing calculation to date, which treated interactions among 18 aerosol size distributions: Jacobson, M. Z., Strong radiative heating due to the mixing state of black carbon in atmospheric aerosols, Nature, 409, 695-697, 2001. The fossil-fuel BC portion of this is +0.25 W/m2, as provided in 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. [Mark Z. Jacobson, U.S.A.] | Bond et al. 2012 have re-evlauated and text modified |
| 7-933 | 7 | 33 | 36 | 33 | 49 | It's not clear what the justification is for scaling up the AEROCOM median forcing of +0.2 W/m2 by a factor of two to 0.4 W/m2, and no citation is given for this adjustment. AERONET observations of AAOD are biased high when applied to the global scale because the retrieval is not done unless AOD is high (i.e., polluted, dusty, or smokey regions). [JOHN OGREN, USA] | Bond et al. 2012 have re-evlauated and text modified |
| 7-934 | 7 | 33 | 36 | | 39 | How do we know that these results are or may be contaminated with dust? If there is noreference for this then the conclusion should (must) be removed. [Larry Thomason, United States of America] | Bond et al. 2012 have re-evlauated and text modified |
| 7-935 | 7 | 33 | 38 | | | The results from Ramanathan and Carmichael are derived using an assimilation method, which could introduce also other unclarified numerical artefacts. Is there more recent literature discussing the differences in BC forcing? [Michael Schulz, Norway] | Bond et al. 2012 have re-evlauated and text modified |
| 7-936 | 7 | 33 | 40 | | | Is the current AeroCom estimate corrected for regional bias against for instance AAOD? I wonder if a more positive BC forcing is as likely as the median value of BCFF forcings from the model ensemble. Are the RFs and AeroCom computations really defined against 1750? [Michael Schulz, Norway] | Too detailed to explicitly go into these details here |
| 7-937 | 7 | 33 | 44 | 33 | 49 | A reference for this work is needed, this is not part of regular AeroCom activities. This seems to be a highly uncertain approach. [Gunnar Myhre, Norway] | Bond et al. 2012 have re-evlauated and text modified |
| 7-938 | 7 | 33 | 44 | 33 | 49 | One concern is that much of the aerosol forcing discussion in this chapter is based upon unpublished results (such as AEROCOM Phase II). This is not a good strategy for an IPCC report, as the community has not had a chance to evaluate the work. For instance, it is stated on page 33, lines 44-49: When the AeroCom BC fossil fuel and BC from biomass burning are combined and scaled by AERONET observations, we derive an anthropogenic BC radiative forcing of 0.4 0.2 W m2, considering uncertainties associated with model diversity, AERONET model bias, AERONET representativeness and clear sky bias (e.g., AERONET does not observe over ocean or on cloudy days), dust contamination of AERONET data, anthropogenic fraction, vertical distribution of BC, underlying surface albedo, radiative transfer and covariance of | citaitons added for SOD |

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| | | | | | | aerosol with clouds. This statement requires substantiation in the text, or citations indicating where the reader may obtain the details that lead to this conclusion. I realize substantiating this statement with text is not possible because space is extremely limited in the AR5 report, but the text should still point the reader to the details in the published literature. If the results have not been published yet, then it is my opinion that they do not belong in the AR5 report; even very recently published work should be presented with caution (and not a mainstay of AR5), since the scienti c vetting process continues after publication. [Gregory Schuster, USA] | |
| 7-939 | 7 | 33 | 44 | 34 | 5 | Is there a reference for this or is this a new finding? The 'We' terminaology suggests that this is a new finding in which case, I am uncomfortable with this being presented as factual as it has not been through peer review and there is not enough detail to review this statement internal to the report. The recommened radiative forcing of a variety of aerosol are more easily understood and defensible as existing work. [Larry Thomason, United States of America] | Text now reduced and Bond et al. study refered to |
| 7-940 | 7 | 33 | 48 | 33 | 48 | There are other limitations to the AERONET observations, such as cloud contamination and limited information about particle properties, especially single-scattering albedo. [Ralph Kahn, United States of America] | Text now reduced and section on optical properties expanded in 7.3.4 |
| 7-941 | 7 | 33 | 51 | 33 | 52 | The POM forcing of -0.05 W/m2 is virtually the same as that from Table 7 of Jacobson, M.Z., Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001 (-0.057 W/m2). Please clarify. [Mark Z. Jacobson, U.S.A.] | noted but old reference no used here |
| 7-942 | 7 | 33 | 51 | 33 | 52 | About the sentence:"For organic carbon aerosol from fossil fuel AeroCom models give a –0.0 to –0.1 W m–2 (90% range) with a –0.05 W m–2 median estimate from fossil fuel emissions and we adopt this our our best estimate." Delete one of the "our". The same comment for the next two sentences. [Rubén D Piacentini, Argentina] | agreed, "our" deleted |
| 7-943 | 7 | 33 | 52 | 33 | 52 | The word "our" is repeated in the sentence "...our our best estimate" [BEGONA ARTINANO, SPAIN] | agreed, "our" deleted |
| 7-944 | 7 | 33 | 52 | | | "our our" --> "as our" [Minghuai Wang, United States of America] | agreed, "our" deleted |
| 7-945 | 7 | 33 | 54 | 33 | 54 | It would be helpful for the reader to specify what "biomass burning aerosol" consist of, e.g., by adding in parenthesis before "AeroCom": "(BC and OC)" [Jón Egill Kristjánsson, Norway] | agreed, text now clarified |
| 7-946 | 7 | 33 | 54 | 33 | 55 | Please try to not mix the species-based with source-based estimation. [Chien Wang, United States of America] | agreed, text now clarified |
| 7-947 | 7 | 33 | 55 | 33 | 55 | "our our" should be "as our" [Jón Egill Kristjánsson, Norway] | agreed, "our" deleted |
| 7-948 | 7 | 34 | 4 | 34 | 5 | Please include the nitrate forcing of -0.05 W/m2 from Table 7 of Jacobson, M.Z., Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001 in your discussio of nitrate forcing as it is the most physically-based calculation of nitrate forcing to date. [Mark Z. Jacobson, U.S.A.] | We do not see the need to single out an individual earlier study here |
| 7-949 | 7 | 34 | 5 | 34 | 5 | Here, gobal RF for nitrate is estimated. Actually, RF for nitrate aerosol show large difference in regional scale, as shown by Wang(2010) 's work. Therefore, it is suggested that this fact be included in the report. Tijian Wang, Shu Li, Fanhui Shen, Junjun Deng, and Min Xie, Investigations on direct and indirect effect of nitrate on temperature and precipitation in China using a regional climate chemistry modeling system, J. Geophys. Res., 115, D00K19, doi:10.1029/2009JD013165, 2010 [Tijian Wang, China] | agreed, citation now added |
| 7-950 | 7 | 34 | 7 | 34 | 7 | Please include the soil dust forcing of -0.07 W/m2 from Table 7 of Jacobson, M.Z., Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001 as it is the only soil-dust chemical-resolved calculation of that forcing to date. [Mark Z. Jacobson, U.S.A.] | We do not see the need to single out an individual earlier study here |
| 7-951 | 7 | 34 | 7 | 34 | 14 | Maybe shorten and just discuss figure here? [Andrew Gettelman, USA] | text slightly shortened |
| 7-952 | 7 | 34 | 17 | 34 | 19 | Figure 7.12 and the text are missing any discussion of ammonium forcing. Table 7 of Jacobson, M.Z., Global | condsidered with nitrate forcing, text made more |

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| | | | | | | direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001 calculated this forcing as +0.06 W/m2. This omission should affect the overall forcing estimate as well. [Mark Z. Jacobson, U.S.A.] | explicit |
| 7-953 | 7 | 34 | 33 | 34 | 37 | Too generalization. It is hard to finde which article gives such formulation. [Bin Zhu, China] | added references to equilibrium climate change studies. Also see table 8.2 of Bond et al (2012). |
| 7-954 | 7 | 34 | 35 | 34 | 36 | Which "other climate forcers"? [Nicolas Bellouin, United Kingdom] | removed "other climate forcers" |
| 7-955 | 7 | 34 | 35 | | | The results in Koch et al suggest a much larger efficacy for this. So here and on next page at end of section, should either be a larger value on the high end or justify why it's 2-4 only. [Drew Shindell, USA] | We now comment on the large efficacy estimate from Koch et al (2009), but also note that it was derived from a very small forcing (0.01 W/m2), and thus could contain substantial noise. Furthermore, the forcing was derived from fixed-SST calculations, whereas the climate signal was determined from Q-flux simulations. Thus, there is a slight inconsistency between the forcing and response components. |
| 7-956 | 7 | 34 | 40 | 34 | 40 | Replace "which" with "that". [Steven Ghan, USA] | Passage no longer included |
| 7-957 | 7 | 34 | 47 | 34 | 51 | It may be interesting to assess whether observations have helped or could help in reducing those key uncertainties. [Nicolas Bellouin, United Kingdom] | Added that the Bond et al assessment applies measurements from Doherty et al (2010) to help constrain Arctic BC-in-snow. |
| 7-958 | 7 | 34 | 52 | 34 | 52 | Replace "We". Please check the same paragraph for the same statements. [Chien Wang, United States of America] | No longer included |
| 7-959 | 7 | 34 | 54 | 34 | 54 | About the sentence: In "...leads to a radiative effect of of +0.04 W m–2", delete one of the "of". [Rubén D Piacentini, Argentina] | Done |
| 7-960 | 7 | 34 | 54 | 34 | 54 | “radiative effect of of +0.04 W m–2” verbosity “of” [Bin Zhu, China] | Done |
| 7-961 | 7 | 35 | 1 | 35 | 7 | I think all this 'we' is not assessing, but actually new work that requires justification. It seems out of character with the rest of the Chapter. It is not possible to reproduce the numbers from the information and published literature. I think this needs to be analyzed and published, and then quoted here. [Andrew Gettelman, USA] | This text has been re-written to summarize work from Bond et al (2012) and remove references to "we". |
| 7-962 | 7 | 35 | 1 | 35 | 7 | This part is unclear, in particular the last sentence. Is it the 1750-2010 change that not necessarily is anthropogenic or the cause for an estimate of 0.04 instead of 0.05 Wm-2? [Gunnar Myhre, Norway] | This passage has been re-written. We now report 1750-2010 RF changes, and refrain from discussing the anthropogenic contribution. Bond et al (2012) mention that biofuel BC sources were non-zero in 1750. |
| 7-963 | 7 | 35 | 6 | | 7 | I am pleased to see the authors dismiss this as lacking robust evidence. [Stephen E Schwartz, USA] | No action needed |
| 7-964 | 7 | 35 | 9 | 35 | 17 | Heterogeneous chemistry of aerosols, affecting the pre-cursor gases such as Ozone, in the radiative forcing estimations needs to be better understood both in polar and other continents. [Panuganti China Sattilingam Devara, India] | comment not relevant here |
| 7-965 | 7 | 35 | 12 | 35 | 12 | Abbreviation BrC looks like a chemical formula, which is confusing, and BrownC is used a few lines below. In fact, no abbreviation is needed at all. [Nicolas Bellouin, United Kingdom] | We no longer refer to brown carbon. |
| 7-966 | 7 | 35 | 19 | 35 | 20 | I'm wondering how you actually determine the uncertainty range quantitatively, given the qualitative scaling that went into the result. [Ralph Kahn, United States of America] | We now describe the uncertainty estimate in slightly more detail, refering to the quadrature calculation of Bond et al (2012). Each of the terms contributing to this calculation, however, carries some subjectivity. |
| 7-967 | 7 | 35 | 19 | 35 | 22 | Statement is fine, but I am not sure how the authors got there: see comment above. There is non-reproduceable work in here that is not referenced properly. (7-34, lines 1-7) [Andrew Gettelman, USA] | We now refer to the assessment from Bond et al (2012) as a source for this estimate and range. |

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| 7-968 | 7 | 35 | 20 | 35 | 22 | The statement of effectiveness of the forcing seems to be noted here for the first time, so a source should be provided. [Andrew Ferrone, Germany] | We now cite studies that quantify this efficacy |
| 7-969 | 7 | 35 | 24 | 36 | 14 | Page 36, lines 6-14 tell it all - after almost 2 pages of details, the authors conclude that the feedback effect from aerosols is probably small. If this is the case, don't waste the IPCC reader's time on this - things that turn out to be unimportant should only be discussed briefly, just enough to let the reader know that the community has thought about the problem, people have published papers on it, and so far it doesn't seem to be a big deal. This can be done in a paragraph or two. This is emblematic of the entire aerosol section - the authors seem not to want to focus on what is important, but rather on a complete discussion of aerosol research. This is a disservice to the IPCC readership, which needs to be told what the community has decided are the most important issues. [Anthony Del Genio, USA] | HL/it is not a disservice to the community to assess a large literature body on aerosol feedback such as CLAW and update the assessment made in the TAR and AR4 on this. The section is now a bit shorter and we have revised the synthesis. We also emphasize that the feedback may be important on the regional scale. |
| 7-970 | 7 | 35 | 28 | 35 | 28 | This might be a little picky, but changes in climate can also modify the sources of anthropogenic aerosol; to take just one example, changing temperature will alter fuel consumption for heating and cooling and possibly also for agriculture. [Ralph Kahn, United States of America] | The sentence has been removed. |
| 7-971 | 7 | 35 | 28 | 35 | 36 | The second sentence "which may in turn feedback…" seems being duplicated by the second last sentence "The response of aerosols…". [Chien Wang, United States of America] | Good point. We have revised the sentences. |
| 7-972 | 7 | 35 | 38 | 35 | 38 | The emissions, distribution or chemistry of aerosols or aerosol precursors could respond significantly to climate change, but there is little consistency across studies in the magnitude or sign of this response [Jacob and Winner, 2009]. The lack of consistency arises in part from the complexity of the dependence of different aerosol components on meteorological variables and in part because of uncertainties in the coupling between aerosols and the hydrological cycle. Using an 11-year data set of surface observations across the United States, Tai et al. [2010] analyzed the response of aerosol to changing meteorology, providing a test for models attempting to simulate the sensitivity of aerosol to climate change. [Loretta Mickley, USA] | HL/This is a good summary of responses of aerosols to climate change. The synthesis now says "The emissions, properties and concentrations of aerosols or aerosol precursors could respond significantly to climate change, but there is little consistency across studies in the magnitude or sign of this response. The lack of consistency arises mostly from our limited understanding of processes governing the source of natural aerosols and the complex interplay of aerosols with the hydrological cycle. " However the Jacob and Winner (2009) reference is too specific to AQ to be used here. |
| 7-973 | 7 | 35 | 38 | 35 | 38 | Jacob, D.J., and D.A. Winner, Effect of climate change on air quality, Atmos. Environ., 43, 51-63, 2009. [Loretta Mickley, USA] | This paper is too specific to AQ and less relevant than others for this section. |
| 7-974 | 7 | 35 | 38 | 35 | 38 | Tai, A.P.K., L.J. Mickley, and D.J. Jacob, Correlations between fine particulate matter (PM2.5) and meteorological variables in the United States: implications for the sensitivity of PM2.5 to climate change, Atmos. Environ., 44, 3976-3984, 2010. [Loretta Mickley, USA] | This paper is too specific to AQ and less relevant than others for this section. |
| 7-975 | 7 | 35 | 38 | | | I am not sure all this is necessary in section 7.6.3.2 [Andrew Gettelman, USA] | See our responses to Comment #7-969. |
| 7-976 | 7 | 35 | 40 | 35 | 57 | The changes in sea salt and soil dust depend on the emission scenario chosen. For example, under the A1B scenario, soil dust may slightly decrease due to lower wind speeds caused by higher aerosol loadings and enhanced stability, whereas under the B1 scenario, it may increase due to higher wind speeds due to lower aerosol loadings in 2030 Jacobson, M.Z., and D.G. Streets, The influence of future anthropogenic emissions on climate, natural emissions, and air quality, J. Geophys. Res., 114, D08118, doi:10.1029/2008JD011476, 2009. Effects on sea spray similarly depended on the scenario. [Mark Z. Jacobson, U.S.A.] | Jacobson and Streets (2009) is now cited. Because of the page limit of the chapter, we can only summarize major conclusions from literature and cannot go into detailed explanation in each reference. |
| 7-977 | 7 | 35 | 41 | 35 | 41 | Struthers et al., ACP (2011) could be referred to in this section. [Gunnar Myhre, Norway] | Struthers et al. (2011) has been added as a reference. |
| 7-978 | 7 | 35 | 41 | | | replace "influence" with "influences the" [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | The sentence has been removed. |
| 7-979 | 7 | 35 | 41 | | | Climate change influences the atmospheric… [Warren Richard Leaitch, Canada] | The sentence has been removed. |
| 7-980 | 7 | 35 | 41 | | | The term "sea salt" to denote aprticles emanatin from the ocean surface is misleading since it neglects recent findings of substantial amounts of organic particles. [Henning Rodhe, Sweden] | sea-salt has been changed to sea-spray where appropriate |

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| 7-981 | 7 | 35 | 48 | 35 | 48 | "feedback" should here be "feed back", because "feedback" is not a verb. [Jón Egill Kristjánsson, Norway] | Corrected. |
| 7-982 | 7 | 35 | 49 | 35 | 49 | "on" should be "of" [Jón Egill Kristjánsson, Norway] | Corrected. |
| 7-983 | 7 | 35 | 49 | | | If there is no concensus in the likely change in wind speed with climate change (some authors/models predict an increase in wind speed, others a decrease - although presumably the sign may be dependent on location), then understanding how air-to-sea gas exchange (for DMS for example) may change is just as problematic as understanding the generation of primary aerosol from the ocean's surface. Additionally deposition rates are dependant on wind speed. It would seem critcal to clarifiy what is likely to happen to wind speed as a result of climate change. [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | There is no certainty there and we reflect that uncertainty in the text. |
| 7-984 | 7 | 35 | 50 | 35 | 51 | "and some models predicting a widespread decrease in ice-free oceanic regions" is highly ambiguous, and must be rephrased. For example, it is not at all clear what the "widespread decrease" refers to. What will decrease? Also, it is not at all clear under what conditions this will happen. Future climate? [Jón Egill Kristjánsson, Norway] | this has been rephrased. |
| 7-985 | 7 | 35 | 50 | 35 | 51 | About the expression:"…..observations suggesting an increase in wind speed over the last two decades (Young et al., 2011) and some models predicting a widespread decrease in ice-free oceanic regions". In the final part of the sentence, as it was done with respect to the increase in wind speed, give references for the prediction of widespread decrease. [Rubén D Piacentini, Argentina] | See our responses to above two comments. |
| 7-986 | 7 | 35 | 53 | 35 | 56 | Again, a very poorly worded and difficult to read sentence. After reading it about 15 times, I concluded that it actually seems to be correct, but with all the parenthes and furhter parentheses within those, it looks more like a mathematical equation than a piece of text. It might be readable for a computer, but not for a human being. [Jón Egill Kristjánsson, Norway] | Those sentences have been rewritten. |
| 7-987 | 7 | 35 | 56 | 35 | 56 | "After "concentration)." add: "Wang and Fu (Wang et al., 2008, Fu et al., 2008) found that time series of dust event over East Asia exhibit a decreasing trend since the mid 1980s." References: Wang X., J. Huang, M. Ji, and K. Higuchi, 2008: Variability of East Asia dust events and their long-term trend, Atmos. Environ., 42, 3156-3165.<br>Fu P., J. Huang, C. Li, and S. Zhong, 2008: The properties of dust aerosol and reducing tendency of the dust storms in northwest China, Atmos. Environ., 42, 5896-5904.<br>" [Jianping Huang, China] | As mentioned at the beginning of the Section 7.3.6, we assess here the relevance and strength of aerosol-climate feedbacks in the context of future climate change scenarios. |
| 7-988 | 7 | 35 | 56 | 36 | 2 | It would be helpful if you could elaborate on those factors that might account for some models predicting *more* dust in the future as well ( I can guess what they might be, but it would be worth having it explicit if possible). [Ralph Kahn, United States of America] | It Is now stated explicitly that "Woodward et al. (2005) found a factor of 3 increase in dust burden in 2100 relative to present-day with the two-way vegetation-climate feedbacks. " |
| 7-989 | 7 | 35 | | | | sec. 7.3.6: nothing on diffuse light effects on CO2 uptake – Mercado et al (Nature, 2009) showed that "global dimming" reduced total surface SW and reduced vegetation carbon uptake, but the increase in the diffuse light fraction acts to increase productivity which more than offsets the decrease due to lower light levels. Can cross reference Ch6 [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland] | Because of the page limit for the chapter, we think it is better to leave this aerosol-CO2 interaction in Chapter 6. |
| 7-990 | 7 | 36 | 6 | 36 | 7 | Suggest rewording something like: "… despite two decades of ressearch that have yielded important insights into this complex, coupled system (see Ayers…" this avoids leaving the impression in the first sensentence that no progress was made, or suggesting that there has been anything like a focused, 20-year effort, that brought to bear all the resources needed to address the many aspects of this system. I realize that you make these points subsequently. [Ralph Kahn, United States of America] | Changed. |
| 7-991 | 7 | 36 | 6 | 36 | 14 | Refer to recent Nature paper by Quinn and Bates. [Warren Richard Leaitch, Canada] | Added. |
| 7-992 | 7 | 36 | 6 | 36 | 14 | See Quinn and Bates, Nature, vol. 480, 51 - 56, 20122 for a review of the CLAW hypothesis that finds a low sensitivty between change and response in each step of the feedback loop. The result is a low likelihood that DMS controls marine boundary CCN concentrations and that a climate feedback loop driven by DMS exists. [Patricia Quinn, US] | Added. |

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| 7-993 | 7 | 36 | 6 | 36 | 36 | Quinn and Bates, 2011, should be referenced here. I would suggest that the use of "CLAW" to provide a feedback loop is flawed or needs to be used with great caution by modellers. It may be that DMS can be used as a proxy for other biologically mediated gases and it would be interesting to see whether the use of Chl-a, rather than DMS, would be sufficient to initiate modelled feedback (signficant effort required in the field and by modellers). However the atmospheric processing and reactions of DMS are highly complex and this complexity is not captured well in models. Additionally the location in the atmosphere of DMS chemistry (losses in Marine Boundary Layer, perhaps NPF in Free Troposphere) is poorly represented. The small changes in DMS emissions with climate change and the small changes in DMS related CCN numbers predicted by models suggest low confidence when contrasted with the limited ability to capture the complexity of DMS in the atmosphere. [Jill Cainey, United Kingdom of Great Britain & Northern Ireland] | HL/We now have cited Quinn and Bates (2011). What other biologically mediated gases do you have in mind here? The model representation of DMS and aerosol microphysics has greatly improved. We agree that the processes are still uncertain, as recognised for DMS production, but the state of play is that CCN is not very sensitive to DMS. |
| 7-994 | 7 | 36 | 6 | | | I do not believe that the DMS-sulfate-cloud-climate feedback loop remains uncertain. Two decades of research have put the hypothesis to rest. The Carslaw, Woodhouse, and Quinn and Bates references all conclude that the sensitivity of the CCN population to DMS changes is weak at best. It is time to state that outright. [Timothy Bates, USA] | the text is now more assertive, but recognises the remaining uncertainties.. |
| 7-995 | 7 | 36 | 7 | 36 | 7 | Quinn and Bates, Nature (2011) is an appropriate reference here [Gunnar Myhre, Norway] | Cited. |
| 7-996 | 7 | 36 | 8 | 36 | 8 | "solar radiation dose" sounds strange. I suggest replacing with e.g., "absorbed solar radiation" [Jón Egill Kristjánsson, Norway] | Changed. |
| 7-997 | 7 | 36 | 11 | 36 | 11 | I understand why you mention, here and elsewhere, where there is "some consistency among …models," but, as you know, this could mean simply that they all make similar (correct or incorrect) assumptions. It is of course much more powerful if models and measurement agree with each other, quantitatively, at the appropriate level-of-detail. I'm thinking that more emphasis could be placed on the cases throughout the chapter where, e.g., "95% confidence" is based solely on model *diversity*, and where critical measurements are needed to test model assumptions. I see this as a key role for the IPCC in pointing the way forward. [Ralph Kahn, United States of America] | We try to assess all lines of evidence here and recognise uncertainties. IPCC does not make recommendations on what research or measurements should be done. |
| 7-998 | 7 | 36 | 16 | 36 | 18 | Using observations together with a chemical transport model, Tai et al. [2012] found that the observed correlations of sulfate aerosol with temperature do not arise from direct dependence, but rather from covariation with synoptic transport. [Loretta Mickley, USA] | Noted, but this does not neessarily contradict the text. |
| 7-999 | 7 | 36 | 16 | 36 | 18 | Tai, A.P.K., L.J. Mickley, D.J. Jacob, E.M. Leibensperger, L. Zhang, J.A. Fisher, and H.O.T. Pye, Meteorological modes of variability for fine particulate matter (PM2.5) air quality the United States: implications for PM2.5 sensitivity to climage change, submitted to Atmos. Chem. Phys., 2012. http://acmg.seas.harvard.edu/publications/tai_2011.pdf [Loretta Mickley, USA] | This paper is too specific to AQ and less relevant than others for this section. |
| 7-1000 | 7 | 36 | 24 | 36 | 30 | "There is some agreement with global aerosol models that climate change will contribute to decrease nitrate…" Nitrate was found to increase under the A1B scenario and decrease under the B1 scenario in Jacobson, M.Z., and D.G. Streets, The influence of future anthropogenic emissions on climate, natural emissions, and air quality, J. Geophys. Res., 114, D08118, doi:10.1029/2008JD011476, 2009, which accounted for country and source-specific emission changes in each scenario. [Mark Z. Jacobson, U.S.A.] | While the predicted nitrate concentrations in Jacobson and Streets (2009) included the effects of future changes in both climate and anthropogenic emissions, the statement here is focused on the effect of climate change alone. This is now clarified in the text. |
| 7-1001 | 7 | 36 | 32 | 36 | 36 | "…influence ammonium formation…" Ammonium ion was found to increase under the A1B scenario and decrease under the B1 scenario by 2030 in Jacobson, M.Z., and D.G. Streets, The influence of future anthropogenic emissions on climate, natural emissions, and air quality, J. Geophys. Res., 114, D08118, doi:10.1029/2008JD011476, 2009. Part of the increase in the ammonium ion in the A1B scenario was a decrease in ammonium sulfate to a greater extent than an increase in ammonium nitrate. [Mark Z. Jacobson, U.S.A.] | As mentioned above, the section here is focused on the effect of climate change alone. |
| 7-1002 | 7 | 36 | 32 | 36 | 36 | The description of the change of ammonium is not clear on whether it is caused by descrease of sulfate and nitrate ot due to change in sink (scavenging). [Chien Wang, United States of America] | Sentence has been shortened. |
| 7-1003 | 7 | 36 | 38 | 36 | 38 | I suggest to add "Natural" before the "Carbonaceous aerosols", because the anthropogenic carbonaceous aerosls were addressed in previous secions, and in this section only natural ones were discussed. [Xuemei Wang, China] | We do not agree with the reviewer because we are not looking at natural aerosols alone. We are interested in how all aerosol species evolve in a |

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| | | | | | | | warmer climate. |
| 7-1004 | 7 | 36 | 43 | 36 | 43 | Replace "which" with "that". [Steven Ghan, USA] | Changed. |
| 7-1005 | 7 | 36 | 47 | 36 | 47 | add "s" to form = forms [Patrick CHAZETTE, France] | Added. |
| 7-1006 | 7 | 36 | 47 | 36 | 54 | add "Leaitch et al., 2011: Temperature Response of Organic Aerosol from Temperate Forests, Atmospheric Environment, 2011, doi:10.1016/j.atmosenv.2011.08.047" to this discussion. [Warren Richard Leaitch, Canada] | Cited. |
| 7-1007 | 7 | 36 | 49 | 36 | 49 | There are experimental evidences that biogenic emissions can be inhibited during ambient stress conditions. Under these conditions (high air temperature or/and water vapour deficit) the Guenter et al. 1993 algorithm does not reproduce the actual emissions of some species. This effect is related to the diurnal physiological cycle of vegetation which closes its stomata as response to water and temperature stress and zero emissions are produced. These stress ambient conditions can be recorded during heat waves and droughts that would affect sensitive areas in a climate change scenario. In the Mediterranean, this beahaviour has been observed and experimetally documented in Plaza et al., 2005. Reference: J. Plaza, L. Núñez, M. Pujadas, R. Pérez-Pastor, V. Bermejo, S. García-Alonso and S. Elvira (2005), Field monoterpene emission of Mediterranean oak (Quercus ilex) in the central Iberian Peninsula measured by enclosure and micrometeorological techniques: Observation of drought stress effect. Journal of Geophysical Research, vol. 110, D03303, doi:10.1029/2004JD005168). A reference to this effect should be included in the text. [BEGONA ARTINANO, SPAIN] | Thank you for pointing this. There is no space to include all references and IPCC AR5 is focused on references that are new since IPCC 2007 report. |
| 7-1008 | 7 | 36 | 50 | 36 | 50 | "T change" is ambiguous. Please replace by "temperature change" [Jón Egill Kristjánsson, Norway] | Replaced. |
| 7-1009 | 7 | 36 | 54 | 36 | 54 | "T effect" is ambiguous. Please replace by "temperature effect" [Jón Egill Kristjánsson, Norway] | Replaced. |
| 7-1010 | 7 | 36 | 56 | 36 | 56 | Wu et al. [2012] found that climate- and CO2-driven changes in vegetation alone would increase the global mean concentration of organic aerosol by 20% in 2100, relative to the present-day. Wu, S., L.J. Mickley, J.O. Kaplan, and D.J. Jacob, Impacts of changes in land use and land cover on atmospheric chemistry and air quality over the 21st century, Atmos. Chem. Phys., accepted, 2012. [Loretta Mickley, USA] | Cited. |
| 7-1011 | 7 | 37 | 4 | 37 | 14 | Just use this paragraph, I am not sure all the work in this section is necessary. [Andrew Gettelman, USA] | See our responses to Comment #7-969. |
| 7-1012 | 7 | 37 | 8 | | | Is there a most likely value? [Andrew Gettelman, USA] | The scientific community is just starting to quantify the feedback factor. It is too early to give a most likely value. |
| 7-1013 | 7 | 37 | 13 | 37 | 14 | We suggest to add the following complement at the end of this paragraph: "Regional online-coupled climate models, with an explicit treatment of atmospheric chemistry and aerosols, provide the potential to better understand and quantify these feedbacks in relevant regions of the globe (Vogel et al., 2009)." REFERENCE: Vogel, B., Vogel, H., Bäumer, D., Bangert, M., Lundgren, K., Rinke, R., and Stanelle, T.: The comprehensive model system COSMO-ART – Radiative impact of aerosol on the state of the atmosphere on the regional scale, Atmos. Chem. Phys., 9, 8661-8680, doi:10.5194/acp-9-8661-2009, 2009 [Andrew Ferrone, Germany] | This is a very interesting point. However IPCC does not make recommendation on what research should be done or what research tools should be used. |
| 7-1014 | 7 | 37 | 16 | 37 | 16 | General comment on 7.4 is that it emphasizes on local and microphysics related aerosol-cloud-precipitation effects.In particular regarding aerosol 's impact on precipitation, a description of works on the "remote" impact or optical-dynamical impact is much needed. This is a quite fast growing field and there are numbers of publications. Many papers cited in the large-scale aerosol-precipitation effects in Tao et al. 2012 (Geophysical Review, in press) could be discussed here. [Chien Wang, United States of America] | GF/BS: Changed --> added discussion in 7.4 and 7.6 on the potential for aerosol gradients to influence precipitation patterns via changes in large-scale circulations (e.g. Lee, S., 2012: Effect of aerosol on circulations and precipitation in deep convective clouds. J. Atmos. Sci. doi:10.1175/JAS-D-11-0111.1, in press.); This includes aerosol-induced changes to anvils that set off radiative feedbacks (van den Heever et al. 2011). [[BS: to address also in 7.6]] |
| 7-1015 | 7 | 37 | 16 | | | In this section "Aerosol-Cloud Interactions", there is no assessment about how the process-level studies and fine-resolution modeling work help to parameterize aerosols and clouds in large-scale models. This is | added text pointing to this important avenue (end of 7.4.1.3) |

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| | | | | | | important as the uncertainties of global model highly depend on the development of aerosol and cloud parameterizations. [Jiwen Fan, United States of America] | |
| 7-1016 | 7 | 37 | 16 | | | Section 7.4: too much detail? Here would be a place to cut back. Also, I do not fully understand what the aerosol forcing definitinons mean and how they are calculated. I fear that the assesment is hiding different methods for estimating the aerosol indirect effects. Why not use mre standard terms like AIE, or Radiative Flux Perturbation (RFP)? I know a lot of thought has gone into this, but it needs to be refined a bit. [Andrew Gettelman, USA] | Much of 7.4.1 has been removed. The terminology comments are being addressed in coordination with the other sections. The section hsd gone through a general process of reorganization, thinning and focusing |
| 7-1017 | 7 | 37 | 27 | 37 | 27 | need to be more specific about “patterns”. “patterns” mean precipitation amount or other aspects of precipitation? As far as I know, Rosenfeld et al. (2008) discussed only about “precipitation amount” [Seoung Soo Lee, United States of America] | Changed sentence to: "but also intensity and spatial pattern of precipitation (e.g. Rosenfeld et al., 2008; Tao et al., 2012) |
| 7-1018 | 7 | 37 | 29 | 37 | 29 | what do you mean by “consistency” in the context of “a greater diversity of aerosol-cloud interactions” Since global models show a greater diversity of the interactions as authors say, I don’t see what constitutes the consistency. [Seoung Soo Lee, United States of America] | Added "internal" before consistency |
| 7-1019 | 7 | 37 | 29 | 37 | 31 | The following reference is appropriate for this statement: Jeong, M.-J. and Z. Li (2010), Separating real and apparent effects of cloud, humidity, and dynamics on aerosol optical thickness near cloud edges, J. Geophys. Res., 115, D00K32, doi:10.1029/2009JD013547. [Myeong-Jae Jeong, Republic of Korea] | added reference in 7.4.1.2 |
| 7-1020 | 7 | 37 | 31 | 37 | 31 | Just to give an example of what you mean here, you might add something like: "… methodological challenges associated with these correlations, such as accounting for changes in liquid water path, along with those of cloud particle properties, associated with differences in aerosol concentration and type (**and you might pick a few favorite references for this)." [Ralph Kahn, United States of America] | text has been removed |
| 7-1021 | 7 | 37 | 31 | 37 | | after "such correlation", add "(Li et al. 2011b)". Add Reference: Li, Z., F. Niu, J. Fan, Y. Liu, and D. Rosenfeld, Y. Ding (2011b), The long-term impacts of aerosols on the vertical development of clouds and precipitation, Nature-Geoscience, doi: 10.1038/NGEO1313. [Zhanqing Li, USA] | text has been removed |
| 7-1022 | 7 | 37 | 33 | 37 | 33 | I think this report is for general public. So, the word “buffer” needs to be explained using common terms. [Seoung Soo Lee, United States of America] | text has been removed. But we believe that "buffer" is clear based on dictionary definition " buffer", verb lessen or moderate the impact of. |
| 7-1023 | 7 | 37 | 39 | 37 | 39 | should be "has not yet been established…" [Anthony Del Genio, USA] | Paragraph has been removed |
| 7-1024 | 7 | 37 | 44 | | | I would not put precip susecptibility in the introduction: probably too much detail. [Andrew Gettelman, USA] | Paragraph has been removed |
| 7-1025 | 7 | 37 | 50 | 37 | 50 | Alter "aerosol cloud interactions" to "aerosol-cloud interactions" [Panuganti China Sattilingam Devara, India] | Paragraph has been removed |
| 7-1026 | 7 | 37 | 52 | 37 | 52 | Change "aerosol-cloud or aerosol-precipitation" to "aerosol-cloud-precipitation". [Chien Wang, United States of America] | Paragraph has been removed |
| 7-1027 | 7 | 37 | | | | Section 7.4: A major omission: the positive radiative forcing due to CCN-induced cloud invigoration. See Koren et al., ACP 2010: Aerosol-induced changes of convective cloud anvils produce strong climate warming. See also: Koren, I., Feingold, G. & Remer, L. A. The invigoration of deep convective clouds over the Atlantic: Aerosol effect, meteorology or retrieval artifact? Atmos. Chem. Phys. 10, 8855 8872 (2010). [Meinrat O. Andreae, Germany] | Invigoration is discussed in section 7.6 |
| 7-1028 | 7 | 38 | 1 | 38 | 1 | "Denman et al. (2007) catalogued several possible contributions to aerosol indirect effects." However, twelve effects of aerosol particles on climate, including several on clouds, were identified previously in Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, [Mark Z. Jacobson, U.S.A.] | Text not changed. Instead of parsing out all individual effects, we focus on the sum of all effects. This is a shift in tone from previous assessments, and given the difficulty of separating out coupled effects, we believe, a positive one. |
| 7-1029 | 7 | 38 | 1 | 38 | 16 | I am still not clear on these definitions. How is iAF calculated? Is it the total radiative flux perturbation? What is iRF? How does DRE fit in here (is it part of iAF)? The terms need better explanations, and perhaps a table of defniitions and how they are calculated. I'm not sure how you can separate iRF and iAF. I am not sure how | A new figure to address the terminology has been added |

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| | | | | | | this relates to the literature for example, where these terms are not common. There are multiple sets of numbers provided, but I am not sure what they mean. I am sure the authors have made a careful determination of these metrics, but they are not well explained. [Andrew Gettelman, USA] | |
| 7-1030 | 7 | 38 | 1 | 38 | 17 | The aerosol-cloud interactions and the associated cloud macro- and micro-physical parameters, particulalrly between below-cloud / cloud edges (clear), in-cloud environments need to be better undersood through both measurements and models. The radiative forcing due to thermodynamical effects in iRF and iAF need to be quantified. Also, models delineating the interactions between small-scale clouds and large-scale environment need to be developed. [Panuganti China Sattilingam Devara, India] | These are all valid points and they are addressed to the extent possible in the text that follows rather than right here. |
| 7-1031 | 7 | 38 | 1 | | 17 | Grouping these effects as the iAF seems useful. [Stephen E Schwartz, USA] | No change required |
| 7-1032 | 7 | 38 | 4 | 38 | 4 | As far as I know, Morrison and Grabowski (2011) do not talk about the compensation but discuss about radiative-convective equilibrium. Lee and Feingold (2010) simulated the compensation and explained the compensation mechanism. (reference: Lee, S. S., and Feingold, G.: Precipitating cloud-system response to aerosol perturbations, Geophys. Res. Lett., 37, L23806, doi:10.1029/2010GL045596, 2010.) [Seoung Soo Lee, United States of America] | No change. The reference is true to the spirit of the statement and is also an attempt to diversify references. |
| 7-1033 | 7 | 38 | 6 | 38 | 7 | The iRF defined here accounts only for changes in particle size. In particular, one can imagine changes that affect radiation but don't change, say, cloud amount or physical thickness. It may be more clear to say "two broad categories: the impact on radiative forcing of changes in particle size and number, all else held constant, and the final result..." [Robert Pincus, USA] | Not changed. "in the absence of macrophysical changes" is equivalent to "all else held constant" |
| 7-1034 | 7 | 38 | 6 | 38 | 8 | Instead of using indirect radiative forcing (iRF) and indirect adjusted forcing, I suggest to use RF of the indirect aerosol effect and AF of the indirect aerosol effect. IAE can be used as an acronym for indirect aerosol effect. We have other forcing mechanisms that are caused by indirect effects and iRF and iAF may cause confusion. [Gunnar Myhre, Norway] | New terminology has been adopted by Chapters 7 and 8 |
| 7-1035 | 7 | 38 | 8 | 38 | 12 | In addition to the Twomey and Albrecht effects, you might also indicate where the "semi-direct" (Ackerman?) effect fits into your iRF-iAF classification, again for continuity with AR4. [Ralph Kahn, United States of America] | The new terminology AFaci indicates the adjusted forcing due to aerosol-cloud-interactions. The semi-direct is mentioned in discussion of AFari (adjusted forcing due to aerosol-radiation interactions). There are aspects of the semidirect effect that are addressed under AFaci. |
| 7-1036 | 7 | 38 | 8 | 38 | 17 | A key factor of defining/deriving iAF in the very limited literature seems the arbitrary selected adjustment time. [Chien Wang, United States of America] | Aadjustment times can be hours (the dynamical/ thermodynamical adjustment time) to days (e.g., the adjustment to larger-scale changes to the system. [[Do we need to discuss adjustment timescales?]] |
| 7-1037 | 7 | 38 | 16 | | | Does iAF include direct effects (AF)? I was not clear regarding what the terms are in the report. [Andrew Gettelman, USA] | The new terminology should now clarify that AFaci does not include direct effects. |
| 7-1038 | 7 | 38 | 24 | 38 | 26 | Effects and artifacts of aerosol and cloud measurements on the correlation between cloud cover and AOD was studied in detail by Jeong and Li (2010). The effect of aerosol humidification on column AOD was investigated quantitatively (Jeong et al., 2007; Jeong, M.-J., Z. Li, E. Andrews, and S.-C. Tsay (2007), Effect of aerosol humidification on the column aerosol optical thickness over the Atmsopheric Radiation Measurement Southern Great Plains site, J. Geophys. Res., 112, D10202, doi:10.1029/2006JD007176.) and its contribution to correlation between cloud cover and AOD were also attempted to quantified (Jeong and Li, 2010). [Myeong-Jae Jeong, Republic of Korea] | Reference included |
| 7-1039 | 7 | 38 | 25 | 38 | 25 | It would be better to cite the updated version of Kahn et al.(2005) here, which is: Kahn, R.A., B.J. Gaitley, M.J. Garay, D.J. Diner, T. Eck, A. Smirnov, and B.N. Holben, 2010. Multiangle Imaging SpectroRadiometer global aerosol product assessment by comparison with the Aerosol Robotic Network. J. Geophys. Res. 115, D23209, doi: 10.1029/2010JD014601. Other references that would be appropriate here: Levy, R. C., Remer, L. A., Kleidman, R. G., Mattoo, S., Ichoku, C., Kahn, R., and Eck, T. F., 2010. Global evaluation of the Collection 5 MODIS dark-target aerosol products over land. Atmos. Chem. Phys. 10, 10399-10420, doi:10.5194/acp-10- | Changed Kahn reference |

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| | | | | | | 10399-2010, and Zhang, J., and J. S. Reid (2006), MODIS aerosol product analysis for data assimilation: Assessment of over-ocean level 2 aerosol optical thickness retrievals, J. Geophys. Res., 111, D22207, doi:10.1029/2005JD006898. [Ralph Kahn, United States of America] | |
| 7-1040 | 7 | 38 | 26 | 38 | 28 | There are 3 separate issues. 1) occasionally (NOT "often" as is stated in these lines) a pixel containing subpixel cloudiness falls into the set of pixels selected for aerosol retrieval or a pixel that is cloudy is misclassified as "cloud-free" and is used in the aerosol retrieval. The bigger the footprint (ie. OMI) the more often this occurs. It does not happen often with MODIS. 2) the cloud-free pixel is affected by adjacency effect of nearby clouds, either by instrument point spread functions along the scan or by photon scattering as described by the Wen paper or the Varnai and Marshak papers that are cited. 3) the cloud field environment with its higher and more variable humidities, particles generated or enlarged by nearby clouds and dissipating cloud fragments, create a situation in which it is unclear exactly what an aerosol is and what the retrieval should report or not. The way this is described here is an attempt, but not precise enough. Too much is missing, new particle generation and cloud processing are left out, and the radiative effects should be separated from the physical situation of swollen particles and cloud fragments. [Lorraine Remer, USA] | Text is modified . Although this text is brief, it is an attenpt to capture the key issues in a few lines, which is all we can afford. Cloud processing is now discussed in 7.4.1.2 |
| 7-1041 | 7 | 38 | 26 | 38 | 33 | There are in fact three issues identified here. 1) The retrieval of aerosol properties in partly-cloudy pixels; 2) the hydration of aerosols in near-cloud environments; 3) The impact of 3-dimensional (“non-columnar”) radiative transfer on aerosol retrievals. Also, the word “photon” is generally avoided in favor of “radiation” in most of the atmospheric radiation community. [Robert Pincus, USA] | text modified. We believe the three main points are captured. This is an attenpt to capture the key issues in a few lines, which is all we can afford. Photon changed to Radiation |
| 7-1042 | 7 | 38 | 28 | 38 | 28 | Not clear what is meant here by the legal term "proximate cause" [Jón Egill Kristjánsson, Norway] | Retained, based on dictionary defn |
| 7-1043 | 7 | 38 | 28 | 38 | 29 | Please add Su et al. [2008] to \The latter results from hydration e ects on aerosol optical properties (Charlson et al., 2007; Su et al.,2008; Twohy et al., 2009)" [Gregory Schuster, USA] | Reference added |
| 7-1044 | 7 | 38 | 29 | 38 | 29 | Again, a key reference: Tackett, JL and L Di Girolamo, 2009. Enhanced aerosol backscatter adjacent to tropical trade wind cloud revealed by satelltie-based lidar. Geophys. Res. Lett. 36, doi:10.1029/2009GL039264. This is actually a very nice paper, worth a look if you are not already familiar with it. [Ralph Kahn, United States of America] | Reference added |
| 7-1045 | 7 | 38 | 31 | 38 | 31 | "Light" instead of jargon "photons" [Daniel Murphy, United States of America] | Changed |
| 7-1046 | 7 | 38 | 33 | 38 | | at the end of the paragraph, add "Jeong and Li (2011) emoployed ARM air-borne experiment data which allows for the removal or accounting for the aerosol swelling effect and cloud contamination, but still found a dependence of aerosol optical depth towards cloud, which is unlikely an artifact but a likely manifesation of aerosols released from evaporation of cloud droplets." Add reference: Jeong, M.-J., Z. Li, (2010), Separating real and apparent effects of cloud, humidity, and dynamics on aerosol optical thickness near clouds, J. Geophy. Res.,, doi:10.1029/2009JD013547 [Zhanqing Li, USA] | Reference to Jeong and Li (2010) added above. See 7-1038 |
| 7-1047 | 7 | 38 | 35 | 38 | 45 | To shorten could delete this paragraph. Some material sort of an advertisement for polarimetry should definitely be deleted. [Daniel Murphy, United States of America] | Paragraph tightened and rearanged |
| 7-1048 | 7 | 38 | 41 | | | Other relevant field studies include: Hegg et al., ACP, 2012; Kleinman et al., ACP, 2012; Leaitch et al., ACP, 2010 [Warren Richard Leaitch, Canada] | Added reference to other field experiments like Vogelmann et al, (2012). Attempt here was to identify a few main field experiments rather than studies that emerged from those field experiments. |
| 7-1049 | 7 | 38 | 43 | 38 | 44 | Insert comma after "result" and remove comma after "clouds". [Steven Ghan, USA] | Changed |
| 7-1050 | 7 | 38 | 51 | | | Consider adding the following on line 51, right before \Furthermore":<br>Su et al. [2010] developed an analysis method to assess the e ects of di erent air masses on cloud properties and TOA albedo. They minimize the aerosol and cloud retrieval artifacts by eliminating the most problematic retrievals and use estimated inversion strength and vertical velocity to constraint the large-scale meteorology. [Gregory Schuster, USA] | Text not changed here in the interests of a synthetic style. However, Su et al reference was added in discussion of artefacts (7-1043) |
| 7-1051 | 7 | 38 | 54 | 38 | 54 | The in-situ observational studies have always been incomplete in terms of assessing a net forcing impact. | Not changed. The text "observationally-based |

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| | | | | | | That is something that satellite and models must do, as this assessement is attempting. To say that observationally based inferences undermine confidence is an unreasonable statement. The empirical based iRF values in Figure 13 are actually closer to your final assessment than many of the more detailed models. [Warren Richard Leaitch, Canada] | inferences undermine confidence" is intended to convey the difficulty of assessing causality from satellite observations. If forcing is to be attempted from satellite studies as the reviewer suggests, then we must be able to separate correlation from causality. |
| 7-1052 | 7 | 39 | 4 | 39 | 4 | Please add references for convective clouds, such as Khain et al. 2005 and Fan et al. 2009. References:(1) Khain A., D. Rosenfeld, and A. Pokrovsky (2005), Aerosol impact on the dynamics and microphysics of deep convective clouds. Q. J. R. Meteorol. Soc., 131, 1. (2) Fan, J., T. Yuan, J. M. Comstock, S. Ghan, et al.(2009), Dominant role by vertical wind shear in regulating aerosol effects on deep convective clouds, J. Geophys. Res., 114, D22206, doi:10.1029/2009JD012352. [Jiwen Fan, United States of America] | These references are now relevant to section 7.6 which addresses deep convection. |
| 7-1053 | 7 | 39 | 4 | 39 | 6 | The sentence "Aerosol-cloud interactions in climate models …." is rather extreme, and the references given in support of it are very old. To day, many climate models explicitly calculate aerosol activation, some models have detailed ice nucleation parameterizations, etc. Indeed, the text on lines 15-21 describes significant advances in recent years. [Jón Egill Kristjánsson, Norway] | Text changed. References to new activation and nucleation parameterizations has been added. |
| 7-1054 | 7 | 39 | 5 | | | Aerosol-cloud interactions were first introduced in climate models by empirically based observations, not "simple calculations or highly idealized models". But perhaps the discussion is referring to precipitation only? The distinction between cloud and precipitation is not always clear in section 7.4. [Warren Richard Leaitch, Canada] | text has been modified for clarity. |
| 7-1055 | 7 | 39 | 7 | 39 | 7 | what does “interactions across scales” mean regarding the buffering? Studies on the buffering have shown compensations among cloud-scale microphysics and dynamics but not among cloud scale and other scales like synoptic scale. Could you clarify this? [Seoung Soo Lee, United States of America] | Valid point but text not changed. In an assessment we can unfortunately not go into the detail we would like to. While buffering has been discussed on varius scales (microphysical, mesoscale), it has, to our knowledge, not been studied at synoptic scales. [[BS and Are some subtle word changes appropriate]] |
| 7-1056 | 7 | 39 | 8 | 39 | 13 | aren’t these examples all about cloud-scale interactions but not across scales? [Seoung Soo Lee, United States of America] | See 7-1056 |
| 7-1057 | 7 | 39 | 10 | 39 | 13 | I was tickled to see that my 2007 mixed layer model paper is used to argue that the physical system may be less sensitive to aerosols than climate models suggest (7.4.1.3). Should the reader take away the idea that climate models are not as advanced in their representation of cloud-aerosol interaction as a mixed layer model? [Robert Wood, USA] | Text not changed. The mixed-layer model produces a mixed layer by design, which is something GCMs struggle to get - e.g. in Scu. |
| 7-1058 | 7 | 39 | 12 | 39 | 12 | The sentence would read much better if "it is more likely than not" were replaced by "it is likely that" [Jón Egill Kristjánsson, Norway] | Text changed |
| 7-1059 | 7 | 39 | 15 | 39 | 17 | Yes, but again the increase of process representation in models has not necessarily advanced the result, as discussed by Penner and by Lohmann in the past. [Warren Richard Leaitch, Canada] | Text changed above in 7.4.1.3 to point out that although parameterizations are more sophisticated, they still rely on unresolved physics as input. |
| 7-1060 | 7 | 39 | 15 | 39 | 21 | "The representation of aerosol effects in large scale models has been advanced… (Quaas et al., 2009; Lohmann et al., 2008" These models were preceeded by GATOR-GCMOM, which is still the only model that treats the evolution of size- and composition-resolved hydrometeor particle number and chemical component mass from size- and composition-resolved aerosol particles, explicit size- and composition-resolved coagulation interactions among aerosols, between aerosols and hydrometeors, and among hydrometeors, and tracking of aerosol particles and their components through precipitation (Jacobson, M. Z., Development of mixed-phase clouds from multiple aerosol size distributions and the effect of the clouds on aerosol removal, J. Geophys. Res., 108 (D8), 4245, doi:10.1029/2002JD002691, 2003;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). That model represented effects of aerosols in convective, ice, and mixed-phase aerosols in Jacobson (2002) [Mark Z. Jacobson, U.S.A.] | To be changed: Since this is just pointing to model development rather than results it seems a fair request; who else can we add for balance. |

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| 7-1061 | 7 | 39 | 15 | 39 | 21 | Findings in cloud-resolving modeling of aerosol-convection processes, however uncertain, should be discussed as well. Again, Tao et al. 2012 (Geophy. Rev) and related references could be used. [Chien Wang, United States of America] | Changed. Tao reference included |
| 7-1062 | 7 | 39 | 15 | | | It would be important to note which CMIP5 models actually include such effects in a comprehensive way. [Johannes Quaas, Germany] | CMIP5 models are now shown specifically in new Figure 7.14 and caption spells out which. |
| 7-1063 | 7 | 39 | 18 | | | Another study of convective microphysics: Song and Zhang, 2011: Song, X., and G. J. Zhang (2011), Microphysics parameterization for convective clouds in a global climate model: Description and single-column model tests, J. Geophys. Res., 116, D02201, doi:10.1029/2010JD014833. A global paper (Song & Zhang 2012) is in review. [Andrew Gettelman, USA] | Added reference to Song et al. 2011 |
| 7-1064 | 7 | 39 | 19 | 39 | 21 | It is worth to mention the PNNL-MMF work here, as the PNNL-MMF has been applied to study aerosol indirect forcing (Wang et al., 2011, GMD; Wang et al., 2011, ACP). (Wang, M., Ghan, S., Ovchinnikov, M., Liu, X., Easter, R., Kassianov, E., Qian, Y., and Morrison, H.: Aerosol indirect effects in a multi-scale aerosol-climate model PNNL-MMF, Atmos. Chem. Phys., 11, 5431-5455, 10.5194/Acp-11-5431-2011, 2011). [Minghuai Wang, United States of America] | Changed; reference added |
| 7-1065 | 7 | 39 | 19 | 39 | 32 | We suggest adding the following sentence to the end of this paragraph: "Alternatively regional climate models could be used to understand and quantify the effect of cloud-aerosols interactions at much higher resolutions and use this information to validate and/or correct parameterizations in coarse grid global models (Bangert et al., 2011)." REFERENCE: Bangert, M., Kottmeier, C., Vogel, B., and Vogel, H.: Regional scale effects of the aerosol cloud interaction simulated with an online coupled comprehensive chemistry model, Atmos. Chem. Phys., 11, 4411-4423, doi:10.5194/acp-11-4411-2011, 2011. [Andrew Ferrone, Germany] | Changed. Reference included in discussion of regional work. |
| 7-1066 | 7 | 39 | 21 | | | There is also a paper by Wang et al 2011 in Geosci. Model Dev. On superparameterization. [Andrew Gettelman, USA] | Reference to Wang's ACP 2011 paper added. |
| 7-1067 | 7 | 39 | 23 | 39 | 32 | "The representation of clouds in large-scale models remains primitive." Please correct this statement. The GATOR-GCMOM model treats microsphysics of clouds explicitly and with size and composition resolution (e.g, Jacobson, M. Z., Development of mixed-phase clouds from multiple aerosol size distributions and the effect of the clouds on aerosol removal, J. Geophys. Res., 108 (D8), 4245, doi:10.1029/2002JD002691, 2003). In fact, the microphysics are the same as those used on the high-resolution (15-m) cloud scale. Please clarify. [Mark Z. Jacobson, U.S.A.] | Text not changed. Although the microphysics in these models is quite sophisticated, the fundamental problem is that the cloud scale motions are not resolved and therefore that the full benefit of the microphysics cannot be realised. |
| 7-1068 | 7 | 39 | 28 | 39 | 28 | Remove comma and replace "which" with "that". [Steven Ghan, USA] | Grammar appears sound. Not changed |
| 7-1069 | 7 | 39 | 29 | 39 | 29 | In line 29 after "(Quaas et al., 2009b)", suggest to add the following paragraph or a paragraph similar to the following paragraph:<br><br>Also, fine-scale modeling shows that cloud-scale interactions (not represented by large-scale models) among cloud microphysics and dynamics outweigh the effect of factors considered in the simple calculations or highly idealized models on the outcome of an aerosol perturbation (Lee and Penner, 2010). A comparison between fine-scale modeling and large-scale modeling shows that this leads to responses of clouds to the aerosol perturbation which are counterintuitive to responses expected from the simple calculations or highly idealized models (Lee and Penner, 2010). This demonstrates that the representation of the effects of the aerosol perturbation based on the simple calculations or idealized models in large-scale models can be misleading.<br><br>Reference:<br><br>Lee S. S., and J. E. Penner, 2010, Comparison of a global-climate model simulation to a cloud-system resolving model simulation for long-term thin stratocumulus clouds in a response to the transition from the preindustrial condition to the present-day condition, Atmos. Chem. Phys, 10, 6371-6389.<br>[Seoung Soo Lee, United States of America] | Not changed. Comment is not clear. |
| 7-1070 | 7 | 39 | 29 | 39 | 30 | About the expression: "However, by examining the interactions between cloud and large-scales in the global models,". The "large-scales" are not physical systems, so they cannot interact with aerosols. Please specify | The text has been revised. |

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| | | | | | | better "the large-scales". [Rubén D Piacentini, Argentina] | |
| 7-1071 | 7 | 39 | 36 | 39 | 41 | This might be a good place to mention the need for multi-platofrm campaigns aimed at reducing the uncertainties by making coincident measurements of as many of the factors involved as possible, at appropriate spatial and temporal scales. [Ralph Kahn, United States of America] | Not changed. Important point but this is more a question of recommendations for the future. |
| 7-1072 | 7 | 39 | 36 | 40 | 9 | These figures ignore the very different role of aerosols between day and night, which cannot rationally be averaged into a single figure and ought to be measured separately [VINCENT GRAY, NEW ZEALAND] | We could not ascertain which figures were being referred to and have not changed the text. |
| 7-1073 | 7 | 39 | 41 | 39 | | after "2011", add "Fan et al. 2012", Add reference: Fan, J., L. R. Leung, Z. Li, H. Morrison, et al. (2012), Aerosol impacts on clouds and precipitation in eastern China: Results from bin and bulk microphysics, J. Geophys. Res., 117, D00K36, doi:10.1029/2011JD016537. [Zhanqing Li, USA] | This section addresses techniques that combine modeling and observations rather than compare models and observations. Reference is not added. |
| 7-1074 | 7 | 39 | 41 | | | Section 7.4.2: This section could be shorter and cut to a brief description with key references and cut to the assessment. [Andrew Gettelman, USA] | Given the historical connection to AR4, we feel it important to add this. But changes have been made to clarify and shorten. |
| 7-1075 | 7 | 39 | 45 | | | As pre-amble - we are considering in Ch 8 the Twomey effect as a hypotheitcal isntantaneous forcing associated with the introduction of aerosol to clouds with all other factors fixed much in the same way as doubling co2is perceived . The key point is the Twomey effect is not the net forcing that ultimately is applied to the system. [Graeme Stephens, USA] | We now make it clear here too that the Twomey effect is a hypothetical construct. (End of 7.4.2.2) |
| 7-1076 | 7 | 39 | 47 | 39 | 54 | Curiously, the reason of not using at all the "1st" or "2nd" indirect forcing adopted in AR4 and prior reports seems not be clearly stated in here or other places. [Chien Wang, United States of America] | AR5 adopts new terminology and justifies why it has not kept to the old terminology with the aid of text (7.4.1.1) and figures. |
| 7-1077 | 7 | 39 | 52 | 39 | 53 | The recent paper by Brenguier et al. (2011, doi:10.5194/acp-11-9771-2011) does not find support for the Liu and Daum claim. This needs to be mentioned! [Jón Egill Kristjánsson, Norway] | As a result of an increase in aerosol, two responses have been identified: (i) is the broadening expected in the condensation growth regime (Liu and Daum); (ii) the narrowing in the collision-coalescence dominated regime (Feingold et al. 1997). The Brenguier result is perhaps not unexpected given that both processes are important. Reference to Brenguier is added and text changed to reflect uncertainty. |
| 7-1078 | 7 | 39 | 53 | | | “Important” is exaggerated. It would be good to quantify the potential share of this effect on the iRF. [Johannes Quaas, Germany] | Text changed to "may also play a role" |
| 7-1079 | 7 | 39 | 54 | 39 | 54 | At this point you might note the finding of a longwave indirect effect in thin arctic clouds, by Garrett Nature (2006) and Lubin Nature (2006). That was only for longwave at the surface, which is undoubtedly larger than at TOA. But it suggests potential for TOA effects involving cirrus. [Steven Ghan, USA] | Added reference to Garrett/Lubin since it is a liquid water effect. Distinguish as a longwave effect. |
| 7-1080 | 7 | 40 | 3 | 40 | 3 | Insert Hudson et al. (2009)<br>Hudson, J.G., S. Noble, V. Jha, and S. Mishra, 2009: Correlations of small cumuli droplet and drizzle drop concentrations with cloud condensation nuclei concentrations, J. Geophys. Res., 114, D05201, doi:10.1029/2008JD010581. [James Hudson, USA] | Reference not added because the work does not address cloud drop closure, but rather correlation between CCN and cloud microphysics. |
| 7-1081 | 7 | 40 | 4 | 40 | 4 | Replace "which" with "that". [Steven Ghan, USA] | changed |
| 7-1082 | 7 | 40 | 13 | 40 | 14 | "There is ample observational evidence for increases in aerosol resulting in an increase in drop concentration and decrease in drop size". This statement applies only to low aerosol optical depths. Satellite data indicate that, as aerosol optical depth increases beyond about 0.2-0.3, a further increase in aerosol optical depth decreases cloud optical depth because cloud absorption effects and semi direct effects become more important ( Ten Hoeve, J.E., L.A. Remer, and M.Z. Jacobson, Microphysical and radiative effects of aerosols on warm clouds during the Amazon biomass burning season as observed by MODIS: impacts of water vapor and land cover, Atmos. Chem. Phys. 11, 3021-3036, 2011.). [Mark Z. Jacobson, U.S.A.] | Added "at low aerosol optical depths" . Higher AOD regime is now also mentioned. |
| 7-1083 | 7 | 40 | 13 | 40 | 15 | Another main question is under what conditions does the liquid water path actually remain constant. This | We now clarify that Twomey's idea is a hypothetical |

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| | | | | | | bears upon the relationship between satellite-derived and in situ measurements of the cloud microphysical response to diffeences in aerosol concentration. [Ralph Kahn, United States of America] | construct. It was not his intent to state that LWP remains constant, but rather a hypothetical "if"…... "then" |
| 7-1084 | 7 | 40 | 14 | | | This isnt the main question at all but rather do aerosol influences lead to a change in cloud albedo - this is quite different from the question posed and surprisingly the observations show that while cloud droplets change in size the albedo doesn't significantly - this is described well in Lebsock et al 2008 (JGR) and also Christensen and Stephens (2011; JGR ) and it expalins why inferrred estimates of the indirect effect from observations are much smaller than applied in models. This ought to be emphasized more. [Graeme Stephens, USA] | Text has been changed to clarify that we are addressing underlying microphysical aspect (to what extent does N increase with increasing aerosol) and not the adjustments. Work of Christensen and Lebsock is included in AFaci discussion. |
| 7-1085 | 7 | 40 | 16 | | | also references in comment 16. [Warren Richard Leaitch, Canada] | Reference to Hegg et al. 2012 added in 7.4.2.2 |
| 7-1086 | 7 | 40 | 17 | 40 | 17 | Michael is a first name of Dr. M. Schulz. Revise a references also. [Toshihiko Takemura, Japan] | Changed |
| 7-1087 | 7 | 40 | 19 | 40 | 20 | "impossible to observe" sounds rather drastic. How about ship tracks? Or, do you mean a clean, theoretical Twomey effect (absolutely no change in LWP)? Maybe, that needs to be made clearer. [Jón Egill Kristjánsson, Norway] | Text clarified. Twomey is a hypothetical construct (unchanged LWP). LWP changes in shiptracks. |
| 7-1088 | 7 | 40 | 19 | 40 | 20 | This statement is way too strong. [Daniel Murphy, United States of America] | Text adjusted to point out that Twomey is a hypothetical construct |
| 7-1089 | 7 | 40 | 20 | | | and therefore it isnt the net forcing applied to the system. [Graeme Stephens, USA] | Text changed |
| 7-1090 | 7 | 40 | 22 | 42 | 30 | The results of Lebsock et al are global in nature and those of Chrtistensen and Stephens (2011) apply to ship tracks observed over large portions of the oceans - both show a distinct liquid water adjustment offsetting particle size adjustments and Chriestensen and Stephens (2011) show clearly how this adjustment occurs in closed cellular convection with minal changes in albedo in the net. The behaviour in open cellular convection is quite different with mesoscale cirlculations enhancing the water content and albedo. Christensen and Stephens (2012, JGR, to appear) discusses the effects on drizzle. The bottom line is the liquid water adjustments occur to offset albedo changes asociated with particle size changes and the net inferred effect is small and mostly in open cellular convection. [Graeme Stephens, USA] | This work is now refered to in 7.4.3.2 (adjustments) |
| 7-1091 | 7 | 40 | 26 | 40 | 26 | 80 nm is way too high. This size would only apply in very polluted environments where cloud supersaturations would be lower and particles would be less soluble and thus larger for the same critical supersaturation. In less than extremely polluted envronments cloud supersaturations usually exceed 1%, which for mostly soluble particles implies dry particle sizes as small as 20 nm for NaCl or 28 nm for ammonium sulfate. Less soluble particles would be larger but few would be as large as 80 nm for 1% supersaturation. Hudson et al. (2010) showed that even marine stratus clouds can often have supersaturations exceeding 1% in cleaner air masses. More convective clouds have even higher supersaturations. The community has been overestimating CCN sizes and this is an extreme example. Hudson, J.G., S. Noble and V. Jha, 2010: Stratus cloud supersaturations. Geophys. Res. Lett., 37, L21813, doi:10.1029/2010GL045197. [James Hudson, USA] | We have changed this value to 60 nm. It depends on the vigour of the clouds and the aerosol concentration. |
| 7-1092 | 7 | 40 | 26 | | | and relative humidity. Instead of "updraft velocity", "cooling rate" is more appropriate. [Johannes Quaas, Germany] | Addded cooling rate. RH isn't the issue since we're talking about activation and RH > 100%. |
| 7-1093 | 7 | 40 | 27 | 40 | 28 | Aerosol compositon is unimportant to CDNC? So if I lift 2 parcels of air, one with all sulfate aerosol and the other with the same concentration of pure mineral dust or pure BC, at the same velocity I get the same CDNC? What am I missing here? [Anthony Del Genio, USA] | Numerous papers have shown relatively modest (order 15%) changes to Nd because of the self-regulating aspect of the supersaturation field; Dust and BC are rarely pure, except very close (few kms) to sources. |
| 7-1094 | 7 | 40 | 27 | 40 | 28 | Isn't this still controversial, especially for organic aerosol? If so, might it be better to say: "…whereas aerosol composition is less important, except…" [Ralph Kahn, United States of America] | The Ervens et al. 2005 paper looks at organic aerosol. Text has been modified to clarify the relative unimportance of organics |

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| 7-1095 | 7 | 40 | 27 | 40 | 28 | Is it really generally true that "aerosol composition is unimportant"? Clearly, hygroscopicity is important, and that is related to composition. [Jón Egill Kristjánsson, Norway] | Numerous papers have shown relatively modest (order 15%) changes to Nd because of the self-regulating aspect of the supersaturation field. The text has been changed to clarify |
| 7-1096 | 7 | 40 | 27 | 40 | 28 | Is it more adequate to say "when aerosol composition is given" or "with known aerosol composition" than "aerosol composition is unimportant"? [Chien Wang, United  States of America] | Text changed to clarify.This is a relative comparison of the importance of various parameters that can influence drop concentration. Composition is unimportant relative to parameters like particle number, particle size and updraft velocity. |
| 7-1097 | 7 | 40 | 27 | 40 | 29 | No [James Hudson, USA] | There is a big difference between the importance of aerosol composition for CCN vs. its importance for drop concentration. Composational effects will be diminished by dynamical responses of supersaturation. Text has been adjusted. |
| 7-1098 | 7 | 40 | 28 | | | change to "...composition is relatively unimportant…" [Meinrat O. Andreae, Germany] | Text has been modified |
| 7-1099 | 7 | 40 | 29 | 40 | 32 | A reference is needed for the sentence "Simple arguments show ….". How large changes in liquid water path and drop concentration are assumed? Are these assumptions compatible? [Jón Egill Kristjánsson, Norway] | Reference added to Boers and Mitchell (1994). Albedo ~ tau ~ LWP^(5/6) N^(1/3) |
| 7-1100 | 7 | 40 | 30 | 40 | 30 | Add 'aerosol' between indirect and effect. [Gunnar Myhre, Norway] | Changed to "there is no RFaci.." |
| 7-1101 | 7 | 40 | 31 | | | "two and a half times more sensitive". Is this in a logarithmic sense, i.e., per percent change in the indicated variable? Otherwise makes no sense; specify.  [Stephen E Schwartz, USA] | Changed to clarify. |
| 7-1102 | 7 | 40 | 32 | 40 | 32 | Need a reference to the preceding sentence about 2.5 times more sensitive [Daniel Murphy, United  States of America] | Added reference (e.g., Boers and Mitchell, 1994). |
| 7-1103 | 7 | 40 | 32 | 40 | 34 | The indirect effect may well be dominated by dynamics, but surely aerosols become an important (if not dominant) factor for the indirect forcing, which is the subject of that section. [Nicolas Bellouin, United Kingdom] | I believe that the sentence reflects our state of understanding |
| 7-1104 | 7 | 40 | 32 | | | This needs a Reference? [Andrew Gettelman, USA] | Boers and Mitchell (1994) reference added. |
| 7-1105 | 7 | 40 | 33 | 40 | 33 | Rather than convective strength you might more generally say turbulence strength. [Anthony Del Genio, USA] | Changed as suggested |
| 7-1106 | 7 | 40 | 33 | | | Assuming that the argument here is that the cloud distributions determine the iRF, the term “forcing” in “dynamical forcing” is highly misleading in this context, since it is not a forcing. If this is the argument, it is equally important to mention that the solar zenith angle is decisive for the iRF. Less polemically, surface albedo and shielding effect by overlying clouds are relevant as well. Overall, my suggestion would be to more precisely develop this statement. [Johannes Quaas, Germany] | Changed as requested. Remove word "forcing" since it is confusing. Concentrate on the physical parameters that determine the microphysical response (as opposed to the RFaci) |
| 7-1107 | 7 | 40 | 34 | | | remove "perhaps". Size distribution is always a key factor. [Meinrat O. Andreae, Germany] | Changed. |
| 7-1108 | 7 | 40 | 44 | | | You need reference the studies of van Den Heever 2011 (JAS) and a followup (2012 that I can get to LAs) - these reveal important insights on  how aerosol influences the state of radiative convective equlibrium as derived from cloud process models run to equilibrium - these studies are much more insightful that aerosl cloud case study numerical experiments that say little about an equlilibrium state and further offer much more insight than GCM studies that do not have resolved convection and less advanced microphysics.    [Graeme Stephens, USA] | We now refer to the work of van den Heever (7.4.1.3) and other larger scale, longer term studies. Section 7.6 now includes discussion of aerosol effects on precipitation in RCE. |
| 7-1109 | 7 | 40 | 47 | 40 | 48 | You might add something like: "… difficulties in representing clouds, aerosol size and concentration, and aerosol-cloud interactions…" [Ralph Kahn, United  States of America] | changed |
| 7-1110 | 7 | 40 | 48 | 40 | 49 | "satellite observations" should be "satellite remote sensing data"? [Chien Wang, United  States of America] | Changed to "satellite-based" |
| 7-1111 | 7 | 40 | 48 | 40 | 55 | It is worth to mention Penner et al. (2011) here about another issue with the satellite approach. Penner et al. | Text added to address this. |

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| | | | | | | (2011) explores some issues with the satellite approach and found that the cloud-aerosol relationship based on the present day simualtion may not be a robust measure of the aerosol indirect forcing calcualted based on the present day and preindustrial simulations, and shows that the apporach used in Quaas et al. (2008) can substantially underestiamte the first aerosol indirect forcing. (Penner, J. E., Xu, L., and Wang, M. H.: Satellite methods underestimate indirect climate forcing by aerosols, Proc. Natl. Acad. Sci. U.S.A., 108, 13404-13408, 10.1073/Pnas.1018526108, 2011). [Minghuai Wang, United States of America] | |
| 7-1112 | 7 | 40 | 49 | 40 | 49 | Use 'weaker' instead of 'smaller' since the RF is negative [Gunnar Myhre, Norway] | Changed |
| 7-1113 | 7 | 40 | 52 | 40 | 52 | Improve readability by replacing "as compared with" by "than" [Jón Egill Kristjánsson, Norway] | Changed |
| 7-1114 | 7 | 40 | 55 | 41 | 1 | a fuller reference list can help bolster this claim, e.g., Mauger and Norris, 2009; Painemal and Zuidema, 2010, ACP. [Paquita Zuidema, USA] | Refs added |
| 7-1115 | 7 | 40 | 56 | 40 | 56 | Replace 'fast feedback' by 'rapid response' [Gunnar Myhre, Norway] | Changed |
| 7-1116 | 7 | 41 | 2 | | | Is the Bottom line summar that the satellite estimates may be too low? Or at least potential biases with models are lower. Summarize [Andrew Gettelman, USA] | TBD: GF/Needs clarification. Results using satellite observations may not be too low but they may be right for the wrong reasons. The microphysical responses based on e.g., Breon et al. are too weak if they are intended to reflect activation and Twomey. They may be more reflective of the multitude of aerosol-cloud interactions that occur during the course of a cloud lifetime. I suggest that we say something along these lines in 7.5 |
| 7-1117 | 7 | 41 | 6 | | | Section 7.4.3.1: I am not sure it makes sense in a climate assessment to separate Albedo and Lifetime effects, since we can't in global estimates. [Andrew Gettelman, USA] | Here we are looking at AFaci which includes RFaci, so we are in fact not separating albedo and lifetime effects. |
| 7-1118 | 7 | 41 | 18 | 40 | 39 | The text continues the questionable practice of explaining the cloud lifetime effect in terms of cloud amount. The mechanism is more compelling when expressed in terms of cloud liquid water content. If precipitation is initiated when droplet radius exceeds a threshold size (admittedly a simplification), increasing droplet number allows clouds of higher liquid water content to persist. Precipitation obviously reduces cloud liquid water, but not necessarily cloud amount. [Steven Ghan, USA] | Water content added. To some extent this is semantics. "cloud amount" isn't a well-defined geophysical quantity but at least conveys some integral of liquid water and fraction. We also want to get away from the simplication that more aerosols manifest itself in more water as there as ample examples of more aerosols leading to less cloud water. The cloud lifetime effect will be folded under one of the many possible cloud adjustments to aerosols. |
| 7-1119 | 7 | 41 | 24 | 41 | 27 | "Some modelling studies suggest the opposite, wherein increased aerosol concentrations actually promote the development of deeper clouds, thereby invigorating precipitation (Rosenfeld et al., 2008; Stevens and Seifert, 2008…' The phenomenon that aerosols reduce precipitation locally and upwind of mountains but increase precipitation downwind was also shown by cause and effect in numerical simulation in Pargraph 52 of Jacobson, M.Z., Y.J. Kaufmann, Y. Rudich, Examining feedbacks of aerosols to urban climate with a model that treats 3-D clouds with aerosol inclusions, J. Geophys. Res., 112, D24205, doi:10.1029/2007JD008922, 2007. Nevertheless, the overall effect of aerosols was to cause a net precipitation decrease (this should occur physically since aerosols enhance stability, reducing surface winds and evaporation (Jacobson, M.Z., and Y.J. Kaufmann, Aerosol reduction of the wind, Geophys. Res. Lett., 33, L24814, doi:10.1029/2006GL027838, 2006), and precipitation equals evaporation in equilibrium. [Mark Z. Jacobson, U.S.A.] | Orographic clouds are now discussed separately in 7.6 since the interesting results tend to be assciated with mixed-phase precipitation. |
| 7-1120 | 7 | 41 | 25 | 41 | 26 | Might be: "… development of deepter clouds under some circumstances, thereby invigorating…" [Ralph Kahn, United States of America] | Text has been changed. Deep convective clouds are now discussed in 7.6 |
| 7-1121 | 7 | 41 | 25 | 41 | 26 | "increased aerosol concentrations actually promote the development of deeper clouds, …. (Rosenfeld et al., 2008, …". The problem with this line of reasoning is that in the Rosenfeld study, this works via the ice phase, | reviewer is correct. Text has been changed. Deep convective clouds are now discussed in 7.6 |

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| | | | | | | which is supposedly not considered in this subsection (7.4.3 Adjustment in Liquid Clouds). [Jón Egill Kristjánsson, Norway] | |
| 7-1122 | 7 | 41 | 27 | 41 | | after "section 7.4.5)." Add "Using long-term observation data, Li et al. (2011b) confirmed that aerosols tend to increase the frequence of occurence deep mixed-phase clouds, but decrease it for shallow water clouds." Reference: Li, Z., F. Niu, J. Fan, Y. Liu, and D. Rosenfeld, Y. Ding (2011b), The long-term impacts of aerosols on the vertical development of clouds and precipitation, Nature-Geoscience, doi: 10.1038/NGEO1313. [Zhanqing Li, USA] | Deep convective clouds are now discussed in 7.6 |
| 7-1123 | 7 | 41 | 30 | 41 | 31 | Please specify to which extent this limitations applies to CMIP5 models used in the present assessment. [Andrew Ferrone, Germany] | TBD: Figure caption or section 7.5 will mention how many GCMs treat AFaci effects |
| 7-1124 | 7 | 41 | 30 | 41 | 31 | Some references are needed in support of the claim that "Many climate models assume such an effect a priori". What climate models? [Jón Egill Kristjánsson, Norway] | TBD: see above |
| 7-1125 | 7 | 41 | 33 | 41 | 34 | Lee et al. (2008) shows that the sign of aerosol effects on cloud amount (or liquid-water path) does not vary with cloud regimes; for stratocumulus clouds and all type of convective clouds, cloud amount increases with increasing aerosol, though the percentage increase in cloud amount varies with varying cloud regime (defined by cloud-top height).<br><br>Reference:<br><br>Lee, S. S., L. J. Donner, V T J Phillips, and Y. Ming, 2008: The dependence of aerosol effects on clouds and precipitation on cloud-system organization, shear and stability. J. Geophys. Res., 113, D16202, doi:10.1029/2007JD009224<br>[Seoung Soo Lee, United States of America] | Not changed. Would be hard to conclude that this statement is true based on one model study when so many studies show conflicting responses. |
| 7-1126 | 7 | 41 | 34 | 41 | 34 | maybe, need to more specific about "locally" what scale "locally" indicates? what defines cloud regime? [Seoung Soo Lee, United States of America] | Not changed, locally is sufficiently precise |
| 7-1127 | 7 | 41 | 37 | 41 | 39 | About the sentence: "Then again, because diverse effects offer the possibility of compensating one another, at least globally, it seems possible that lifetime effects may be considerably less important than previously thought (Stevens and Feingold, 2009"). Please change the expression "lifetimes effects", for in this same page 41, line 10 it is said: "....and in the past have been referred to as 'lifetime' effects. However this nomenclature is misleading". [Rubén D Piacentini, Argentina] | Changed |
| 7-1128 | 7 | 41 | 38 | 41 | 39 | I realize the current large-scale models might err on the side of an exaggerated "lifetime" effect; however, given the difficulty of constraining this effect observationally, is the sign of the discrepancy (i.e., smaller effect) that well established? [This is just a question -- I don't have an opinion on the answer here, only some idea of the difficulty in making the critical measurements.] [Ralph Kahn, United States of America] | The balance of evidence from theory (Kostinksi) models (Stevens and Seifert (2008), Wang and Feingold 2009) and obs(Brenguier, Stevens and colleagues) indicates much weaker dependence of autoconversion on N than is often assumed in GCMs. The observations show N^(-0.5) to N^(-1), and not N^(-1.79) as GCMs tend to assume. The fine-scale models suggest N^(-0.5) to N^(-2/3). Theory suggests N^(-2/3). |
| 7-1129 | 7 | 41 | 41 | 41 | 55 | Regional evidence for aerosols affecting clouds, see also: Bennartz, R., J. Fan, J. Rausch, R. Leung, A. Heidinger, 2011: Pollution from China increases cloud droplet number, suppresses rain over the East China Sea. Geophys. Res. Lett., 38, L09704, doi:10.1029/2011GL047235. [Bennartz Ralf, US] | On balance we decided not to add this reference. The problem is that changes in meteorology over the observation period were not controlled for. The model simulations with fixed meteorology are suggestive but not compelling. |
| 7-1130 | 7 | 41 | 41 | | | What I understand from the text about the "adjustment" is more or less "response". [Chien Wang, United States of America] | yes.. |
| 7-1131 | 7 | 41 | 43 | 41 | 49 | The omission of the Coakley and Walsh (2002) study on shiptracks showing that LWP is typically reduced in shiptracks is an oversight, especially since this also gives the most incontrovertible evidence that LWP can | Added reference |

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| | | | | | | decrease as well as increase in some clouds. [Robert Wood, USA] | |
| 7-1132 | 7 | 41 | 49 | 41 | 49 | As you reference Durkee et al. 2000, you might also reference the classic on this topic: Coakley, J. A., Jr., R. L. Bernstein, and P. A. Durkee, 1987: Effect of ship-stack effluents on cloud reflectivity. Science, 237, 1020–1022. [Ralph Kahn, United States of America] | We have added Coakley and Walsh instead to make the point that LWP can have different signs of response. |
| 7-1133 | 7 | 41 | 50 | 41 | 50 | About the expression (with unit):"..satellite and found to be insignificant at 0.5 m W m–2". The unit milliWatt must be given as a one word: mW. [Rubén D Piacentini, Argentina] | Changed |
| 7-1134 | 7 | 41 | 51 | | | Investigating satellite data up- and downwind of ship routes, no statistically significant large-scale effect on cloud micro- or macrophysics, and subsequently no effect on the radiation budget, can be isolated from the background cloud variability (Peters et al., A search for large-scale effects of ship emissions on clouds and radiation in satellite data, J. Geophys. Res., doi 10.1029/2011JD016531, 2011.) [Johannes Quaas, Germany] | Text added |
| 7-1135 | 7 | 42 | 3 | 42 | 9 | This is harder to follow for the uninitiated than it need be. Readers will need to hear what closed- and open-cell convection look like, then be told about POCS (pockets of open cells), then be told that the evidence suggests that precip plays a role. I suspect the paragraph can be significantly shortened while make the point that aerosol concentrations can affect radiative forcing through precipitation. [Robert Pincus, USA] | Text has been rewritten and simplified. |
| 7-1136 | 7 | 42 | 10 | 42 | 16 | The VOCALS experience is relevant here (and will undoubtedly be incorporated within the 2nd draft). It is worth noting that precipitation rates can be similar in open- and closed-cell environments (Wood et al., 2011b, ACP) and that mesoscale free-tropospheric subsidence is associated with open-cells by Berner et al. (2011, ACP). [Paquita Zuidema, USA] | Changed to include observation that precipitation rates can be similar in open and closed-cell environments. |
| 7-1137 | 7 | 42 | 11 | 42 | 12 | I think the "Wang and Feingold (2009)" here refers to " Wang, H., and G. Feingold, 2009a: Modeling open cellular structures and drizzle in marine stratocumulus. Part I: Impact of drizzle on the formation and evolution of open cells. J. Atmos. Sci., 66, 3237-3256.", which is missing from the reference list (page 95 line 41). The existing " Wang, H., and G. Feingold, 2009: Modeling open cellular structures and drizzle in marine stratocumulus. Part II: The microphysics and dynamics of the boundary region between open and closed cells. J. Atmos. Sci., 66, 3257-3275." should be " Wang, H., and Feingold, 2009b: ...." [Hailong Wang, USA] | Will check and modify references |
| 7-1138 | 7 | 42 | 18 | 42 | 26 | What is the area of these regions. Is the RF or aerosol perturbation to these regions globally significant? Are there any estimates? [Andrew Gettelman, USA] | Good question. No one has looked at this to our knowledge. Would require satellite analysis. And, open cells are not necessarily due to drizzle; they exist over warmer oceans. I don't believe we have data to answer this question. |
| 7-1139 | 7 | 42 | 21 | 42 | 26 | Added CCN were shown to be responsible for preventing the opening of closed cells. This is shown in Wang et al. (2011) (already in the reference list). Therefore, the possibility of high CCN maintaining the cells closed should be explicitly stated here. [Meinrat O. Andreae, Germany] | Text changed. Reference to Goren and Rosenfeld and Wang et al 2011 added. |
| 7-1140 | 7 | 42 | 31 | 42 | 34 | Wind shear is also observed to both limit the further development of precipitating trade-wind cumulus and increase cloud fraction (e.g., Zuidema et al., 2012, JAS). [Paquita Zuidema, USA] | Reference added |
| 7-1141 | 7 | 42 | 33 | 42 | 33 | Replace "which" with "that". [Steven Ghan, USA] | Changed |
| 7-1142 | 7 | 42 | 34 | | | change "cumulus" to "cumuli", or "tend" to "tends" [Hailong Wang, USA] | Changed |
| 7-1143 | 7 | 42 | 34 | | | Need references for that statement ('because precipitating trade cumulus tend to regenerate through colliding outflows'). [Minghuai Wang, United States of America] | Added reference to Zuidema et al. 2012 |
| 7-1144 | 7 | 42 | 40 | 42 | 42 | the statement that simple models of the autoconversion process scaling with the square of the inverse of droplet number concentration needs a reference. Parameterizations use exponent values ranging from -0.3 to -3.3 (e.g., Tripoli and Cotton, 1980; Beheng (1994)). [Paquita Zuidema, USA] | Added reference to Khairotdinov and Kogan 2000 |
| 7-1145 | 7 | 42 | 40 | 42 | 51 | Three scaling relations between rain rate and drop number are presented in eleven lines. Is this “progress in process-level understanding” or does it reflect substantial uncertainty? [Robert Pincus, USA] | Text changed to clarify that smaller dependency on N is more likely than larger one |
| 7-1146 | 7 | 42 | 41 | 42 | 41 | Delete the first "process" occurrence. [Anthony Del Genio, USA] | changed |

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| 7-1147 | 7 | 42 | 46 | 42 | 47 | Accrtion may be insensitive to droplet number but certainly not to droplet size, which is inherently dependent on CCN concentrations. [James Hudson, USA] | Not changed. Accretion is dependent on prodiuct of cloud and rain liquid water contents. See e.g., Berry papers from the early 70s. |
| 7-1148 | 7 | 42 | 46 | 42 | 47 | "cloud drops by rain drops" to "smaller droplets by the larger drops"? Also, are there papersr arguing "…insensitive to droplet concentration" that could be cited? What is the "droplet" referred to, cloud droplets? Perhaps the category of hydrometeors by size ought to be introduced somewhere? [Chien Wang, United States of America] | Not changed. This can be found in refs to Berry and Berry and Reinhardt (late 60s, early 70s). This is also covered in Khairoutdinov and Kogan (2000) which is now referred to. |
| 7-1149 | 7 | 42 | 49 | 43 | 5 | In addition to LWP, rain is controlled by cloud top effective radius. Using Tropical Rainfall Measuring Mission satellite, it was shown that detectable rain rate (~0.7 mm/hr) does not occur for cloud top effective radius of 14 um (Rosenfeld, Science 2000). Aircraft measurements showed the same for marine stratocumulus (Gerber et al., JAS 1996, 53 (12), 1649–1662) and in continental convective clouds (Freud and Rosenfeld., JGR 2012, doi 10.1029/2011JD016457). Therefore, CCN can suppress rain by decreasing cloud top effective radius below ~14 um. The total suppression of rain was also evident in the simulations of Wang et al., 2011 (already in the reference list). [Meinrat O. Andreae, Germany] | Reference to Reff thresholds added (Rosenfeld 2000) |
| 7-1150 | 7 | 42 | 52 | 40 | 53 | Clouds alone without precipitation considerably alter CCN. Coalescence and coagulation within clouds reduces CCN concentrations. This would also result in larger CCN, i.e., lower critical supersaturations. The numbers of CCN determine droplet concentrations. [James Hudson, USA] | Not changed. This is an important cloud processing issue (coalescence scavenging). But it's not directly relevant here. We have, however added more discussion on cloud processing in 7.4.1.2 |
| 7-1151 | 7 | 43 | 1 | 42 | 4 | Also useful to mention Stevens and Seifert (2008) find precipitating trade-wind cumulus clouds with lower aerosol concentrations deepen less here (otherwise connection to sentence on page 41, lines 25-26 is confusing), and that the semi-direct effect is not considered within the sentence. [Paquita Zuidema, USA] | Clarified that we are concerned here with non absorbing aerosol. Have not inclued discussion of the "deepening effect" since this is a different theme and has been touched on in various other places in 7.4. At it's core it is a "Pincus and Baker" effect. But it also relates to buffering (more aerosol, less drizzle, more cloud top evaporation, deeper clouds, greater LWP, more drizzle). |
| 7-1152 | 7 | 43 | 1 | 43 | 5 | We suggest adding the following sentence to the end of this paragraph: "Regional climate models allow the representation of the interactions between aerosols and clouds in a much more detailed way, and can help to better link the results of small-scale models to mesoscale processes (Bangert et al, 2011). REFERENCE: Bangert, M., Kottmeier, C., Vogel, B., and Vogel, H.: Regional scale effects of the aerosol cloud interaction simulated with an online coupled comprehensive chemistry model, Atmos. Chem. Phys., 11, 4411-4423, doi:10.5194/acp-11-4411-2011, 2011. [Andrew Ferrone, Germany] | Short section on regional scale modelling is added with reference to Bangert et al, Seifert et al ACP and others. See also Section 7.4.1.3. |
| 7-1153 | 7 | 43 | 5 | 43 | 5 | Might be: "… is not currently feasible in GCMs." [Ralph Kahn, United States of America] | Changed. wont be foreseabble in forseeable future.. |
| 7-1154 | 7 | 43 | 10 | 43 | 12 | Line 10-12: There are studies showing changes in cloud morphology induced by aerosol in warm cumulus and deep convective clouds such as Jiang et al. (2009) and Lee (2011) in addition to Wang and Feingold (2009) for stratocumulus clouds. These studies for convective clouds discuss about aerosol-induced changes in cloud size, size distribution, cloud depth and cloud population.<br><br>Reference:<br><br>Lee, S. S., 2011, Dependence of aerosol-precipitation interactions on humidity in a multiple-cloud system, Atmos. Chem. Phys., 11, 2179-2196.<br><br>Jiang, H., Feingold G., and I. Koren, 2009: Effect of aerosol on cumulus cloud morphology, J. Geophys. Res., 114, D11209, doi:10.1029/2009JD011750.<br>[Seoung Soo Lee, United States of America] | Possibly add reference to Lee in 7.6 [for consideration. The issue is that when aerosol only fills part of the domain it may generate circulation changes in deep convective patterns. The relevant reference is actually Lee, JAS 2012] |
| 7-1155 | 7 | 43 | 12 | 43 | 12 | Might be: "…underscore the limitations imposed by applying simplistic rules…" Not that I favor simplistic rules, but just to acknowledge that there are *some* serious practical limitations for the larger-scale models, at least at present. Alternatively, you might say something about the folly of drawing strong conclusions based upon | Wording maintained in the interests of clarity and an unambiguous message. |

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| | | | | | | simplistic rules... [Ralph Kahn, United States of America] | |
| 7-1156 | 7 | 43 | 12 | 43 | 13 | The choice of word is unhelpful. The alternative used to be simple parameterisations versus none at all. Why would the latter be more justified? [Nicolas Bellouin, United Kingdom] | Wording maintained in the interests of clarity and an unambiguous message. We cannot apply rules like "aerosol always increases cloud fraction and lifetime" when our knowledge of the system suggests that it doesn't. |
| 7-1157 | 7 | 43 | 12 | | | “Folly” is pretty strong language. [Robert Pincus, USA] | Wording maintained in the interests of clarity and an unambiguous message. |
| 7-1158 | 7 | 43 | 12 | | | "underscore the folly of applying simplistic rules". Nicely written. [Stephen E Schwartz, USA] | Thank you. Some seem less sanguine about this message. |
| 7-1159 | 7 | 43 | 12 | | | change "2009" to "2009b"; see #13 comment [Hailong Wang, USA] | Changed |
| 7-1160 | 7 | 43 | 13 | | | In a similar line of arguments, Sandu et al. (2008) have shown that when continuing an small-scale simulations for a longer time than often done, the response of a simulated cloud to the aerosol perturbation can be inversed (Sandu, I., J.-L. Brenguier, O. Geoffroy, O. Thouron and V. Masson, "Aerosols impacts on the diurnal cycleof marine stratocumulus ", Journal of the Atmospheric Sciences, 65, 2705-2718, 2008). [Johannes Quaas, Germany] | included reference to Sandu in 7.4.3.1 where it is more relevant. |
| 7-1161 | 7 | 43 | 17 | 43 | 17 | About the expression: "... Attempts to quantify cloud-mediated aerosol effects using global models suggest that lifetime effects". Same comment as before about the use of "lifetime effects". [Rubén D Piacentini, Argentina] | Changed to AFaci |
| 7-1162 | 7 | 43 | 23 | 43 | 24 | "cloud-mediated aerosol effects" and "cloud lifetime effects" should be further explained. Again, consistency in terms with other parts of the report and also with previous reports should be considered as well. [Chien Wang, United States of America] | Changed to the new terminology (RFaci and AFaci) |
| 7-1163 | 7 | 43 | 24 | 43 | 24 | About the expression: "... extension weaker cloud lifetime effects (Quaas et al., 2009b)". Same comment as before about the use of "lifetime effects". [Rubén D Piacentini, Argentina] | Changed |
| 7-1164 | 7 | 43 | 26 | 43 | 33 | I feel the first sentence of this paragrah is not that related to the remaing part of that paragah. The first sentence seems focus on shallow marinetime, but then the second sentence is about the aerosol effects on deep convective clouds [Minghuai Wang, United States of America] | Not changed. Both shallow and deep convection suffer from a lack of representation of cloud-scale motions. |
| 7-1165 | 7 | 43 | 27 | 43 | 30 | The PNNL-MMF (Wang et al., 2011, ACP) can explicitly simualtes aerosol effects on deep convective clouds and represent a major advancement in the field. So it is worth mention the PNNL-MMF here. [Minghuai Wang, United States of America] | Subject now treated in 7.6 |
| 7-1166 | 7 | 43 | 28 | 43 | 28 | There is a paper (Suzuki et al., 2008, GRL, 35, L19817, doi:10.1029/2008GL035449) to compare satellite-observed and NICAM 7km global simulated aerosol-cloud interaction signatures without cumulus parameterization. The results show that three type cloud systems (stratus, stratocumulus, cumulus) show similar RE slope and LWP slope against CCN number. [Teruyuki Nakajima, Japan] | Reference added |
| 7-1167 | 7 | 43 | 28 | | | It would be useful to inform the reader that most GCMs neglect the radiative effect of convective clouds entirely, not just its perturbation by anthropogenic aerosol. [Johannes Quaas, Germany] | Changed. |
| 7-1168 | 7 | 43 | 29 | 43 | 29 | "Recent efforts to consistently address both types of cloud representations…" This was done first not in Lohmann (2008) but in Jacobson, M. Z., Development of mixed-phase clouds from multiple aerosol size distributions and the effect of the clouds on aerosol removal, J. Geophys. Res., 108 (D8), 4245, doi:10.1029/2002JD002691, 2003 and 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, which used those algorithms. [Mark Z. Jacobson, U.S.A.] | Reference added |
| 7-1169 | 7 | 43 | 30 | 43 | 33 | Here might be a place to mention critical measurement suites needed to advance the field. [Ralph Kahn, United States of America] | Not changed. This is a good point but not the place to raise it. |

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| 7-1170 | 7 | 43 | 33 | 43 | 33 | I don't think that there is a controversy per se. It is more a lack of confidence in the model estimates, as they may not adequately represent the actual mechanisms. [Nicolas Bellouin, United Kingdom] | controversial changed to "uncertain" |
| 7-1171 | 7 | 43 | 46 | 43 | 48 | In cirrus clouds associated with deep convection, it is likely that the homogeneous freezing is not that sensitive to IN variation, since cloud droplet number concentration (CDNC) is predominantly much larger than IN concentration so that the general IN variation is just very small portion of CDNC (Lee et al. (2009))<br><br>Reference:<br><br>Lee, S. S., L. J. Donner, and V T J Phillips, 2009: Sensitivity of aerosol and cloud effects on radiation to cloud types: comparison between deep convective clouds and warm stratiform clouds over one-day period. Atmos. Chem. Phys., 9, 2555-2575.<br>[Seoung Soo Lee, United States of America] | Text changed to clarify message.. |
| 7-1172 | 7 | 43 | 47 | 43 | 47 | Both homogeneous and heterogeneous freezing are important in cirrus. Maybe just delete the statement rather than put more caveats in. [Daniel Murphy, United States of America] | Sentence erased as suggested. |
| 7-1173 | 7 | 43 | 47 | 43 | 48 | There is a good possibility that the "condensation-freezing" heterogeneous nucleation mode might be in play. Also, homogeneous freezing of what particles need to be clearly stated. [Chien Wang, United States of America] | Sentence has been removed |
| 7-1174 | 7 | 43 | 48 | 43 | 50 | Connection between these sentences and previous ones is loose. [Seoung Soo Lee, United States of America] | Now that the previous sentence has been removed this should be clearer. |
| 7-1175 | 7 | 43 | 53 | | | In the interests of communicating with a broader audience it is worth replacing “enthalpy” with a less technical term, even if this means marginally less accuracy. [Robert Pincus, USA] | Do you want to relent? |
| 7-1176 | 7 | 44 | 1 | 44 | 1 | I think “Bergeron-Wegener-Findeisen” should be changed to “Wegener-Bergeron-Findeisen (WBF)” based on the time order of the references, i.e., Wegener, 1911; Bergeron, 1935; Findeisen, 1938. Also it was the way people used in many literature studies. This should be changed througout the chapter since it was used at a few other places. [Jiwen Fan, United States of America] | Changed |
| 7-1177 | 7 | 44 | 1 | 44 | 1 | Hudson et al. (2010) demonstrated this process by finding that droplet concentrations lose their relationship with CCN concentrations where there is more ice present. The ice removes droplets so that CCN concentrations no longer correlate with droplet concentrations the more ice that exists in clouds.<br>Hudson, J.G., S. Noble and V. Jha, 2010: Comparisons of CCN with supercooled clouds. J. Atmos. Sci., 67, No. 9, 3006–3018. [James Hudson, USA] | References to Verheggen et al. ACP 2007 and Hudson et al. 2010 added a little later. |
| 7-1178 | 7 | 44 | 1 | 44 | 3 | Just want to note that Bergeron-Wegener-Findeisen process is important in dynamically inactive mixed-phase clouds such as the Arctic stratiform clouds. In dynamically active clouds such as cumulonimbus clouds, riming process is dominant over Bergeron-Wegener-Findeisen process for the ice-particle growth and sedimentation. [Seoung Soo Lee, United States of America] | Not changed. This is just a general description of WBF. Two references to observations of WBF are added. |
| 7-1179 | 7 | 44 | 3 | | | "the ease with which ice forms". Rate, maybe; extent, maybe; but ease? [Stephen E Schwartz, USA] | Changed to rate |
| 7-1180 | 7 | 44 | 12 | 44 | 13 | What is the basis for the claim that "The presence of more soluble aerosol particles would make it harder to form atmospheric ice homogeneously"? Do you perhaps mean because the smaller, more numerous droplets would require higher supersaturations? If so, then please explain that and include a reference. [Jón Egill Kristjánsson, Norway] | Sentence has been removed. But the reason is freezing point depression. Earlier the text states. Soluble matter or physiochemical transformations can hinder glaciation by depressing the freezing temperature of super-cooled drops (e.g., Baker and Peter, 2008; Girard et al., 2004). |
| 7-1181 | 7 | 44 | 12 | 44 | 13 | Lee et al. (2009) and Lee and Penner (2010) have shown that homogeneous freezing of haze particles (formed on soluble CCN) accounts for large portion of ice particles formed via homogeneous freezing and the number ice particles formed homogeneously increases with the number of haze particles. So, soluble particles can make it easier to form atmospheric ice homogeneously.<br><br>Reference: | Not changed. This statement is at odds with the known suppression of freezing point by soluble aerosol. (e.g., Baker and Peter, 2008; Girard et al., 2004). Did the models involved adequately include this effect. |

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| | | | | | | Lee, S. S., L. J. Donner, and V T J Phillips, 2009: Sensitivity of aerosol and cloud effects on radiation to cloud types: comparison between deep convective clouds and warm stratiform clouds over one-day period. Atmos. Chem. Phys., 9, 2555-2575.<br><br>Lee, S. S., and J. E. Penner, 2010, Aerosol effects on ice clouds: Can the traditional concept of aerosol indirect effects be applied to aerosol-cloud interactions in cirrus clouds?, Atmos. Chem. Phys., 10, 10345-10358.<br>[Seoung Soo Lee, United States of America] | |
| 7-1182 | 7 | 44 | 12 | 44 | 13 | "The presence of…", references could be useful. [Chien Wang, United States of America] | Sentence no longer appears. Instead reference is to Baker and Peter (2008) and Girard et al. (2004) earlier on. |
| 7-1183 | 7 | 44 | 12 | | | Pratt et al. (2009) have shown direct evidence for the important role of biological particles in ice clouds. They found that at least one third of ice particles sampled in cloud contained a biological particle. Biological particles tend to nucleate ice at higher temperatures than mineral dust, thus being potentially more important in relatively warm clouds. [Meinrat O. Andreae, Germany] | Add. Expand this section based on new review article by Després et al.; note though that the Pratt study only found biological particles in one cloud |
| 7-1184 | 7 | 44 | 15 | 44 | 15 | "7.2.2.4", the section does not exist. [Chien Wang, United States of America] | Changed to 7.3.3.5 |
| 7-1185 | 7 | 44 | 21 | | | 7.4.4.2 mainly discusses Arctic mixed-phase clouds so "Arctic" could be added in the title. [Chien Wang, United States of America] | Changed |
| 7-1186 | 7 | 44 | 23 | 44 | 25 | Much of the SHEBA data had not yet been fully analyzed by the time of AR4. It is also worth mentioning here, e.g., Uttal et al., 2002, BAMS). [Paquita Zuidema, USA] | Added |
| 7-1187 | 7 | 44 | 25 | 44 | 27 | A good example of a long-lasting mixed-phase case from SHEBA is detailed in Zuidema et al., 2005, JAS. [Paquita Zuidema, USA] | Added |
| 7-1188 | 7 | 44 | 31 | 44 | 31 | Please add a reference for that statement, i.e.,Ovchinnikov et al. 2011. Reference: Ovchinnikov, M., A. Korolev, and J. Fan (2011), Effects of ice number concentration on dynamics of a shallow mixed-phase stratiform cloud, J. Geophys. Res., 116, D00T06, doi:10.1029/2011JD015888. [Jiwen Fan, United States of America] | Added |
| 7-1189 | 7 | 44 | 37 | | | Observations on Bergeron-Findeisen process in mixed-phase and glaciated clouds: strong decrease of activated fraction with increasing ice mass fraction, e.g. B. Verheggen, J. Cozic, E. Weingartner, K. Bower, S. Mertes, P. Connolly, M. Gallagher, M. Flynn, T. Choularton, U. Baltensperger, Aerosol activation in mixed phase clouds at the high alpine site Jungfraujoch, J. Geophys. Res., 112, D23202, doi:10.1029/2007/JD008714, 2007; and: T. W. Choularton, K.N. Bower, E. Weingartner, I. Crawford, H. Coe, M.W. Gallagher, M. Flynn, J. Crosier, P. Connolly, A. Targino, M.R. Alfarra, U. Baltensperger, S. Sjögren, B. Verheggen, J. Cozic, M. Gysel, The influence of small aerosol particles on the properties of water and ice clouds, Faraday Discuss., 137, doi:10.1039/b702722m, 205-222, 2008. [Urs Baltensperger, Switzerland] | Reference to Verheggen added |
| 7-1190 | 7 | 44 | 39 | | | Section 7.4.4.3: The section is titled "Advances in process level understanding" (as with the other sections in the chapter) but it is entirely devoted to issues of ice nucleation. Is this where the whole community's attention has been focused? [Robert Pincus, USA] | it's certainly a key gap but, e.g., habit growth also receives attention (Harrington and coworkers) in 7.4.4.3. Other gaps (e.g., collection kernels)? Are we missing work here? I would agree with Robert. Suggestions for new text?? |
| 7-1191 | 7 | 44 | 39 | | | This section seems discussing modeling skill or parameterization rather than "process level understanding". [Chien Wang, United States of America] | Not changed. Korolev's studies are process level understanding as are the studies by Khvorostyanov |
| 7-1192 | 7 | 44 | 41 | 44 | 47 | "Since the AR4 research on ice-microphysical processes has been active, to a large degree with an eye toward<br>a better representation of such processes in models. Korolev (2007)" The first treatment of the B-W-F process explicitly with size- and composition resolution in a global model was in Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, | Not changed. The text simply states that Korolev built a theoretically-based parameterization of WBF. |

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| | | | | | | using algorithms from Jacobson, M. Z., Development of mixed-phase clouds from multiple aerosol size distributions and the effect of the clouds on aerosol removal, J. Geophys. Res., 108 (D8), 4245, doi:10.1029/2002JD002691, 2003. [Mark Z. Jacobson, U.S.A.] | |
| 7-1193 | 7 | 44 | 41 | 44 | 58 | The GCSS modeling intercomparison led by Hugh Morrison (Morrison et al., 2011, JAMES) is also useful for demonstrating the sensitivity of simulations to feedbacks from ice depositionn back to the mixed-phase cloud longevity. [Paquita Zuidema, USA] | Not changed since Morrison et al. (2012) covers this. |
| 7-1194 | 7 | 44 | 49 | 44 | 52 | This sentence is very difficult to follow. [Robert Pincus, USA] | Modified to clarify. |
| 7-1195 | 7 | 44 | 50 | 44 | 50 | Delete "the dependence of". [Anthony Del Genio, USA] | Modified |
| 7-1196 | 7 | 44 | 54 | 44 | 55 | "makes ice initiation interesting" is strange wording. What does it mean? [Jón Egill Kristjánsson, Norway] | Changed |
| 7-1197 | 7 | 44 | 54 | 44 | 56 | "...heterogeneous freezing parameterizations employed in cloud or larger-scale models remain mostly empirical." This is not entirely correct. Contact freezing and homogeneous/heterogeneous freezing and evaporation freezing are treated as a function of size, particle volume, temperature, and composition in a global model with some physical interactions, as described in Sections 4.6, 4.7, and 4.10 of Jacobson, M. Z., Development of mixed-phase clouds from multiple aerosol size distributions and the effect of the clouds on aerosol removal, J. Geophys. Res., 108 (D8), 4245, doi:10.1029/2002JD002691, 2003. For example, contact freezing is calculated by coagulating size-resolved interstitial aerosol particles with size-resolved hydrometeor particles. [Mark Z. Jacobson, U.S.A.] | Added reference |
| 7-1198 | 7 | 45 | 2 | 45 | 2 | Suggest to add the following text to state the effect of different ice nucleation parameterizations on clouds to be complete: "Different freezing parameterizations produce large differences in cloud properties (Fan et al., 2010)". References: Fan, J., J. M. Comstock, M. Ovchinnikov, S. A. McFarlane, G. McFarquhar, and G. Allen (2010), Tropical anvil characteristics and water vapor of the tropical tropopause layer: Impact of heterogeneous and homogeneous freezing parameterizations, J. Geophys. Res., 115, D12201, doi:10.1029/2009JD012696. [Jiwen Fan, United States of America] | Text and reference added. |
| 7-1199 | 7 | 45 | 5 | 45 | 6 | it is not clear to me what you mean "the threshold realtive humidity" for homogeneous nucleation. If you mean the onset RH for homogeneous nucleation it is ~150% slightly dependent on temperature. However it is the maximum RH (depending on updraft velocity) in the uprising parcels that determines the overal ice number nucleated. [Xiaohong Liu, United States of America] | replace by "onset RH" |
| 7-1200 | 7 | 45 | 5 | 45 | 10 | I think it worthes mentioning the work of Liu and Penner (2005) here. This work is among the earliest prameterizations developed for GCMs based on the results of parcel model simulations. This parameterizations has been implemented in some major climate models (e.g., NCAR CAM5; GFDL AM3). Liu, X., and J. E. Penner (2005) Ice nucleation parameterization for global models. Meteorologische Zeitschrift, 14, No.4, 499-514. [Xiaohong Liu, United States of America] | Reference added |
| 7-1201 | 7 | 45 | 12 | 45 | 12 | Just for clarity: "… requires lower threshold relative humidities (or allows higher threshold temperatures)…" [Ralph Kahn, United States of America] | Not changed. Seems clear as is.. |
| 7-1202 | 7 | 45 | 18 | 45 | 32 | It should be highlighted that the study of Penner et al. (2009) and Liu et al. (2009), mentioned in the next paragraph, came to different results despite using nearly the same model, but a different representation of ice nucleation. This clearly highlights the importance of this parametrization in the models and results should be taken with care. [Andrew Ferrone, Germany] | Text changed. A comment from Liu clarifies the difference "There is a mis-representation of Penner et al. (2009) cirrus study which is not an adjusted RF, but an instantaceous RF. The difference between Penner et al. (2009) and Liu et al. (2009) is that Liu et al. is an adjusted RF, while Penner et al. is not." [[UL?]] |
| 7-1203 | 7 | 45 | 18 | 45 | 32 | This shows the models are not much use. [VINCENT GRAY, NEW ZEALAND] | Not changed. |
| 7-1204 | 7 | 45 | 19 | 45 | 32 | There is a mis-representation of Penner et al. (2009) cirrus study which is not an adjusted RF, but an instantaceous RF. The difference between Penner et al. (2009) and Liu et al. (2009) is that Liu et al. is an adjusted RF, while Penner et al. is not. [Xiaohong Liu, United States of America] | Clarified |

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| 7-1205 | 7 | 45 | 33 | | | New Study by Gettelman et al (In prep) will estimate indirect effect of aerosols on cirrus clouds using 2 models and 3 different ice nucleation paramaterizations. Estimated impacts are +0.3Wm-2 ± 0.1 Wm-2 [Andrew Gettelman, USA] | Not changed at this point. Will consider if submitted by deadline. |
| 7-1206 | 7 | 45 | 38 | 45 | 38 | I'm out of my sphere of expertise here, but deep convection can arise from more than differential heating, can't it? What about mesoscale circulations and orographic lifting? [Lorraine Remer, USA] | Change and consider 7-1206 as well. |
| 7-1207 | 7 | 45 | 38 | 45 | 39 | Well sort of, except that it depends on how humid the boundary layer is too, and in many places that humidity comes from other places. There's lots of differential surface-atmosphere heating in the Sahara, but not much deep convection. [Anthony Del Genio, USA] | change |
| 7-1208 | 7 | 45 | 39 | 45 | 40 | Not clear what is meant by "convective instabilities" in plural. [Jón Egill Kristjánsson, Norway] | change to singular. |
| 7-1209 | 7 | 45 | 45 | 45 | 45 | A good new paper to cite here is Tau et al. (2012): Tao, W.-K., J.-P. Chen, Z. Li, C. Wang, and C. Zhang, 2012: Impact of aerosols on convective clouds and precipitation. Rev. Geophys., 50, doi:10.1029/2011RG000369. [Steven Ghan, USA] | Add ref |
| 7-1210 | 7 | 45 | 47 | 45 | 50 | Many simulations have shown that aerosol-induced increase in water loading is maximized in the middle portion of deep convective clouds, which is around the freezing level. [Seoung Soo Lee, United States of America] | Consider modifying |
| 7-1211 | 7 | 45 | 50 | 44 | 51 | I'm not sure an observational reference is what you want here. The modeling study of Stevens and Seifert, 2008 is better able to elucidate the aerosol effect. [Paquita Zuidema, USA] | consider ading/modifying |
| 7-1212 | 7 | 45 | 50 | 45 | 51 | I don't understand this sentence; how the lack of precipitation makes clouds deeper? [Seoung Soo Lee, United States of America] | Removal of water, stabilization |
| 7-1213 | 7 | 45 | 50 | 45 | 51 | "the lack of precipitation…more precipitation", many references could be added here, e.g., Wang 2005 (JGR, 110, D21211), Cheng et al. 2010 (Atmos. Res., 96, 461-476). [Chien Wang, United States of America] | Add refs. |
| 7-1214 | 7 | 45 | 51 | 44 | 52 | The importance of trade-wind cumulus cold pools is further stressed by the observations shown in Zuidema et al. (2012). [Paquita Zuidema, USA] | Add ref |
| 7-1215 | 7 | 45 | 51 | 45 | 51 | Please add a typical reference for this topic, i.e., Rosenfeld et al. 2008. It is already in the reference list. [Jiwen Fan, United States of America] | Add refs |
| 7-1216 | 7 | 45 | 51 | 45 | 52 | There is absolutely no question that cold pools are crucial for secondary precipitation development in convective storms. "May be" is inappropriate language. [Anthony Del Genio, USA] | change |
| 7-1217 | 7 | 45 | 51 | | | It appears at least as pertinent here to cite Rosenfeld et al. (2008) than Nuijens et al. (2009) [Meinrat O. Andreae, Germany] | We need to select a handful of refs (see 1211-1217) |
| 7-1218 | 7 | 45 | 53 | 45 | 53 | Tao et al 2012 (Geophy Rev in press) could be cited to support the statement in the last sentence. [Chien Wang, United States of America] | add ref |
| 7-1219 | 7 | 46 | 1 | 46 | 1 | The word "retarded" is ambiguous. Better: "delayed" [Jón Egill Kristjánsson, Norway] | change perhaps |
| 7-1220 | 7 | 46 | 1 | 46 | 6 | The statements in this paragrap are talking about the concept model, i.e., the idealized condition. In real case studies, this invigoration effect is changed at different environemntal conditions. So, it is necessary to add "However, in the real atmospheric environment, vertical wind shear is found to strongly regulate this invigoration of deep convective clouds by aerosols (Fan et al. 2009)" to the end of paragraph. [Jiwen Fan, United States of America] | Add words to the effect that shear plays a role and ref Fan |
| 7-1221 | 7 | 46 | 4 | 46 | 6 | The sentence starting with "it has been" in line 5 is suggested to be replaced with the following paragraph or a paragraph similar to the following paragraph, since Lee (2012) showed that although latent heat release from freezing is smaller than that from condensation for unit water mass, increase in latent heat release from freezing induces stronger updrafts and then more condensation, leading to positive feedbacks among freezing, condensation and updrafts in turn leading to substantial invigoration of clouds after sufficient time elapses (stated differently, aerosol-induced increase in freezing does not change cloud intensity much initially | This seeems pertinent. |

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| | | | | | | but after some time, it induces sufficiently increased condensation and associated latent heat release through the feedbacks, leading to larger change in cloud intensity) :<br><br>it has been nonetheless been simulated and thus hypothesized to have a critical effect on cloud development by triggering feedbacks among dynamics and microphysics (Andreae et al., 2004; Rosenfeld et al., 2008; Lee, 2012)<br><br>Reference:<br><br>Lee, S. S., 2012, Effect of aerosol on circulations and precipitation in deep convective clouds, J. Atmos. Sci., accepted.<br><br>[Seoung Soo Lee, United States of America] | |
| 7-1222 | 7 | 46 | 10 | 46 | 10 | "such mechanisms" is not clear. What mechanisms? [Jón Egill Kristjánsson, Norway] | indeed. Change to "invigoration" mechanisms? Or define invigoration concept first? |
| 7-1223 | 7 | 46 | 12 | 46 | 12 | In line 12 after “rainfall rates”, suggest to add the following paragraph or a paragraph similar to the following paragraph:<br><br>Studies in Oklahoma plain and in the tropical western Pacific show that while changes in total precipitation induced by an aerosol perturbation are not significant, those in cloud population, cloud size and the spatial temporal distribution of precipitation are significant (e.g., Lee et al., 2008; Lee, 2011). These significant changes lead to more frequent low and high rainfall rates and less frequent mid rainfall rates with higher CCN concentrations. This stresses that we need to go beyond single cloud to systems of multiple clouds for the study of aerosol-cloud interactions, since the multiple-cloud systems are more relevant to climate than single clouds and these studies demonstrate that there are a variety of different aspects of aerosol effects on clouds in the multiple-cloud systems which we are not able to see in single clouds.<br><br>Reference:<br><br>Lee, S. S., L. J. Donner,V T J Phillips, and Y. Ming, 2008: Examination of aerosol effects on precipitation in deep convective clouds during the 1997 ARM summer experiment. Quarterly Journal of the Royal Meteorological Society, 134(634), doi: 10.1002/qj.287<br><br>Lee, S. S., 2011, Dependence of aerosol-precipitation interactions on humidity in a multiple-cloud system, Atmos. Chem. Phys., 11, 2179-2196.<br>[Seoung Soo Lee, United States of America] | Include ref but more importantly convey the idea that total precip is only part of the story and that freq distribution/spatial distribution might change. |
| 7-1224 | 7 | 46 | 14 | 46 | 14 | Add "microphysical" between "aerosol effects", this is based on the context. [Chien Wang, United States of America] | Change. Otherwise I gues it could mean semidirect effect |
| 7-1225 | 7 | 46 | 14 | 46 | 16 | Just use this statement as a summary and remove the rest of the section? [Andrew Gettelman, USA] | I think this would give short shrift to a lot of studies. |
| 7-1226 | 7 | 46 | 20 | | | The studies of Lebsock et al (2008). Christensen and Stephens (2012; JGR) L'Ecuyer et al, 2009 (JGR); Berg 2008 (JGR) all show evidence of aeorsol effecs on preciptation [Graeme Stephens, USA] | We're discussing deep convection here. Check which of these refs are relevant. E.g., Lebsock is for shallow clouds. |
| 7-1227 | 7 | 46 | 23 | 46 | 25 | Too general about satellite advances. [Daniel Murphy, United States of America] | Perhaps. Consider specificity |
| 7-1228 | 7 | 46 | 25 | 46 | 25 | Should add a sentence here about the utility of long-term surface-based remote sensing for such studies. The aerosol below and/or before the cloud can be retrieved with high time resolution. [Steven Ghan, USA] | Agreed. Add. |
| 7-1229 | 7 | 46 | 27 | 46 | 27 | also mention analysis of surface-based remote sensing. [Steven Ghan, USA] | Agreed. Add. |
| 7-1230 | 7 | 46 | 27 | 46 | 37 | A nice example study for in situ measurements should be included in this paragraph, i.e.,, “Long –term | Consider adding |

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| | | | | | | ground-nased measrements over U.S. Southern Great Plain (SGP) revealed a net invigoration of clouds by aerosols with the increase of cloud top height and frequency of heavy rain for warm-based mixed-phase clouds especially in the summer seasons (Li et al. 2011)" [Jiwen Fan, United States of America] | |
| 7-1231 | 7 | 46 | 27 | 46 | 37 | "..increases in aerosol loadings…with larger cloud fractions…" This applies only at low AODs. At higher AODs, for example, satellite data show that increases in aerosol optical depth further from biomass burning aerosols reduce cloud fraction and cloud optial depth (Ten Hoeve, J.E., L.A. Remer, and M.Z. Jacobson, Microphysical and radiative effects of aerosols on warm clouds during the Amazon biomass burning season as observed by MODIS: impacts of water vapor and land cover, Atmos. Chem. Phys. 11, 3021-3036, 2011) [Mark Z. Jacobson, U.S.A.] | This is the absorbing aspect. Make sure we are adequately covering semidirect effect. We cover it in the aerosol section but may need to revise. |
| 7-1232 | 7 | 46 | 27 | 46 | 37 | It would be natural to include a reference to the paper of Stjern et al. (2011, doi:10.1029/2010JD014603), who found a negative trend in the frequency of light precipitation over Europe with decreasing aerosol loading. [Jón Egill Kristjánsson, Norway] | Check ref. Consider adding. |
| 7-1233 | 7 | 46 | 29 | 46 | 29 | "After "portions." Add: "Analysis of satellite observations indicated that, on average, both naturally transported and local anthropogenic dust aerosols can significantly reduce water cloud particle size, optical depth, and liquid water path and act to exacerbate drought conditions over semi-arid areas of northwest China (Huang et al., 2006a, 2006b, 2010)."<br><br>References:<br>1. Huang J., B. Lin, P. Minnis, T. Wang, X. Wang, Y. Hu, Y. Yi, and J. R. Ayers, 2006a: Satellite-based assessment of possible dust aerosols semi-direct effect on cloud water path over East Asia, Geophys. Res. Lett., 33, doi: 10.1029/2006GL026561.<br>2. Huang J., P. Minnis, B. Lin, T. Wang, Y. Yi, Y. Hu, S. Sun-Mack, and K. Ayers, 2006b: Possible influences of Asian dust aerosols on cloud properties and radiative forcing observed from MODIS and CERES, Geophys. Res. Lett., 33, L06824, doi: 10.1029/2005GL024724.<br>3. Huang J., P. Minnis, H. Yan, Y. Yi, B. Chen, L. Zhang, and J. K. Ayers, 2010: Dust aerosol effect on semi-arid climate over Northwest China detected from A-Train satellite measurements, Atmos. Chem. Phys., 10, 12465-12495.<br>" [Jianping Huang, China] | Check refs and decide on importance. |
| 7-1234 | 7 | 46 | 29 | 46 | 32 | To be complete, tropical western Pacific should be added to the list and volcanic sulfate (Yuan et al., 2011) Yuan, T., L. Remer, K.E. Pickering, and H. Yu (2011). Observational evidence of aerosol enhancement of lightning activity and convective invigoration Geophys. Res. Lett, 38, L04701 doi:10.1029/2010GL046052 [Lorraine Remer, USA] | consider adding. |
| 7-1235 | 7 | 46 | 31 | 46 | 31 | Correct the Reference "Lindsey and Fromm 2008" to "Lindsey and Fromm, 2008" [Panuganti China Sattilingam Devara, India] | Is the comma the issue?! |
| 7-1236 | 7 | 46 | 31 | 46 | 31 | Add citation for Li et al. (2011): Li, Z., F. Niu, J. Fan, Y. Liu, D. Rosenfeld and Y. Ding, 2011: Long-term impacts of aerosols on the vertical development of clouds and precipitatio. Nature Geosci., DOI: 10.1038/NGEO1313. Consider adding a sentence on this inportant paper. [Steven Ghan, USA] | Consider adding ref. |
| 7-1237 | 7 | 46 | 33 | 41 | | after "(Rosenfeld et al., 2008)", add "The hypothesis of aersol invigoration was confirmed by an analysis and modeling study using 10 years of Atmospheric Radiation Measurements in Oklahoma by Li et al. (2011b). They found that cloud top height, thickness and precipitation increase systematically with increasing aerosol condensation number for deep convective clouds with warm base and cold tops. For thin warm clouds, however, precipitation is suppressed and the frequency of clouds is reduced by aerosols". Reference: Li, Z., F. Niu, J. Fan, Y. Liu, and D. Rosenfeld, Y. Ding (2011b), The long-term impacts of aerosols on the vertical development of clouds and precipitation, Nature-Geoscience, doi: 10.1038/NGEO1313. [Zhanqing Li, USA] | Consider adding ref. |
| 7-1238 | 7 | 46 | 36 | 46 | 37 | "likewise changes…", is this related to aerosol loading change? [Chien Wang, United States of America] | The suggestion si that it might be but there could obviously be confounding aspects. Clarify this sentence. |
| 7-1239 | 7 | 46 | 39 | 46 | 40 | I do not think the statement is really true. A few observational studies such as ground-based measrement | I think we need to provide clarity. We shouldn't be |

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| | | | | | | study by Li et al (2011) and a very recent satellite study (Koren et al. 2012) show the invigoration of clouds by aerosols and the increased rain rate for mixed-phase clouds. It is usually the case for stratiform mixed-phase clouds, but not for deep convective mixed-phase clouds. References: Koren I., O. Altaratz, L. A. Remer, G. Feingold, et al. (2012), Aerosol-induced intensification of rain from the tropics to the mid-latitudes, Nature Geoscience, doi:10.1038/ngeo1364. [Jiwen Fan, United States of America] | mixing orographic with deep convective as a counterpoint to "invigoration". And we should do a better job of describing what the invigoration papers are saying and what the potential issues might be. |
| 7-1240 | 7 | 46 | 46 | 46 | 46 | "these observations" are mostly "remote sensing data". Also, it might be good to briefly discuss the limitation of the remote sensing data in analyzing the aerosol-cloud effects. [Chien Wang, United States of America] | Check on whethere we have adequately stated remote-sensing artefacts |
| 7-1241 | 7 | 46 | 46 | 46 | 51 | Again, no consensus, could just use this summary. [Andrew Gettelman, USA] | The lack of consensus shouldn't mean that we don't present the different aspects/issues. |
| 7-1242 | 7 | 46 | 55 | 46 | 57 | To be accurately, I think the sentence should be changed to “Modelling studies suggest that the dynamic and thermodynamic environment in which the clouds grow is an important factor in the determination of the aerosol effect on the ground precipitation (Khain et al., 2005; Lynn et al., 2005; Tao et al., 2007; Fan et al. 2009)". [Jiwen Fan, United States of America] | Alluding to shear. Add. |
| 7-1243 | 7 | 46 | 57 | 46 | 57 | "dry unstable air" is misleading, because it sounds like one is talking about absolutely unstable air. I assume what is meant is "relatively dry, conditionally unstable air" [Jón Egill Kristjánsson, Norway] | Change as suggested. |
| 7-1244 | 7 | 46 | | | | Section 7.4.5.2 Observations of Aerosol Effects on Precipitating Systems: Studies in Sc clouds are the most clear here in terms of demonstrable suppression of drizzle by increased aerosols, but are not even mentioned. [Robert Wood, USA] | This section limited itself to convective clouds. Have we given short shrift to precip in warm clouds? That was done in 7.4.3.2 but we should revisit. |
| 7-1245 | 7 | 47 | 1 | 47 | 2 | "for deep maritime clouds… increase in precipitation", could cite Wang 2005 (JGR). [Chien Wang, United States of America] | consider adding ref. |
| 7-1246 | 7 | 47 | 3 | 47 | 5 | In a single cloud, an increase in wind shear increases detrainment of cloud particles. Regarding this, aerosol-induced increase in cloud liquid can be weakened with increasing wind shear, since with increasing wind shear, the increased cloud liquid by increasing aerosol can be transported to unsaturated areas more efficiently, leading to its more efficient evaporation. This also weakens aerosol-induced increase in the magnitude of cloud intensity, since increasing wind shear causes more entrainment-detrainment cooling in air parcel as found in Fan et al. (2009). However, for multiple clouds (which are more relevant to climate than a single cloud), the more efficient transportation of increased cloud liquid induced by aerosol increase with high wind shear can intensify downdrafts and gust fronts more efficiently, leading to more intensified secondary clouds at high wind shear than at low wind shear. These more intensified secondary clouds at high wind shear can make aerosol-induced domain-averaged increase in cloud liquid and cloud intensity higher at high wind shear than at low wind shear as found in Lee et al. (2008).<br><br>References:<br><br>Fan, J., and Coauthors, 2009: Dominant role by vertical wind shear in regulating aerosol effects on deep convective clouds. Journal of Geophysical Research-Atmospheres, 114.<br><br>Lee, S. S., L. J. Donner, V T J Phillips, and Y. Ming, 2008: The dependence of aerosol effects on clouds and precipitation on cloud-system organization, shear and stability. J. Geophys. Res., 113, D16202, doi:10.1029/2007JD009224<br>[Seoung Soo Lee, United States of America] | In discussing shear, as we do by referencing Fan, we should conside rthis contrast between single clouds and cloud systems. |
| 7-1247 | 7 | 47 | 7 | 47 | 7 | The phrase "Weekly cycles …… have emerged to tackle this problem" does not make sense. Please rephrase. [Jón Egill Kristjánsson, Norway] | Agreed. Rephrase. |
| 7-1248 | 7 | 47 | 7 | 47 | 22 | These lines could be deleted to shorten - not critical [Daniel Murphy, United States of America] | Orographic precip should be retained. (spatial redistribution). Other sections also seem worthy of retaining. |
| 7-1249 | 7 | 47 | 7 | | 8 | "all studies"; better something like "studies consistently find ... "; hard to make a representation to "all". | Agreed. Change |

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| | | | | | | [Stephen E Schwartz, USA] | |
| 7-1250 | 7 | 47 | 7 | | | This sentence doesn't make sense. Weekly cycles didn't emerge to tackle a problem. (Perhaps "This problem has been addressed by examining weekly cycles of aerosol loading and precipitation." ) [Robert Pincus, USA] | Agreed. Change |
| 7-1251 | 7 | 47 | 10 | 47 | 10 | Correct the Reference " Stjern 2011" to "Stjern, 2011" [Panuganti China Sattilingam Devara, India] | The comma! |
| 7-1252 | 7 | 47 | 12 | 45 | 15 | "Looking at summer season forecasts using a cloud-resolving regional model the question has been posed as to whether changes<br>to the aerosol systematically affect precipitation over large regions. Little to no systematic effect of the aerosol could be documented (Seifert et al., 2011)." However, other studies have found an impact. High resolution studies over Cafifornia found that aerosols reduced precipitation overall but increased it on the downslope side of mountains. (e.g., Pargraph 52 of Jacobson, M.Z., Y.J. Kaufmann, Y. Rudich, Examining feedbacks of aerosols to urban climate with a model that treats 3-D clouds with aerosol inclusions, J. Geophys. Res., 112, D24205, doi:10.1029/2007JD008922, 2007; Jacobson, M.Z., and Y.J. Kaufmann, Aerosol reduction of the wind, Geophys. Res. Lett., 33, L24814, doi:10.1029/2006GL027838, 2006). These are the only studies to date looking at the effects of aerosols on precipitation on the regional scale with a model that treats discrete size and composition resolved aerosol particles and hydormeteor particles both. [Mark Z. Jacobson, U.S.A.] | Orographic. Consider including ref in discussion of orographic clouds. |
| 7-1253 | 7 | 47 | 12 | 47 | 15 | The following study supports the findings of Seifert et al. (2011) and should be added as further example: Bangert, M., Kottmeier, C., Vogel, B., and Vogel, H.: Regional scale effects of the aerosol cloud interaction simulated with an online coupled comprehensive chemistry model, Atmos. Chem. Phys., 11, 4411-4423, doi:10.5194/acp-11-4411-2011, 2011. [Andrew Ferrone, Germany] | consider adding ref. |
| 7-1254 | 7 | 47 | 12 | 47 | 15 | This reviewer believes that question posed here is closely linked to the teleconnection process discussed above. [Seoung Soo Lee, United States of America] | Again addresses the question of potential larger scale effects. We should add an expanded discussion on aerosol induced changes in mesoscale circulations. Touched upon on pg 43, line 7 |
| 7-1255 | 7 | 47 | 17 | 47 | 22 | This reviewer believes "precipitation displacement" here can be categorized as one of teleconnection processes, since this reviewer thinks that the region where the displacement occurs is not directly affected by aerosol pollution. [Seoung Soo Lee, United States of America] | no, I think the orographic barrier is different. |
| 7-1256 | 7 | 47 | 17 | | | "Cloud-resolving modelling of orographic precipitation... [Meinrat O. Andreae, Germany] | change. |
| 7-1257 | 7 | 47 | 19 | 47 | 22 | "Regional studies confirm a reduction of precipitation on the windward side of a mountain barrier and a tendency for the precipitation to shift downstream to the leeward side but…" This was shown in an earlier study iny Pargraph 52 of Jacobson, M.Z., Y.J. Kaufmann, Y. Rudich, Examining feedbacks of aerosols to urban climate with a model that treats 3-D clouds with aerosol inclusions, J. Geophys. Res., 112, D24205, doi:10.1029/2007JD008922, 2007. [Mark Z. Jacobson, U.S.A.] | consider adding ref. |
| 7-1258 | 7 | 47 | 24 | 47 | 26 | Lee et al. (2012) have shown the complicated picture for long-term simulations; clouds adjust their environmental instability to minimize aerosol impacts, relevant to the buffered system suggested by Stevens and Feingold (2009).<br><br>References:<br><br>Lee, S. S., and G. Feingold, P. Y. Chuang, 2012, Effect of aerosol on cloud-environment interactions in trade cumulus, Journal of the Atmospheric Sciences, submitted.<br><br>Stevens, B., and Feingold, G.: Untangling aerosol effects on clouds and precipitation in a buffered system, Nature, 461, doi:10.1038/nature08281, 2009.<br>[Seoung Soo Lee, United States of America] | Paper likely wont make the deadline. |
| 7-1259 | 7 | 47 | 24 | 47 | 31 | Statement here is closely associated with studies mentioned in my comments above for Line 12 in 7-46. [Seoung Soo Lee, United States of America] | yes but not sure changes are necessary. |

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| 7-1260 | 7 | 47 | 30 | | | Note my comment 17 above. [Graeme Stephens, USA] | Can we track down the original files. Otherwise we have to guess. |
| 7-1261 | 7 | 47 | 33 | | | The section head is entitled: "7.4.5.4 Advances in and Insights Gained from Large-Scale Modelling Studies". The section itself is two brief paragraphs. Is one to infer from that that this is the extent of Advances in and Insights Gained from Large-Scale Modelling Studies over the past 6 years? If that is a correct inference, should the section not explicitly offer the assessment that there has been little in the way of Advances in and Insights that have been Gained from Large-Scale Modelling Studies in the past six years. [Stephen E Schwartz, USA] | This subsection (now 7.4.3.4) is still brief but now points to the important role of regional models and their progress in representing aerosol-cloud interactions. The large scale models have made significant progress in including more of these processes and this improves our ability to assess the radiative forcing associated with these processes. The radiative forcing aspects are now discussed in 7.5. The brevity of 7.4.3.4 reflects the fact that at the process level, which 7.4 is mostly concerned with, it is primarily the small-scale models and observations that have improved our understanding. |
| 7-1262 | 7 | 47 | 35 | 47 | 36 | Fast and slow feedbacks can be confusing terms with other chapters. Use 'rapid response' and 'response to surface temperature changes' [Gunnar Myhre, Norway] | The terminology is now explained in 7.1 and homogeneised throughout the chapter. "Fast feedback" has been banned |
| 7-1263 | 7 | 47 | 35 | 47 | 38 | This reviewer believes that argument here is closely linked to the teleconnection process discussed above. [Seoung Soo Lee, United States of America] | But this section discusses large-scale models |
| 7-1264 | 7 | 47 | 35 | | 36 | "Fast feedbacks associated with the aerosol indirect effects do not cause much change in precipitation if an average over a big enough domain is considered."<br><br>First, careful here. The processes that are referred to here as fast feedbacks are now not classified as feedbacks, but are viewed as part of the adjustment to imposed forcings. Feedbacks are being restricted to changes in climate that result from longer term responses to forcings.<br><br>Substantively, on a global basis precip = evap, so if the domain is large enough nothing other than change in evapotranspiration changes precip. So the question is not one of a domain being large enough, but rather whether on the scale of model grid cells of current large scale models aerosols are found to significantly (statistically) or substantially change precipitation, and if so, in what direction and how much (quantitatively) and whether this sort of finding is robust across models. The language "do not cause much change in precipitation" is unacceptably vague and non quantitative, irrespective of the scale issue.<br><br>The text goes on to state:<br><br>"These latter effects have been estimated in AR4. Here the decrease in the global annual mean shortwave radiation at the surface since pre-industrial times due to scattering and absorbing aerosols amounted to –2.3 W m– with a range between –1.3 to –3.3 W m– (Denman et al., 2007). The associated change in the global mean precipitation amounts to between 0 and –0.13 mm day–1 (Denman et al., 2007). "<br><br>That statement is, of course, a recapitulation of the state of affairs at AR4, not an assessment of any " Advances in and Insights Gained from Large-Scale Modelling Studies" that have been gained in the last six years. A read of that statement is that the increase in aerosols results in a decrease in (downwelling or absorbed; it is not clear, but needs to be specified in a quantitative statement) global mean shortwave irradiance of 1.3 to 3.3 W m-2, confidence level not specified. It is not clear that this result required large scale models, ostensibly the subject of the section, or that it is an advance. The statement goes on to say that the resultant change in evapotranspiration is between 0 and 0.13 mm day-1. I am somewhat surprised in that for latent heat of water, 1 m yr-1 corresponds to 78 W m-2, so 0.13 mm day-1 corresp to 3.7 W m-2, somewhat greater than the maximum reduction in insolation (incident or absorbed not specified) which suggests that essentially all of the reduction in insolation may be going into reduced evapotranspiration, none into reduced infrared emission or sensible heat. I would think this needs to be discussed. But the range 0 to 0.13 mm day-1 | These all seem like very pertinent criticisms. |

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| | | | | | | hardly seems like a robust result. [Stephen E Schwartz, USA] | |
| 7-1265 | 7 | 47 | 43 | | 44 | The second and final Advance in and Insight Gained from Large-Scale Modelling Studies is the weekly cycle in precipitation. From a check of the title of the single cited paper, this seems to be the result of application of two climate models to Europe. So it hardly seems robust or general enough to rise to the level of an Advance in and Insight Gained from Large-Scale Modelling Studies.<br><br>So at the end of the section it would seem that there are no Advances in and Insights that have been Gained from Large-Scale Modelling Studies in the past six years. I suggest that the authors try harder to identify and report Advances in and Insights that have been Gained from Large-Scale Modelling Studies in the past six years or else that the section conclude that there have been no such advances and insights. [Stephen E Schwartz, USA] | Needs attention |
| 7-1266 | 7 | 47 | 43 | | | It is important that this study combined observations and modelling. Observations alone would have suggested a significant weekly signal in temperature, for example. [Johannes Quaas, Germany] | worth clarifying. But does this mean that the weekly cycle is indeed weaker because the model suggests it is so? |
| 7-1267 | 7 | 47 | 46 | 47 | 46 | Under this section, why is only SAL effects on hurricane activity discussed? There are so many studies about aerosol impact on monsoon systems. Should it be discussed here? [Jiwen Fan, United States of America] | Seems a valid criticism; although the studies I can think of (Lau and colleagues) have the semidirect effect included too. I think we may be able to do a better job either separating out semidirect effects or folding them in with statements that point to the role of absorption. Current;ly semidirect effects are in 7.3.5. Consider rearrangement, or some brief recap. |
| 7-1268 | 7 | 47 | 48 | 48 | 4 | Description of a rapidly growing field in connecting aerosols (particularly absorbing aerosols) and large-scale convective system such as ITCZ and the monsoons is missing here. Again, related discussions in Tao et al. 2012 (Geophy Rev - in press) could be used. [Chien Wang, United States of America] | See response above to 1268. |
| 7-1269 | 7 | 47 | 49 | | | References to microphysical effects of aerosols on tropical cyclones should be added, and also the mechanism can be mentioned briefly - CCN-induced invigoration of the peripheral clouds on expense of the inflow to the eyewall. See Rosenfeld et al., BAMS 2012, early online release, and references therein. [Meinrat O. Andreae, Germany] | consider adding ref. |
| 7-1270 | 7 | 48 | 6 | 49 | 40 | In section 7.4.6 there is a mix of usage of (ensemble-)average and median values. This should be treated in a more systematic way and median values seem more appropriate in the present case, as the forcing have a non-Gaussian distribution and thus the median is easier to understand. [Andrew Ferrone, Germany] | taken into account such that the new Figure 7.18 now only shows the original values from each panel and then a summary whisker plot. |
| 7-1271 | 7 | 48 | 6 | 50 | 35 | Table 7.3 contains estimates of total AF in the CMIP5 models. These GCM models, as I understand it, assume DMS-SO4-CCN to be the major (only? - seasalt in some cases?) pathway for CCN generation in the marine atmosphere. Considering the remarks in Comment #1, it is qualitatively clear that aerosol direct forcing (ADF) is somewhat reduced upon significantly diminishing the influence of the DMS-SO4-CCN pathway. Further, the iAF (as DMS-derived sulfate CCN numbers diminish in GCM models) may be reduced in magnitude by as much as 1/5th (fraction to emphasize semi-quantitative nature of estimate) from the "likely between -0.7 and -0.2 W/m2" iAF values in the FOD -- if CMIP5 modeling were to account for the Comment #1 remarks. Many publications support a much diminished role for the DMS-SO4-CCN pathway. Among them are: Sievering, Pandis etal., Nature 360, 1992; Chameides and Stelson, J. Geophys. Res. 97, 1993; Kim etal., J. Goephys. Res.100, 1996; and a culminating synthesis paper in late 2011 by Quinn and Bates (Nature, doi: 10.1038). [Herman Sievering, United States of America] | Figure 7.18 now contains the results from the CMIP5 models. We take their less negative total adjusted forcings (AFaci+ari) into account for coming up with the best estimate and likely range of AF. |
| 7-1272 | 7 | 48 | 6 | 50 | 35 | Consideration of the marine atmosphere organic particle-CCN pathway in GCMs --- perhaps still possible to consider, at least roughly, in AR5 CMIP5 modeling?? --- may lead to a compensating influence on the ADF and the iAF to that of a substatially reduced influence by the DMS-SO4-CCN pathway. It is not possible, at this time, to quantify the organic particle-CCN pathway W/m2 influence on ADF and on iAF based upon presently available field and laboratory studies. However, AR5 CMIP5 modeling may be able to place reasonable minimum and maximum bounds on this important contributor to possible reduction in final AR5 WG1 values for iAF and ADF magnitudes in the marine atmosphere. [Herman Sievering, United States of | It's too late to do that estimate in a coordinated way with CMIP5 models, but there are individual models who are considering the DMS-SO4-CCN pathway. |

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| | | | | | | America] | |
| 7-1273 | 7 | 48 | 6 | | | Section 7.4.6: The preceding sections of this chapter have made it clear that climate models have limited ability to provide insight into the magnitude of the iRF. Why on earth, then, would one use an ensemble of models to estimate the iRF and its uncertainty? What does the time evolution of the model estimates tell us about the real magnitude of this quantity? It is not unreasonable to report these values, especially as a complement to the observational estimates, but the length allocated makes them seem primary. [Robert Pincus, USA] | we now do a more careful job in saying that some estimates from climate models, like the multi-framework model, the ones that also consider secondary aerosol-cloud-interactions such as taking aerosol effects on mixed-phase or convective clouds into account, have more skill. We now also consider AFaci+ari from the CMIP5 models that are used for time evolutions. |
| 7-1274 | 7 | 48 | 8 | 48 | 15 | The fraction of forcing exerted by aerosols and clouds due to down-welling long-wave component of solar radiation needs to be quantified. [Panuganti China Sattilingam Devara, India] | Figure 2 of Lohmann et al., ACP, 2010 shows that the LW forcing of the formerly called indirect forcing is at most 0.05 W/m2 |
| 7-1275 | 7 | 48 | 8 | 48 | 15 | What about direct effects? Might need to restate what iRF and iAF mean. Not clear if the reader jumps to here. At least reference where defined. [Andrew Gettelman, USA] | direct effects are included in AF; we now call it AFaci+ari |
| 7-1276 | 7 | 48 | 8 | 50 | 35 | This also shows the models are not much use. [VINCENT GRAY, NEW ZEALAND] | see above (response to Robert Pincus) |
| 7-1277 | 7 | 48 | 12 | | | “no changes in clear sky”: It is crucially important that the iAF values that include the aerosol direct effect are clearly labelled as such and disentangled from those that purely investigate the indirect effect. Depending on the simulated direct effect, the two quantities can differ by a factor of two. It would be less confusing if for total aerosol forcings a term different from iAF (where the “i” is interpreted as “indirect”) would be chosen. [Johannes Quaas, Germany] | done |
| 7-1278 | 7 | 48 | 17 | 48 | 19 | About the sentences: "Ensemble-averaged global-mean model estimates of the iRF have remained rather constant over time (Figure 7.13a) and amount to roughly –1 W m–2 . This estimate is obtained from the average over all published estimates, treating each of them as equal (one vote per model per paper)". In relation to a similar subject, could you inform the Reviewers (and if it is possible in the AR5-WGI), how the selection of articles published or submitted for publication was made? From the list of references of the Chapters and items that I analyzed, I saw that there are very few articles from the Physical Review or the Physical Review Letters, two very recognized Journals. I suggest to include in this Chapter (or in another ones), the following published work: Pablo F. Verdes. Global Warming Is Driven by Anthropogenic Emissions: A Time Series Analysis Approach. Physical Review Letters, 99, 048501-1, 2007. Abstract:The solar influence on global climate is nonstationary. Processes such as the Schwabe and Gleissberg cycles of the Sun, or the many intrinsic atmospheric oscillation modes, yield a complex pattern of interaction with multiple time scales. In addition, emissions of greenhouse gases, aerosols, or volcanic dust perturb the dynamics of this coupled system to different and still uncertain extents. Here we show, using two independent driving force reconstruction techniques, that the combined effect of greenhouse gases and aerosol emissions has been the main external driver of global climate during the past decades. [Rubén D Piacentini, Argentina] | the mentioned paper has no estimate of the total aerosol effect, it only includes the direct effect and therefore it is not considered in Figure 7.18 |
| 7-1279 | 7 | 48 | 17 | 48 | 35 | I think the iRF reported by most of the global modes does not include aerosol effects on convective clouds. A note should be made for this. [Jiwen Fan, United States of America] | taken into account |
| 7-1280 | 7 | 48 | 17 | 48 | 35 | Maybe better to state that there is some variance as the methods have changed over time, rather than focus on dates of publication. A bit unclear whether it is publication date or actual date for evaluating a forcing in models. [Andrew Gettelman, USA] | it's the publication date as noted in the figure caption; stating that the method have changed is a good point. |
| 7-1281 | 7 | 48 | 17 | | | The fact that all estimates of iRF/iAF ever published are treated equally is strange. In many cases, groups have revised their implementation of aerosol-cloud interactions, and thus newer results should be regarded as an update of previous ones, rather than as an equally likely estimate. Also, some models are known to be biased, e.g. because they prescribe aerosols, often as monthly-mean distributions, or because they only consider sulfur as an anthropogenic component, or because they neglect important natural sources relevant in the pre-industrial reference. Such estimates exaggerate the iRF, a problem that should be taken into account in the assessment. Other models use a minimum value of CDNC and thus diminish the simulated iRF in an | disgarded because there is no good basis for weighting models. What we did do and which we now state more clearly is that from each paper only the best estimate per model is taken (and not sensitivity studies). |

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| | | | | | | arbitrary way. These estimates cannot be taken with equal weight. [Johannes Quaas, Germany] | |
| 7-1282 | 7 | 48 | 17 | | | There should be an explanation why estimates that would lead to a negative total forcing are included. [Johannes Quaas, Germany] | taken into account |
| 7-1283 | 7 | 48 | 17 | | | "Ensemble-averaged global-mean model estimates of the iRF have remained rather constant over time (Figure 7.13a)"; not immed clear whether "over time" refers to secular change in the forcing or secular change in the estimate. I think the latter, though it is not possible to tell this from the figure, as the date is not shown in the figure (as it was in Lohmann's paper). [Stephen E Schwartz, USA] | wording changed |
| 7-1284 | 7 | 48 | 20 | 48 | 20 | Please explain shortly the differences in weighting procedure and give also a short explanation why it has been changed. [Andrew Ferrone, Germany] | the -0.7 W/m2 in AR4 was based on an expert judgement, whereas here the best estimate per publication and model is taken |
| 7-1285 | 7 | 48 | 22 | 48 | 23 | For clarity, please add "of -1 W m-2" after "within 0.15 W m-2" [Jón Egill Kristjánsson, Norway] | Agreed |
| 7-1286 | 7 | 48 | 23 | 48 | 23 | The dates cited (2001 to 2006) seem to relate to the publication date of the different studies. Please clarify this point as it might be misunderstood as a variability in the physical system during the period 2001 to 2006. [Andrew Ferrone, Germany] | taken into account |
| 7-1287 | 7 | 48 | 30 | 48 | 30 | "did show" is strange wording. Better: "have shown" [Jón Egill Kristjánsson, Norway] | wording changed |
| 7-1288 | 7 | 48 | 31 | | 35 | The uncertainty in forcing indicated here, e.g., the 1.3 W m-2 from Storelvmo, is of great consequence. It feeds into the uncertainty in forcing over the industrial period and ultimately into understanding of climate sensitivity that impacts ultimately on policy formulation. The great importance of this uncertainty assessment would seem to require some additional gravitas here. [Stephen E Schwartz, USA] | the importance of the uncertainty is now discussed more prominently |
| 7-1289 | 7 | 48 | 32 | 48 | 32 | Please add "estimates" after "in the iRF" [Jón Egill Kristjánsson, Norway] | Agreed |
| 7-1290 | 7 | 48 | 34 | 48 | 34 | The list of references concerning susceptibility of couds and its dependence on assumed minimum CDNC may be extended with at least two earlier works (Lohmann and Feichter, 2005; Kirkevåg et al., 2008; Hoose et al., 2009). [Alf Kirkevåg, Norway] | will be considered if space permits |
| 7-1291 | 7 | 48 | 35 | 48 | 35 | Please add a reference after "observational constraints" [Jón Egill Kristjánsson, Norway] | Agreed |
| 7-1292 | 7 | 48 | 35 | | | "observational constraints"; not sure what is meant here. same as ch 7, p 49 line 8? Still not sure what the constraints are. [Stephen E Schwartz, USA] | yes. Sentence rephrased |
| 7-1293 | 7 | 48 | 37 | 48 | 39 | It occurs to me that the "cloud lifetime effect" concept is abandoned in this report, but it is used here again; absorbing aerosols do not always produce a "reduction" in cloud cover (e.g., McFarquhar and Wang, 2006; please refer to comment #12) [Hailong Wang, USA] | check residual usage of "lifetime effect" |
| 7-1294 | 7 | 48 | 37 | 48 | 40 | Lee et al. (2009) shows that in deep convective clouds, ~ 30% of an increase in reflected shortwave radiation by clouds is offset by an decrease in outgoing longwave radiation and this is substantially larger than the offset of ~ 1% in stratocumulus clouds with a 10-fold increase in aerosol concentration. This can be one of adjustments here and this type of adjustment is controlled by cloud-scale feedbacks among aerosol, dynamics and microphysics not represented in climate models.<br><br>Reference:<br><br>Lee, S. S., L. J. Donner, and V T J Phillips, 2009: Sensitivity of aerosol and cloud effects on radiation to cloud types: comparison between deep convective clouds and warm stratiform clouds over one-day period. Atmos. Chem. Phys., 9, 2555-2575.<br><br>[Seoung Soo Lee, United States of America] | Disregarded because we focus on global studies in this section. |
| 7-1295 | 7 | 48 | 38 | 48 | 39 | "reduction in cloud cover due to absorption of solar radiation by BC or other absorbing aerosols (semi-direct | Let's carefully consider how tightly we want to define |

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| | | | | | | effect)" No model study of the semidirect radiative focing has included either Cloud Absorption Effect I or II, which are defined as the effects on cloud heating of absorbing inclusions in hydrometeor particles and of absorbing aerosol particles interstitially between hydrometeor particles at their actual relative humidity (RH), respectively. These are not part of the semidirect effect (Jacobson, M.Z., Investigating cloud absorption effects: Global absorption properties of black carbon, tar balls, and soil dust in clouds and aerosols, J. Geophys. Res., doi:10.1029/2011JD017218, in press, 2012, http://www.agu.org/pubs/crossref/pip/2011JD017218.shtml) yet have been (at least CAE I) included in climate response studies to date (Jacobson, M.Z., Effects of absorption by soot inclusions within clouds and precipitation on global climate, J. Phys. Chem., 110, 6860-6873, 2006; Jacobson, M.Z., Short-term effects of controlling fossil-fuel soot, biofuel soot and gases, and methane on climate, Arctic ice, and air pollution health, J. Geophys. Res., 115, D14209, doi:10.1029/2009JD013795, 2010).The omission of cloud absorption effects should be stated as a cause of error in the overall forcing estimates from these studies. [Mark Z. Jacobson, U.S.A.] | the semidirect effect and be consistent. |
| 7-1296 | 7 | 48 | 52 | | | mixed phase and cirrus? [Andrew Gettelman, USA] | the important new aspect in these studies are the effects on mixed-phase clouds |
| 7-1297 | 7 | 48 | 53 | 48 | 53 | Please add "then" between "average" and "amounts" [Jón Egill Kristjánsson, Norway] | Agreed |
| 7-1298 | 7 | 48 | 53 | 48 | 53 | Please add "in magnitude" between "larger" and "than" [Jón Egill Kristjánsson, Norway] | Agreed |
| 7-1299 | 7 | 48 | 56 | | | "polluted climate"; I dont think the authors mean this. Perhaps better "locations with high CCN concentration" or the like. [Stephen E Schwartz, USA] | Agreed |
| 7-1300 | 7 | 49 | 3 | 49 | 3 | The longwave warming doesn't reduce the shortwave cooling. It compensates for the shortwave cooling, or reduces the net IAF. [Steven Ghan, USA] | TRUE |
| 7-1301 | 7 | 49 | 5 | 49 | 6 | I would suggest to change this first sentence. The rather large iAF for this category mainly comes from the estimates of Menon and Rostayn (2006) which incorporated a very crude parameterization of aerosol effects on convective clouds. We just do not have enough model results to draw any conclusion yet. Wang et al. (2011, ACP) use the PNNL-MMF to esimate the aerosol indirect forcing for both stratiform and convective clouds, and found a much smaller forcing than that estimated from the host model CAM5. [Minghuai Wang, United States of America] | The study of Wang et al. 2011 (ACP) is included. |
| 7-1302 | 7 | 49 | 5 | 49 | 7 | Wang et al. (2011b) find that an aerosol-MMF (in which aerosols influence droplet formation in cloud resolving models embedded within each grid cell of a GCM) produced an iAF about half as large as a GCM with the same representation of aerosols but parameterized convective clouds. This result contradicts the first sentence, and provides a much stronger basis for representing aerosol effects on convective clouds. Subsequent work recently submitted demonstrates that the aerosol-MMF produces a much more realistic simulation of the sensitivity of liquid water path to changes in CCN. This important work should be discussed here. I suggest the following text to replace this paragraph with the following: "The iAF increases substantially in magnitude in most estimates that allow aerosol particles to change convective clouds, but those estimates are highly uncertain because they are based on cumulus parameterizations. A more reliable and much smaller estimate (-0.77 W m-2) is from a GCM with a superparameterization of clouds (see section 7.2.3.5.2) that treats cloud-aerosol interactions (Wang et al., 2011b), which is consistent with the other estimates if they are scaled to match constraints from satellite retrievals." [Steven Ghan, USA] | good point, taken into account |
| 7-1303 | 7 | 49 | 11 | | 23 | "inverse estimates". This of course assumes the answer, i.e., equilibrium or transient sensitivty. So it is really illegitimate in this chapter, especially so if it is then used to infer the equilibrium sensitivity (circular reasoning; Rodhe, 2000; Curry and Webster, BAMS, 2011).<br><br>Perhaps the equilibrium sensitivity that led to the individual estimates of the total AF (and in turn iAF) should be specified, or even plotted against the iAF to see if anticorrelated. In any event it must be made clear that these are not to be viewed as independnt estimates of the iAF.<br><br>The language "This is likely reflects limitations in our ability to parameterize clouds, aerosols, and aerosol- | good point, taken into account |

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| | | | | | | cloud interactions in GCMs." could equally well read "This likely reflects assumptions about equilibrium climate sensitivity as much as or more than it reflects on aerosol forcing." Once again watch first person plural. It is not the chapter authors' ability (at least with their Assessors' hats on).<br><br>Rodhe, H., Charlson, R.J. and Anderson, T.L. (2000). Avoiding circular logic in climate modeling. Climatic Change 44, 419-422. [Stephen E Schwartz, USA] | |
| 7-1304 | 7 | 49 | 13 | | | Were volcanic aerosols considered in these studies (or rather: in which of these were they considered)? [Johannes Quaas, Germany] | there is a 2012 paper that considers this, which is included in the revised figure |
| 7-1305 | 7 | 49 | 22 | 49 | 22 | Remove "is" between "This" and "likely" [Jón Egill Kristjánsson, Norway] | done |
| 7-1306 | 7 | 49 | 25 | 49 | 25 | "Because GCMs tend to include negative forcings but not positive ones" is a strange claim. It should be either removed or substantiated. [Jón Egill Kristjánsson, Norway] | sentence has been reworded |
| 7-1307 | 7 | 49 | 25 | | | Some GCMs include positive effects (ice clouds) [Andrew Gettelman, USA] | sentence has been reworded |
| 7-1308 | 7 | 49 | 25 | | | summarising paragraph: "The studies that take satellite data into account" earn their credibility due to the evaluation with the observational datasets. The credibility of estimates purely relying on models is, however, questionable. [Johannes Quaas, Germany] | sentence has been reworded |
| 7-1309 | 7 | 49 | 25 | | | "negative forcings"; I presume "negative aerosol forcings" is what is meant. [Stephen E Schwartz, USA] | yes, sentence has been changed |
| 7-1310 | 7 | 49 | 26 | | | Larger than some small-scale studies; other small-scale with an idealised set-up produce a very large effect, particularly if precipitation is involved. [Johannes Quaas, Germany] | sentence has been reworded |
| 7-1311 | 7 | 49 | 28 | 49 | 31 | I am confused. What doe these different estimates and bounds mean [Andrew Gettelman, USA] | clarified |
| 7-1312 | 7 | 49 | 29 | 48 | 29 | Why are there two lower bounds given here for iRF? [Andrew Ferrone, Germany] | clarified |
| 7-1313 | 7 | 49 | 36 | 49 | 40 | At first read I did not understand this at all. iRF and iAF? "lower bound" for negatives is confusing. For iAF, the bound seems weighted by satellite estimates you already said were biased? [Andrew Gettelman, USA] | clarified |
| 7-1314 | 7 | 49 | 36 | 49 | 40 | The highest value of both the iRF and iAF is "likely" -0.2 W/m2. But in Figure 7.13b, none of the 33 precentiles reach above -0.5 W/m2 and it is not clear that any of the 5 percentiles reach -0.2 W/m2. Perhaps I am missing something (quite possible), but otherwise please clarify. [Warren Richard Leaitch, Canada] | clarified |
| 7-1315 | 7 | 49 | 43 | 49 | 43 | About Figure 7.13a: i) In this figure (as well as in b), there is no horizontal bar for the mean value of the iAF-inverse (light blue) rectangle, at the end part of the figure; ii) concerning the following expression in the figure 13a caption: "The iRF studies from GCMs are divided into those published prior to TAR: iRF-TAR", there is no data in this figure concerning this period; iii) in relation to the expression: "cloud lifetime effect", the same comment as before, in the sense that it is a non- appropriate word to describe the effect; iv) the words that explain the colour codes do not have an exact correspondence with the text in the figure caption. [Rubén D Piacentini, Argentina] | clarified |
| 7-1316 | 7 | 49 | 43 | | | The lengthy list of papers in each category does not belong in a figure caption. [Robert Pincus, USA] | Editorial - Presentation in compliance with WGI style guide |
| 7-1317 | 7 | 49 | 47 | 49 | 47 | Correct the Reference "Boucher and Lohmann 1995" to "Boucher and Lohmann, 1995" [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1318 | 7 | 49 | 48 | 49 | 48 | Correct the References "Kaufman and Chou 1993" and "Rotstayn 1999" to "Kaufman and Chou, 1993" and "Rotstayn, 1999" [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1319 | 7 | 49 | 50 | 49 | 51 | Correct the References "Kristjansson 2002", "Quass and Boucher 2005" and "Rotstayn and Penner 2001" to "Kristjansson, 2002", "Quass and Boucher, 2005" and "Rotstayn and Penner, 2001" [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |

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| 7-1320 | 7 | 49 | 52 | 49 | 52 | Correct the Reference "Kvalevag and Myhre 2007" to "Kvalevag and Myhre, 2007"  [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1321 | 7 | 49 | 53 | 49 | 53 | Correct the References "Rotstayn and Liu 2009" and "Storelvmo 2011" to "Rotstayn and Liu, 2009" and "Storelvmo, 2011"  [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1322 | 7 | 49 | 54 | 49 | 54 | Correct the Reference "Wang and Penner 2009" to "Wang and Penner, 2009"  [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1323 | 7 | 49 | 56 | 49 | 56 | Correct the Reference "Lohmann 2002b" to "Lohmann, 2002b"  [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1324 | 7 | 49 | 57 | 49 | 57 | Correct the Reference "Peng and Lohmann 2003" to "Peng and Lohmann, 2003"  [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1325 | 7 | 49 | 58 | 49 | 58 | Correct the References "Rotstayn 1999" and "Rotstayn and Liu 2005" to "Rotstayn, 1999" and "Rotstayn and Liu, 2005"  [Panuganti China Sattilingam Devara, India] | Ah the comma. |
| 7-1326 | 7 | 49 | | 50 | | Figure 7.13a: I realize that the work of many groups has gone into this figure, but perhaps we could try to confine the citations to the in-text discussion of the figure as opposed to the caption? [Jeffrey Taylor, United States of America] | Editorial - Presentation in compliance with WGI style guide |
| 7-1327 | 7 | 50 | 2 | 50 | 2 | Correct the Reference "Rotstayn and Liu 2009" to "Rotstayn and Liu, 2009"  [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1328 | 7 | 50 | 4 | 50 | 4 | Correct the Reference "Lohmann and Lesins 2002" to "Lohmann and Lesins, 2002" [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1329 | 7 | 50 | 7 | 50 | 8 | Correct the References "Harvey and Kaufmann 2002", "Libardoni and Forest 2011" and "Shindell and Faluvegi 2009" to "Harvey and Kaufmann, 2002", "Libardoni and Forest, 2011" and "Shindell and Faluvegi, 2009" [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1330 | 7 | 50 | 10 | 50 | 10 | Correct the Reference "Lohmann and Feichter 2001" to "Lohmann and Feichter, 2001"  [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1331 | 7 | 50 | 11 | 50 | 11 | Correct the Reference "Posselt and Lohmann 2009" to "Posselt and Lohmann, 2009"  [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1332 | 7 | 50 | 13 | 50 | 13 | Correct the References "Jacobson 2006", Lohmann 2004" and Lohmann and Diehl 2006" to "Jacobson, 2006", Lohmann, 2004" and Lohmann and Diehl, 2006"  [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1333 | 7 | 50 | 15 | 50 | 15 | Correct the Reference "Lohmann 2008"  to "Lohmann, 2008" [Panuganti China Sattilingam Devara, India] | Ah, the comma.. |
| 7-1334 | 7 | 50 | 16 | 50 | 16 | Correct the References "Menon and Rotstayn 2006" and Menon and DelGenio 2007" to "Menon and Rotstayn, 2006" and Menon and DelGenio, 2007"  [Panuganti China Sattilingam Devara, India] | Ah the comma. |
| 7-1335 | 7 | 50 | 37 | 52 | 37 | Admittedly this is not my area of expertise, but my understanding of the whole cosmic ray - cloud problem is that cosmic rays can indeed nucleate aerosols, but that they do so at sizes several orders of magnitude too small to act as CCN themselves and they have no ready way to grow to CCN size.  If this is correct, it should be stated clearly in the synthesis paragraph (p. 52, lines 32-37).  If the explanation is something different, then whatever the physical explanation is, it should be stated clearly in the synthesis paragraph.  [Anthony Del Genio, USA] | The synthesis paragraph was modified in ordet to better bring out our main messages. |
| 7-1336 | 7 | 50 | 37 | 52 | 37 | This is a well written, concise section; the first, intro para was especially well written.  The phrase "medium evidence" p 52 line 33 is awkward; not sure what it is trying to convey: not strong but not weak either. Have to get off the fence and say what you mean.  [Stephen E Schwartz, USA] | I think this terminology is standard IPCC |
| 7-1337 | 7 | 50 | 37 | | | Section 7.4.7: comment regarding AR5's near complete omission of the massive evidence for a solar-magnetic climate driver | There is ample evidence that the solar forcing does not show up strongly in detection and attribution studies, the latest example being found in Lockwood |

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| | | | | | | My training is in economics where we are very familiar with what statisticians call "the omitted variable problem" (or when it is intentional, "omitted variable fraud"). Whenever an explanatory variable is omitted from a statistical analysis, its explanatory power gets misattributed to any correlated variables that are included. This problem is manifest at the very highest level of AR5, and is built into each step of its analysis.<br><br>For the 1750-2010 period examined, two variables correlate strongly the observed warming (and hence with each other). Solar magnetic activity and atmospheric CO2 were both trending upwards over the period, and both stepped up to much higher levels over the second half of the 20th century. This pair of correlations with temperature change give rise to the two main competing theories of 20th century warming. Was it driven by rapidly increasing human release of CO2, or by the 80 year "grand maximum" of solar activity that began in the early 1920's. ("Grand minima and maxima of solar activity: new observational constraints," Usoskin et al. 2007.)<br><br>The empirical evidence in favor of the solar explanation is overwhelming. Dozens of peer-reviewed studies have found a very high degree of correlation (.5 to .8) between solar-magnetic activity and global temperature going back many thousands of years (Bond 2001, Neff 2001, Shaviv 2003, Usoskin 2005, and many others listed below). In other words, solar activity "explains," in the statistical sense, 50 to 80% of past temperature change.<br><br>Such a high degree of correlation over such long time periods implies causality, which can only go one way. Global temperature cannot be driving solar activity, so there must be some mechanism by which solar activity is driving or modulating global temperature change. The high degree of correlation also suggests that solar activity is the PRIMARY driver of global temperature on every time scale studied (which is pretty much every time scale but the Milankovitch cycle).<br><br>In contrast, CO2 and temperature records reveal no discernable warming effect of CO2. There is a correlation between CO2 and temperature, but with CO2 changes following temperature changes by an average of about 800 years (Caillon 2003), indicating that it is temperature change that is driving CO2 change (as it should, since warming oceans are able to hold less CO2). This does not rule out the possibility that CO2 also drives temperature, and in theory a doubling of CO2 should cause about a 1 degree increase in temperature before any feedback effects are accounted, but feedbacks could be negative, so there no reason, just from what we know about the greenhouse mechanism, that CO2 has to be a significant player. The one thing we can say is that whatever the warming effect of CO2, it is not detectable in the raw CO2 vs. temperature data.<br><br>This is in glaring contrast to solar activity, which lights up like a neon sign in the raw data. Literally dozens of studies finding .5 to .8 degrees of correlation with temperature. So how is it that the IPCC's current generation of general circulation models start with the ASSUMPTION that CO2 has done 40 times as much to warm the planet as solar activity since 1750? This is the ratio of AR5's radiative forcing estimates for variation in CO2 and variation in total solar effects listed in table 8.9 on page 8-45. RF for CO2 is entered as 2.79 W/m^2 while RF for total solar effects is entered as .07 W/m^2. The 50% driver of global temperature according to mountains of temperature correlation data is ASSUMED to have 1/40th the warming effect of something whose warming effect is not even discernable in the temperature record. And this is on the INPUT side of the GCM's. The models aren't using gigaflops of computing power to FIND that CO2 has that much larger a warming effect. The warming ratio is fixed at the outset. Garbage in, garbage out.<br><br>The "how" is very simple. The 40 times greater warming effect of CO2 is achieved by blatant omitted variable fraud. As I will fully document, all of the evidence for a strong solar magnetic driver of climate is simply left out of AR5. Of the many careful empirical studies that show a high correlation between solar activity and climate, not a single one is even mentioned ANYWHERE in the First Order Draft. On page 7-50, line 52, there is a single reference to a single paper (Kirkby 2007) where the text suggests some correlation between solar activity and climate, but it fails to mention even that the correlation to temperature is positive, never mind its dramatic magnitude, or the numerous repeated findings of this result. And that's it. One oblique reference in the entire report. A person reading AR5 from cover to cover would come away with not even a hint that for more than ten years a veritable flood of studies have been finding solar activity to explain something on the | (2012). For the rest we do not reply to insulting comments. |

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| | | | | | | order of half of all past temperature variation. It is COMPLETELY omitted. [Sorry for using ALL CAPS for emphasis but Excel is not letting me use italics.]<br><br>As a result, AR5 misattributes virtually all of the explanatory power of solar-magnetic activity to the correlated CO2 variable. This misattribution can be found both in AR5's analytical discussions and in its statistical estimations and projections, and the error could not be more consequential. If it is solar-magnetic activity that drives climate then the sun's recent descent into a state of profound quiescence portends imminent global cooling, possibly rapid and severe, and unlike warming, cooling is actually dangerous, and really can feed back on itself in runaway fashion.<br><br>Nothing could be more perverse in such a circumstance than to unplug the modern world in a misbegotten jihad against CO2. The IPCC's omitted variable fraud must stop. AR5's misattribution of 20th century warming to CO2 must stop. The EVIDENCE overwhelmingly supports the solar-magnetic warming theory. The only support for the CO2 theory is the fact that models built on it can achieve a reasonable fit to the last couple centuries of temperature history, but that is only because CO2 is roughly correlated with solar activity over this period, while these models themselves are invalidated by their demonstrable omitted variable fraud. If warming is attributed to solar-magnetic effects at all in accordance with the evidence then the warming that is left to attribute to CO2 becomes utterly benign.<br><br>With natural temperature variation almost certainly both substantially larger than CO2 effects, and headed in the cooling direction, the expected external value of CO2 is unambiguously positive. If anything, we should subsidizing and promoting increases in atmospheric CO2, exactly the opposite of the Executive Summary's opening claim that developments since AR4 "further strengthen the basis for human activities being the primary driver in the concerns about climate change." (Page 1-2, lines 4-5.)<br><br>As someone who recognizes the scientific errors in this disastrous report, I can at least make sure that the issue is put properly before the authors of AR5. Thus I am documenting as concisely as possible the solar-magnetic omission and the errors it leads to. The discussion is substantial but I have kept it well under the character limit for a single comment. This comment is being submitted as a top-level comment on AR5 as a whole, and it is being submitted unaltered as a comment on three different sub-chapter headings where the omitted solar-magnetic evidence ought to be taken into account (on FAQ 5.2 starting on page 5-43, on section 7.4.7 starting on page 7-50, and on table 8.6 starting on page 8-45).<br><br><br>A sample of the omitted evidence<br><br>Listed below are a few of the most prominent and compelling studies that have found a high correlation between solar activity and climate, together with a semi-random collection of similar findings, totaling two dozen citations all together. It would be easy to list two dozen more, but the purpose here is just to show a sample of the omitted evidence, to document up-front the existence and validity of it. Included are brief descriptions of the findings for about ten of the studies. None of the observed correlations are reported anywhere in AR5. The first four are the ones I mentioned above:<br><br>Bond et al. 2001, "Persistent Solar Influence on North Atlantic Climate During the Holocene," Science.<br><br>Excerpt from Bond: "Over the last 12,000 years virtually every centennial time scale increase in drift ice documented in our North Atlantic records was tied to a distinct interval of variable and, overall, reduced solar output."<br><br>Neff et al. 2001, "Strong coherence between solar variability and the monsoon in Oman between 9 and 6 kyr ago," Nature.<br><br>Finding from Neff: Correlation coefficients of .55 and .60. | |

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| | | | | | | Usoskin et. al. 2005, "Solar Activity Over the Last 1150 years: does it Correlate with Climate?" Proc. 13th Cool Stars Workshop.<br><br>Excerpt from Usoskin: "The long term trends in solar data and in northern hemisphere temperatures have a correlation coefficient of about 0.7 — .8 at a 94% — 98% confidence level."<br><br>Shaviv and Veizer, 2003, "Celestial driver of Phanerozoic climate?" GSA Today.<br><br>Excerpt from Shaviv: "We find that at least 66% of the variance in the paleotemperature trend could be attributed to CRF [Cosmic Ray Flux] variations likely due to solar system passages through the spiral arms of the galaxy." [Not strictly due to solar activity, but implicating the GCR, or CRF, that solar activity modulates.]<br><br>Plenty of anti-CO2 alarmists know about this stuff. Mike Lockwood and Claus Fröhlich, for instance, in their 2007 paper: "Recent oppositely directed trends in solar climate forcings and the global mean surface air temperature" (Proc. R. Soc. A), began by documenting how "[a] number of studies have indicated that solar variations had an effect on preindustrial climate throughout the Holocene." In support, they cited 17 papers: the Bond and Neff articles from above, plus Davis & Shafer 1992; Jirikowic et al. 1993; Davis 1994; vanGeel et al. 1998; Yu&Ito 1999; Hu et al. 2003; Sarnthein et al. 2003; Christla et al. 2004; Prasad et al. 2004; Wei & Wang 2004; Maasch et al. 2005; Mayewski et al. 2005; Wang et al. 2005a; Bard & Frank 2006; and Polissar et al. 2006.<br><br>The correlations in a lot of these papers are not directly to temperature. They are to temperature proxies, some of which have a complex relationship with temperature, like Neff 2001, which found a correlation between solar activity and rainfall. Even so, the correlations tend to be strong, as if the whole gyre is somehow moving in broad synchrony with solar activity.<br><br>Some studies do examine correlations between solar activity proxies and direct temperature proxies, like the ratio of Oxygen18 to Oxygen16 in geologic samples. One such study was highlighted in Kirkby 2007. Mangini et. al. 2005, "Reconstruction of temperature in the Central Alps during the past 2000 yr from a δ18O stalagmite record," found:<br><br>Excerpt from Mangini: "... a high correlation between δ18O in SPA 12 and D14C (r =0.61). The maxima of δ18O coincide with solar minima (Dalton, Maunder, Sporer, Wolf, as well as with minima at around AD 700, 500 and 300). This correlation indicates that the variability of δ18O is driven by solar changes, in agreement with previous results on Holocene stalagmites from Oman, and from Central Germany."<br><br>And that's just old stuff. Want some new stuff? Here are four random recent papers.<br><br>Ogurtsov et al, 2010, "Variations in tree ring stable isotope records from northern Finland and their possible connection to solar activity," JASTP.<br><br>Excerpt from Ogurtsov: "Statistical analysis of the carbon and oxygen stable isotope records reveals variations in the periods around 100, 11 and 3 years. A century scale connection between the 13C/12C record and solar activity is most evident."<br><br>Di Rita, 2011, "A possible solar pacemaker for Holocene fluctuations of a salt-marsh in southern Italy," Quaternary International.<br><br>Excerpt from Di Rita: "The chronological correspondence between the ages of saltmarsh vegetation reductions and the minimum concentration values of 10Be in the GISP2 ice core supports the hypothesis that important fluctuations in the extent of the salt-marsh in the coastal Tavoliere plain are related to variations of solar activity."<br><br>Raspopov et al, 2011, "Variations in climate parameters at time intervals from hundreds to tens of millions of | |

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| | | | | | | years in the past and its relation to solar activity," JASTP.<br><br>Excerpt from Raspopov: "Our analysis of 200-year climatic oscillations in modern times and also data of other researchers referred to above suggest that these climatic oscillations can be attributed to solar forcing. The results obtained in our study for climatic variations millions of years ago indicate, in our opinion, that the 200-year solar cycle exerted a strong influence on climate parameters at those time intervals as well."<br><br>Tan et al, 2011, "Climate patterns in north central China during the last 1800 yr and their possible driving force," Clim. Past.<br><br>Excerpt from Tan: "Solar activity may be the dominant force that drove the same-phase variations of the temperature and precipitation in north central China."<br><br>Saltmarshes, precipitation, "oscillations." It's all so science-fair. How about something just plain scary?<br><br>Solheim et al. 2011, "Temperature prognosis based on long sunspot cycle 23," (not sure if this has been published yet, but you can find it here: http://www.au.agwscam.com/pdf/SolheimSolarTemperature.pdf).<br><br>Excerpt from Solheim: "We find that for the Norwegian local stations investigated that 30-90% of the temperature increase in this period may be attributed to the Sun. For the average of 60 European stations we find ≈ 60% and globally (HadCRUT3) ≈ 50%. The same relations predict a temperature decrease of ≈ 0.9°C globally and 1.1−1.7°C for the Norwegian stations investigated from solar cycle 23 to 24."<br><br><br>First Chapter 5 error: omitting all solar variables besides TSI<br><br>Chapter 5, the paleo observations chapter, is the right place for the evidence for a solar-magnetic climate driver to be introduced because most of this evidence is obtained from the deposition of cosmogenic isotopes in various paleologic strata: ice cores, geologic cores and tree rings. When solar activity is strong, less galactic cosmic radiation (GCR) is able to penetrate the solar wind and reach earth, so variation in cosmogenic isotopes found in time-dated strata serves as a proxy for solar activity. But when chapter 5 does get around to looking at cosmogenic records, it only looks at how they can be used to reconstruct total solar irradiance (TSI). It never even hints at the flood of studies that show a high degree of correlation between solar activity and various paleo proxies for climate and temperature!<br><br>This occurs under the subheading "FAQ 5.2: Is the Sun a Major Driver of Climate Changes?" which is placed as an addendum to Chapter 5, starting on page 5-43. This FAQ mentions the long-period chang in TSI that come with orbital variation (Milankovitch cycles), a factor which hasn't changed enough since 1750 to account for any significant amount of the warming since that date. Neither can TSI be responsible for significant recent warming because, as solar activity jumps dramatically up and down over the roughly 11 year solar cycle, TSI is known to remain remarkably stable, varying only .1 to .2% (as noted on page 5-43, line 53).<br><br>Thus, concludes FAQ 5.2, solar variation cannot be responsible for any significant amount of the warming since 1750. But it is only able to reach this conclusion by completely omitting any consideration those solar variables other than TSI that could be affecting global temperature. Unlike TSI, solar wind speed and pressure vary considerably over the solar cycle and between solar cycles. So do the Ap index and the F10.7cm radio flux progression. The GCR that the solar wind modulates, the neutron counts measured at Climax and Oulu and other locations, can vary by a full order of magnitude over the solar cycle. In contrast, TSI varies so little that it is called "the solar constant." If there is a mechanism by which solar variation is driving global temperature, it is most likely to work through those solar variables that actually vary significantly with solar activity. Yet the discussion in FAQ 5.2 pretends that these other solar variables do not even exist.<br><br>So that's the first error in FAQ 5.2: pretending to have addressed the range of possible solar effects while studiously neglecting to mention that there are a bunch of solar variables that, unlike TSI, vary tremendously | |

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| | | | | | | over the solar cycle and might affect our climate in ways that we do not yet understand. We in-effect live inside of the sun's "atmosphere," the extended corona created by the sun's magnetic field and the solar wind. AR5 simply assumes that this solar environment has no effect on global climate, and they do it by rank omission of the relevant variables. The omitted variable problems that result are not an accident. They are omitted variable fraud.<br><br>Second Chapter 5 error: the highly irrational assumption that temperature would be driven by the trend in solar activity rather than the level<br><br>Perhaps in an effort to justify ignoring all solar variables other than TSI, FAQ 5.2 ends with what it presents as a general reason to dismiss the possibility that solar variation made any significant contribution to late 20th century warming by ANY mechanism. Page 5-44, lines 25-28:<br><br>"[The sun can't be] a major driver of the climate changes over the past 40 years because instrumental TSI and SSI records contain no significant trend; whereas records of global mean temperature and GHG concentrations contain significant trends of increasing values. This lack of agreement in trends demonstrates that the Sun did not play a role during this period."<br><br>TSI peaks at the high point of the solar cycle, just as the other solar variables do, so no matter what solar variable you look at, it can't have been the cause of recent warming, because these variables showed no upward trend over this period, right? Wrong. That's like saying you can't heat a pot of water by turning the flame to maximum and leaving it there, that you have to turn up the flame slooooowly if you want the water to heat. It is incredible to see something so completely unscientific in AR5, passing as highly vetted science.<br><br>And the "flame" DID stay on maximum. Again, there was an 80 year "grand maximum" of solar activity starting in the early 1920's (Usoskin 2007). AR5 is in-effect assuming that the oceans had already equilibrated to whatever temperature forcing effect this high level of solar activity might have. Otherwise the continued temperature forcing from the continued high level of solar activity would have caused continued warming.<br><br>Claims of rapid ocean equilibration have been made (Schwartz 2007), but they don't stand up to scrutiny. In order to get his result, Schwartz used an energy balance model with the oceans represented by a single heat sink. That is, he assumed that the whole ocean changed temperature at once! Once you move to a 2 heat sink model where it takes time for heat to transfer from one ocean layer to another (Kirk-Davidoff 2009), it becomes clear that the rapid temperature adjustment of the ocean surface tells us next to nothing about how long it takes for the ocean to equilibrate to a long term forcing.<br><br>The paleo-temperature record is typified by multi-century warming and cooling phases, suggesting that equilibration can easily take centuries, making it ludicrous to assume that the warming effect of a grand maximum that began in the 1920's must have been spent by 1970 or 1980 or by ANY particular date.<br><br>So no, there is no way to save the utterly incompetent argument in FAQ 5.2 that a solar driver of temperature can only cause warming when it is on the increase. If solar wind pressure or GCR does in some way drive global temperature, there is every reason to believe that it would have continued to warm the planet for as long as solar activity remained at grand maximum levels. There is NO EXCUSE for the IPCC to be omitting these variables, which are much more likely than TSI to be responsible for the high observed degree of correlation between solar activity and climate. For chapter 5 to be tenable, all of the now massive evidence that there is SOME mechanism by which solar activity is driving MOST temperature change must be laid out in full.<br><br>Technical note: misattribution is assigned manually in AR5, but the concept is the same as for purely statistical omitted variable fraud | |

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| | | | | | | If TSI and the other solar variables all move roughly together, won't omitting the solar variables other than TSI cause any explanatory power they might have to be attributed to TSI rather than CO2, since they are more closely correlated with TSI?<br><br>In a purely statistical estimation scheme yes, but the IPCC uses a combination of parameterized elements and estimated elements, and one of the elements that is parameterized is radiative forcings of CO2 and TSI, meaning that their relative warming effects are parameterized as well, with CO2 being assigned 40 times the warming effect of TSI over the 1750 to 2010 period.<br><br>This parameterization means that the explanatory power of the omitted solar magnetic variables gets attributed forty parts to CO2 for every one part to TSI. This structure forces the misattribution onto CO2. You can think of it a manual assignment of the misattribution.<br><br>The general concept of the omitted variable remains the same. There is only so much attribution for warming to go around (100%). If attribution is given to the solar-magnetic variables in accordance with the evidence from the historic and paleo records—at least 50%—then there less than 50% that can possibly be attributable to other causes.<br><br>Which again beings the scientific competence of IPCC into question. If CO2 has 40 times the warming effect of the 50% driver of global temperature (total solar effects), that makes it what? The 2000% driver of global temperature?<br><br><br>Chapter 7 inverts the scientific method, using theory to dismiss evidence<br><br>Where chapter 5 simply pretends that no solar variable other than TSI exists, Chapter 7 doesn't have that option. It is tasked to address directly the possibility that variables like the solar wind and GCR could be affecting climate. But Chapter 7 still comes up with a way to avoid mentioning any of the massive evidence that there must be SOME mechanism by which solar activity is driving climate. Just as it starts to touch on the subject, it jumps instead to examining the tenability of PARTICULAR THEORIES about the mechanism by which solar activity might drive climate.<br><br>This happens right at the beginning of section 7.4.7.1. "Correlations Between Cosmic Rays and Properties of Aerosols and Clouds." This is on page 7-50, lines 50-53:<br><br>"Many empirical relationships or correlations have been reported between GCR or cosmogenic isotope archives and some aspects of the climate system, such as SSTs in the Pacific Ocean (Meehl et al., 2009), some reconstruction of past climate (Kirkby, 2007) or tree rings (Dengel et al., 2009). We focus here on observed relationships between GCR and aerosol- and cloud-properties."<br><br>The first sentence of 7.4.7.1 is as close as AR5 comes to making any mention of overwhelming evidence that there is SOME mechanism by which solar activity drives global temperature. The Kirkby citation suggest some correlation between solar activity and climate, but what the correlation might be is completely obscured, and that's it. The second sentence effects the transition into looking at the evidence for particular theories of the mechanism involved. A short discussion later, the evidence for these particular mechanisms is asserted (quite tendentiously) to be "too weak" for the mechanisms to be "climatically-significant" (page 7-52, lines 33-35). This proclaimed weakness in turn becomes the rationale for omitting the mechanisms from the IPCC's general circulation models, and hence from the projections that are made with those models.<br><br>What do the AR5 draft authors do with the overwhelming evidence that there is SOME mechanism at work that makes solar magnetic the primary driver of global temperature? So they don't like the particular theories offered. They have to still acknowledge that SOME such mechanism must be at work, don't they? Ahh, but readers don't know about that evidence, because it was skipped over with that single oblique reference to Kirkby 2007, and AR5 continues as if the evidence doesn't exist. They never use it. They never mention it. | |

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| | | | | | | They never think about it. It is GONE. They declare their dissatisfaction with the available theories for how such a mechanism would work, and use this as an excuse to completely ignore the massive evidence that there is some such mechanism at work.<br><br>This is an exact inversion of the scientific method, which says that evidence always trumps theory. The IPCC is throwing away the evidence for a solar-magnetic driver of climate because it isn't satisfied with the theories that have been proposed to account for it. This is the DEFINITION of anti-science: putting theory (or ideology, or ANYTHING) over evidence. Evidence has to be the trump card, or its not science. The IPCC is engaged in actual, definitional, anti-science, exactly inverting the scientific method.<br><br>It is as if a pre-Newtonian "scientist" were to predict that a rock released into the air will waft away on the breeze, because we understand the force that the breeze imparts on the rock, but we have no good theory of the mechanism by which heavy objects are pulled to the ground. We should therefore ignore the overwhelming evidence that there is SOME mechanism that pulls heavy objects to the ground, and until such time as we can identify the mechanism, proceed as if no such mechanism existed. This is what the IPCC is actually doing with the solar-climate evidence. Y'all aren't scientists. You are pure, definitional, ANTI-SCIENTISTS.<br><br>More anti-science: Chapter 7 repeats the second Chapter 5 error<br><br>You know, that bit about thinking that a climate driver can only cause continued warming if its own level continues to increase? Chapter 7 says it again: just leaving a proposed climate driver on maximum can't possibly cause warming. From page 7-52, lines 35-37:<br><br>Moreover it should be noted that one study infers no trend in cosmic ray intensity over the last 50 years (McCracken and Beer 2007).<br><br>And that's the end of the section, AR5's punctuation mark on why solar activity and GCR should be dismissed as an explanation for late 20th century warming. This is anti-scientific in its own way. Scientists are supposed to be smart. They aren't supposed to think that you have to slowly turn up the flame under a pot of water in order to heat it. You could collect every imbecile in the world together and not a one of them would ever come up with the idea that they have to turn the heat up slowly. It's beyond stupid. It's like, insanely stupid. And multiple chapter-writing teams are proclaiming the same nonsense. Fruitcakes.<br><br>Okay, I guess that means I'm ready to wrap up. Y'all have taken all these tens of billions of dollars of research money and used it perpetrate a fraud. As I have documented above, you have perpetrated the grandest and most blatant example of omitted variable fraud in history, but so far only the skeptic half the world knows it. You still have a shot, before global cooling is an established fact, to make a rapid turn around and save some shred of your reputations. But if AR5 comes out insisting that CO2 is a dominant warming influence just as global cooling is becoming an established fact, then you all are finished on the spot. You'll still have your filthy lucre, but the tap is going to turn off, and your reputations will be destroyed forever.<br><br>Can you imagine a worse juxtoposition? And this is what the evidence says is going to happen, ALL of that evidence that you have been so studiously omitting. I'm eager for your embarrassment, but I would much rather see you save yourselves, so that the needed policy reversals can some that much sooner. The anti-CO2 policies that your fraudulent "science" has supported are right now destroying the world economy. You idiots are KILLING our future. Please wake up and try to save your own reputations before your lunatic anti-science ruins us all.<br><br>End comment<br>[Alec Rawls, United States] | |
| 7-1338 | 7 | 50 | 39 | 50 | 40 | No comment is made regarding how the heliosphere reduces the flux of GCRs reaching Earth; best to add a | adopted |

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| | | | | | | few words to clarify, along the lines of '...Earth's atmosphere by increasing the deflection of low energy GCR'. While this is a simple point and a basic explanation, I think it may be important for clarity due to the diverse readership. [Benjamin Laken, Spain] | |
| 7-1339 | 7 | 50 | 39 | | | "A high solar activity", would read more correctly as 'A high state of solar activity' or simply 'High solar activity'. [Benjamin Laken, Spain] | adopted |
| 7-1340 | 7 | 50 | 40 | | | "…(GCR) in the Earths atmosphere", this is incorrect, and should be changed to '…(GCR) impinging upon the Earths atmosphere'. Currently, the text gives the impression the cosmic rays are somehow within the atmosphere, rather than colliding with molecules at the top of the Earths atmosphere generating cascades of energetic subsequent particles. [Benjamin Laken, Spain] | adopted |
| 7-1341 | 7 | 50 | 40 | | | The statement 'it has been hypothesized that a lower flux of GCR would modify cloudiness in a way that would amplify…' is not strictly correct. The original suggestion is that the GCR flux is POSITIVLEY CORRELATED to cloud variations. This implies more than has been stated, i.e. it suggested that not only would low GCR be linked to cloud reductions, but also that increases in GCR correspond to increases in cloud. [Benjamin Laken, Spain] | ok we have deleted "a lower flux of" |
| 7-1342 | 7 | 50 | 48 | 52 | 28 | It seems to me that 7.4.7.2 should be arranged before 7.4.7.1. [Chien Wang, United States of America] | After careful consideration we decided to keep the order as it was. |
| 7-1343 | 7 | 50 | 50 | 50 | 53 | Indeed, a brief discussion and references to palaeoclimatic studies should be given as is attempted here, but it is currently not a good representation of the literature. Furthermore, the comment about Meehl et al. (2009) is simply not correct; this paper is about changes in solar irradiance and NOT the flux of GCR. Additionally, the Kirkby (2007) paper is a review, and does not provide a useful reference to the point for which it is cited ("some reconstructions of past climate"). I would recommend that these lines be altered, and you include citations to some of the following studies to establish palaeoclimatic evidence of a solar – climate link: Ram and Stolz, 1999, doi:10.1029/1999GL900199; Flietmann et al., 2003, doi: 10.1126/science.1083130; Bond et al., 2001, doi:10.1126/science.106580; Mauas et al. ( 2011) doi:10.1016/j.jastp.210.02.019. [Benjamin Laken, Spain] | reference to Meehl et al (2009) has been removed. References to Ram and Stolz (1999à and Bond et al (2001) have been added. |
| 7-1344 | 7 | 51 | 3 | | | . The note that Forbush Decreases happen over periods of days is not accurate, as they occur at sub-daily timescales also. Also, the description of Forbush decrease events currently given ('sporadic variations') provides almost no information as to why these events are useful: it is much better to say they are rapid onset, high magnitude low frequency reductions (of at least 3%) in the flux of GCR. [Benjamin Laken, Spain] | text has been modified but cannot go into great details. |
| 7-1345 | 7 | 51 | 4 | | | The point expressed here is that possibly correlations giving the impression of causality between the flux of GCR and clouds may arise from alternate sources. This is correct, however I think the point is currently under expressed here, as the point really is, possibly other solar parameters such as solar irradiance, UV irradiance, or heliospheric effects are influencing climate over co-temporal timescales, and due to the ambiguity and correlation of solar parameters we may mistake them. This point should be extended further to completion in the text. It may be helpful to add a citation to Laken, Kniveton & Wolfendale (2011) (doi: 10.1029/2010JD014900) which shows that Forbush decrease events are associated with changes in solar irradiance, and that it is likely that many FD-based studies have been influenced by these effects, making distinguishing causality between GCR and irradiance highly problematic. [Benjamin Laken, Spain] | adopted and reference cited. |
| 7-1346 | 7 | 51 | 7 | 51 | 35 | As further recent additions to the literature (and continuing the conflicting conclusions), Svensmark et al have a paper in open review in Atmos. Chem. Phys. Discuss., 12, 3595-3617, 2012 www.atmos-chem-phys-discuss.net/12/3595/2012/ doi:10.5194/acpd-12-3595-2012 while Laken et al are in press with Journal of Climate 2012 ; e-View doi: http://dx.doi.org/10.1175/JCLI-D-11-00306.1 . Both these papers use MODIS data but appear to draw different conclusions. [Richard Betts, United Kingdom of Great Britain & Northern Ireland] | thank you for the references. We will see if the Svensmark et al 2012 paper is published or not. The Laken et al 2012 paper is now cited. |
| 7-1347 | 7 | 51 | 7 | 52 | 35 | This paragraph needs to be shorter and more critical. The bottom line is that there are no rigorous, statistically significant studies showing significant effects. [Daniel Murphy, United States of America] | The paragraph was fully revised and considerably shortened |
| 7-1348 | 7 | 51 | 8 | | | The citation of Svensmark and FriisChristensen (1997) is incorrect in this context, as you have asserted that this study shows a 'low cloud – GCR correlation': it does not, it simply shows a correlation between TOTAL | comment adopted. |

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| | | | | | | (1,000 – 50 mb) ocean area geostationary satellite cloud only, which was subsequently found incorrect. A study which does show a low cloud – GCR correlation and is most appropriate to cite here (as they identified the low cloud restriction BEFORE the currently cited Marsh and Svensmark paper) is Pallé and Butler, (2000), doi: 10.1046/j.1468-4004.2000.00418.x. [Benjamin Laken, Spain] | |
| 7-1349 | 7 | 51 | 13 | | | The following statement is not correct, and is not supported by the given citation '…artefacts of the satellite data due to the solar cycle…(Pallé, 2005)'. The cited article does indeed comment on satellite artefacts, but they are simply instrumental and data-processing biases, and are NOT due to the solar cycle. [Benjamin Laken, Spain] | OK: comment adopted. |
| 7-1350 | 7 | 51 | 14 | 52 | 15 | I wasn't familiar with the 2006 Harrison paper and looked it up. It has serious methodological and statistical flaws and should not be cited. [Daniel Murphy, United  States of America] | the correlation is indeed weak, but volcanic eruptions cannot be blamed (the effect would of opposite sign, wouldn't they? And Fig. 2b shows a signal, albeit maybe not significant, that is more than for a particular month (Climat count around 3200). I think we should give the benefit of doubt to this study. |
| 7-1351 | 7 | 51 | 16 | 51 | 16 | "higher chance of being overcast …". Higher than what? Please explain. [Jón Egill Kristjánsson, Norway] | sentence has been deleted. |
| 7-1352 | 7 | 51 | 20 | 51 | 20 | "which they believe is associated with variations in cloudiness". This sounds very unscientific. We can not base the IPCC report on people's beliefs. Please rephrase. [Jón Egill Kristjánsson, Norway] | sentence modified. |
| 7-1353 | 7 | 51 | 23 | 51 | 23 | The correct spelling includes the ring over the "A", i.e., Ångström [JOHN OGREN, USA] | Corrected. |
| 7-1354 | 7 | 51 | 25 | | | The results of Svensmark et al (2009) have also been questioned by Čalogović et al. (2010); it is worth adding to the Laken et al. (2009) reference. [Benjamin Laken, Spain] | The citation was added to the text. |
| 7-1355 | 7 | 51 | 25 | | | "…Kristjánsson et al. (2008)…suggests a weaker impact of Forbush Decreases events on clouds over the Southern Ocean". This is incorrect, they suggest NO statistically significant impact of Forbush Decreases, despite testing for their effects in locations deemed to be theoretically the most sensitive to their effects. [Benjamin Laken, Spain] | The text was corrected as suggested. |
| 7-1356 | 7 | 51 | 28 | | | Suggest also adding a citation to Laken and Čalogović (2011) [doi: 10.1029/2011GL049764], which examined the isolated impacts of GCR reductions on widespread cloud changes and found no significant response. [Benjamin Laken, Spain] | The suggested citation was added to the text. |
| 7-1357 | 7 | 51 | 31 | | | Contention that only around 6 'large' FD events has occurred in the satellite era is illogical, with no size threshold on the FD event it is totally unclear where ~6 has come from! The NOAA website lists 142 individual FD events detected at Mt. Washington neutron monitor from 1970 – 1995 alone, many of which are 'large'. [Benjamin Laken, Spain] | This statement was taken away from the text. |
| 7-1358 | 7 | 51 | 32 | 51 | 35 | Studies on aerosols and clouds, and their interactions in the free troposphere are sparse.  Interplay between the processes embedded in the boundary-layer and free troposphere needs to be investigated.   [Panuganti China Sattilingam Devara, India] | this is correct, however IPCC does not make recommendation for research. |
| 7-1359 | 7 | 51 | 32 | 51 | 35 | The point regarding no new particle formation is very out of place here; it should be moved to the next section, which deals with physical mechanisms (ion-induced nucleation). I suggest citing it in conjunction with model experiment evidence from (Pierce and Adams, 2009) that finds the flux of GCR is unable to significantly change CCN concentrations. [Benjamin Laken, Spain] | This statement was moved into next subsection, as suggested. |
| 7-1360 | 7 | 51 | 45 | 51 | 45 | Again, I think too much emphasis on CERN as ion-induced nucleation has been well established. The key point is the statement on page 52 line 21 that the response is weeak because of the low sensitivity of CCN concentrations. Shifting emphasis to the latter would be helpful [Daniel Murphy, United  States of America] | The text regarding the CERN experiment was shortened and modified. We strongly disagree with the claim that ion-induced nucleation is well established. |
| 7-1361 | 7 | 51 | 45 | 51 | 48 | The statement that CLOUD is the only experiment on ion-induced nucleation where it has been possible to isolate the role of ions is not correct. A similar experiment was done prior to CLOUD using a ultrarelativistic electron beam by Enghoff et al. (2011) [M.B. Enghoff, J.O.P. Pedersen, U.I. Uggerhøj, S.M. Paling, and H. | The text was modified to avoid misinterpretations in this regard. |

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| | | | | | | Svensmark: Aerosol nucleation induced by a high energy proton beam, Geophysical Research Letters 38 (2011) L09805] and even earlier by Svensmark et al. (2007) using a chamber similar to CLOUD [H. Svensmark, J.O.P. Pedersen, N.D. Marsh, M.B. Enghoff, U.I. Uggerhøj: “Experimental evidence for the role of ions in particle nucleation under atmospheric conditions”, Proc.Roy.Soc. 463 (2007) 385-396 10.1098/rspa.2006.1773] [Jens Olaf Pepke Pedersen, Denmark] | |
| 7-1362 | 7 | 51 | 46 | | | I would be cautious about claiming the CLOUD results as the only results of their kind. Stating that CLOUD is the ‘only laboratory experiment on ion-induced nucleation’ it is not true, as Svensmark and co-workers ran a much simplified (yet laboratory-based) experiment in 2007 (Svensmark, H. (2007) Cosmoclimatogy: a new theory emerges, Astron. & Geophys., Royal Astronomical Society, London, 48, 1). While I do not suggest you should necessarily add a reference to this work, the CLOUD experiment is far more comprehensive, perhaps you should be somewhat cautious with the wording, as it may give the impression to some readers that you are unaware of this work. [Benjamin Laken, Spain] | The text was modified to avoid misinterpretations in this regard. |
| 7-1363 | 7 | 51 | 56 | | | Information: The CLOUD consortium will submit a paper on the NPF enhancement by GCR in a large temperature range down to -60 degrees Celsius. [Urs Baltensperger, Switzerland] | Thank you. This information will be used in case the paper will be submitted before the July 2012 deadline. |
| 7-1364 | 7 | 52 | 1 | 52 | 15 | Delete to shorten. This reads like review. [Daniel Murphy, United States of America] | This part of the text was considerably shortened. |
| 7-1365 | 7 | 52 | 12 | 52 | 12 | Need to add "to" between "shown" and "depend" [Jón Egill Kristjánsson, Norway] | Does not apply, this sentence was deleted. |
| 7-1366 | 7 | 52 | 17 | 52 | 17 | "understanding on" should be "understanding of" [Jón Egill Kristjánsson, Norway] | This typo was corrected. |
| 7-1367 | 7 | 52 | 17 | 52 | 17 | Insert "mechanism" before "as a whole" [Jón Egill Kristjánsson, Norway] | This typo was corrected. |
| 7-1368 | 7 | 52 | 17 | 52 | 19 | Another recent relevant paper is: Kazil, J., K. Zhang, P. Stier, J. Feichter, U. Lohmann, and K. O'Brien (2012), The present-day decadal solar cycle modulation of Earth's radiative forcing via charged H2SO4/H2O aerosol nucleation, Geophys. Res. Lett., 39, L02805, doi:10.1029/2011GL050058. [Richard Betts, United Kingdom of Great Britain & Northern Ireland] | We agree, a citation to this paper was added to the text. |
| 7-1369 | 7 | 52 | 24 | 52 | 28 | While much attention has been given to the (admittedly far more understood) ion-induced nucleation mechanism of a GCR – cloud link hypothesis, virtually no attention has been given to the clean-air (Global electric circuit based) mechanism. Currently only a single study is cited and discussed. Future developments in the field may show the global electric circuit to play a complex and diverse role in influencing cloud properties. For example, recent work suggests that the global electric circuit may modify storm dynamics by altering the amount of latent heat of freezing released, this process provides a large amplification from the original energy input (Tinsley, Zhou and Liu , 2012, ‘The role of volcanic aerosols and relativistic electrons in modulating winter storm vorticity’). At the moment the text gives the reader little impression of this field. [Benjamin Laken, Spain] | This paragraph has been modified based on referee comments |
| 7-1370 | 7 | 52 | 24 | 52 | 29 | Comment on text: The role of the global electric circuit has received much less attention, presumably because it is less well understood. There are several reasons for which this mechanism deserves to be discussed in more than 5 lines, as compared to the cosmic ray-cloud connection. First, its level of scientific understanding is still very low [B. A. Tinsley, Influence of Solar Wind on the Global Electric Circuit, and Inferred Effects on Cloud Microphysics, Temperature, and Dynamics in the Troposphere, Space Science Reviews, 94 (2000), pp. 231– 258.] and phenomena such as transient luminous effects only add more complexity to it. Second, and more importantly, these effects are mainly driven by the solar wind and so their long-term evolution is does not necessarily have to follow that of cosmic rays. This means that during periods such as the Maunder minimum, one may have been active and the other not. [Thierry Dudok de Wit, France] | useful reference. We have to rely on existing evidence. |
| 7-1371 | 7 | 52 | 25 | 52 | 25 | Insert ", namely that" after "clouds" [Jón Egill Kristjánsson, Norway] | sentence modified. |
| 7-1372 | 7 | 52 | 28 | 52 | 28 | "low" should be "weak" [Jón Egill Kristjánsson, Norway] | "low evidence" as per IPCC formatted language. |
| 7-1373 | 7 | 52 | 32 | 52 | 37 | There is no mention of a global electric circuit effect in the summary, yet it is a clear and valid hypothesis. I think that this should be mentioned, if only to say that the current level of understanding is relatively low. [Benjamin Laken, Spain] | synthesis has been reworded and addresses this. |

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| 7-1374 | 7 | 52 | 36 | 52 | 37 | I believe the point of a lack of cosmic ray trend is currently under-developed; the point should be more fully expressed that, even if a relationship were to exist between cosmic rays and cloud there is no chance that it could have contributed significantly to recent climate trends, as there has been no trend in cosmic ray intensity over recent decades. [Benjamin Laken, Spain] | The last sentence of synthesis was rewritten in order to consider this valid point. |
| 7-1375 | 7 | 52 | 39 | 56 | 2 | I find it very good that the IPCC WG1 addresses the issue of climate engineering ("geoengineering") this time. However, as written, the draft section does not inform the public and political dialogue adequately, and it would be better to leave out the discussion and simply point to the various dedicated assessments than to publish a discussion of climate engineering in this form. I trust that this will be improved subsantially by the next draft, and thus provide comments accordingly. In revising, it will be particularly important to consider the target audience. As is, the section is not accurate enough or deep enough to be of use for scientist colleagues from other parts of the field, and it is too disconnected from the motivations and directly too detailed to be of use for the public or policy audience. One should be chosen and focused on, or a two-level chapter structure should be used with emphasized main text (for the lay reader) and sub-level detailed text for the scientist readers. [Mark Lawrence, Germany] | Taken Into Account. |
| 7-1376 | 7 | 52 | 39 | 56 | 2 | Solar management. I question whether this section is within the mandate of the WG1, and if so whether within the mandate of the Clouds and Aerosols chapter, although to be sure, some approaches do involve clouds and aerosols. The material in this section, plus other approaches to geoengineering might better be in a special report rather than in the AR5. The review here, while brief, hardly does justice to the work and understanding that has been gained in recent years. At the same time, even considering geoengineering might be taken as an endorsement and might redound negatively on IPCC, WMO, UNEP as taking a position rather than assessing scientific understanding of climate change. [Stephen E Schwartz, USA] | Taken into Account. |
| 7-1377 | 7 | 52 | 39 | | | Section 7.5: I think this is a mitigation strategy, and does not go into the WGI assessment, but rather for WGII and WGIII as noted in this introduction. [Andrew Gettelman, USA] | Rejected -- These issues were discussed extensively during a meeting in Peru in June 2011, and revisited during discussions with chapter authors and the TSU. The decision is that it belongs here |
| 7-1378 | 7 | 52 | 39 | | | Section 7.5: I appreciate need to discuss the state of scientific understanding of geoengineering, and that such discussion naturally fits in the Working Group 1 report, and that solar management techniques are related to clouds and aerosols. The material still seems out of place here. Would it be too much to, for example, have an Annex on geoengineering? [Robert Pincus, USA] | rejected -- see response to comment 1379 |
| 7-1379 | 7 | 52 | 43 | 52 | 43 | The section should at least mention the issue around the designation: here the term "geoengineering" is used, but in many circles "climate engineering", which is more specific to what is being discussed here, is used instead. In chapter 6, "climate intervention" is introduced along with climate engineering - coordination would be sensible. Furthermore, the issue of what is included in the term should be briefly addressed, referring to the discussion in Chapter 6 that there is unclarity as to whether CCS should be considered climate engineering, though there is generally broad agreement that all forms of SRM fall under the heading. [Mark Lawrence, Germany] | accepted -- We will add the phrase "also identified by other terms like "climate engineering, and "climate intervention". Part 2 of comment related to CCS is rejected in the interest of brevity. It is better to be discussed in chapter 6. |
| 7-1380 | 7 | 52 | 43 | 52 | 43 | It would be valuable to motivate the section better, otherwise it can be taken as "just another detail among the many aerosol-cloud issues", which it really isn't given the ethical, economic, legal and political implications; this can partly be done by referring to the discussions in Chapter 6. [Mark Lawrence, Germany] | Accepted -- We will add a sentence indicating that "there are also ethical, economics, legal and political implications that are beyond the scope of this assessment". |
| 7-1381 | 7 | 52 | 43 | 52 | 43 | "…to describe the deliberate large scale…" -> "…to describe the the collection of proposals for deliberate large scale…" [Mark Lawrence, Germany] | editorial |
| 7-1382 | 7 | 52 | 53 | 52 | 53 | "…that appear to influence components…" -> "…that appear to have the potential to have a targeted influence on components…" [we cannot say that they appear to influence these components, since it has not yet been observed that they will, only through proxys] [Mark Lawrence, Germany] | Accepted. |
| 7-1383 | 7 | 52 | 55 | 52 | 55 | Why are only "governance issues" mentioned here? The list should definitely include ethical, psychological/sociological, security/risk, political, and international treaty/law issues, otherwise it seems as if these are not recognized by the IPCC WG1 o be of relevance, which would really make a poor impression for | Accepted. See response to comment 7-1380. |

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| | | | | | | the coordination with the other WGs. [Mark Lawrence, Germany] | |
| 7-1384 | 7 | 52 | 56 | | | Change "Geo-engineering" to "Geoengineering" for consistency. [Hailong Wang, USA] | Agreed. |
| 7-1385 | 7 | 53 | 1 | 53 | 11 | It would be useful to add something here acknowledging that the are an infinite number of ways that deployment of SRM can be undertaken, which makes quantitative inter-model comparison very difficult, hence the requirement for GeoMIP. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | Agreed. We have inserted sentences at the top of section section 7.5.2 indicating that the many ways that that deployment of SRM can be undertaken makes quantitative inter-model comparison very difficult. This motivates the need for a idealised experiments where the varied responses of models can be explored with simple, well defined simulation experiments that are relevant to geoengineering. |
| 7-1386 | 7 | 53 | 3 | 53 | 3 | "to the planet" should be "on the planet" [Jón Egill Kristjánsson, Norway] | Editorial. I believe both sentence structures are appropriate, but since two reviewers made the same suggestion, I am amenable to changing it. |
| 7-1387 | 7 | 53 | 3 | 53 | 3 | "phenomena to the planet" -> "phenomena on the planet" -> [Mark Lawrence, Germany] | Editorial. See response to comment 1386 |
| 7-1388 | 7 | 53 | 7 | 53 | 7 | "subject to the same limitations": this is too limited - in many cases the SRM simulations are subject to large additional uncertainties, because they use models which are designed for "standard" climate change, but push them into a phase space which is in many cases quite different than this. An important example is gravitational sedimentation: the parameterizations used in many contemporary climate models have recently been shown (Benduhn and Lawrence, submitted to Atmos. Chem. Phys., hopefully available for the next draft) to be quite inadequate for the purposes of stratospheric aerosol injections for SRM schemes. Another important example is microphysics schemes. This should be expounded on in the next draft. [Mark Lawrence, Germany] | Accepted. We have completely revised the section |
| 7-1389 | 7 | 53 | 11 | | | The nice summary of SRM in the bullets should be stated here. [Andrew Gettelman, USA] | Accepted. We have added a lot of text about this and much of it appears in the synthesis section. The points are relevant to the idealized simulations but also to the more specific strategies |
| 7-1390 | 7 | 53 | 15 | 53 | 15 | About the expression:"solar constant". Since it is not a constant (its name was introduced when it was considered that solar extraterrestrial radiation did not vary), the most correct name is "total solar irradiance" (see for example, the reference given in Chapter 8, page 71, lines 23-24: Kopp, G., and J. Lean, 2011: A new, lower value of total solar irradiance: Evidence and climate significance. Geophysical Research Letters, 38, L01706. [Rubén D Piacentini, Argentina] | Accepted. |
| 7-1391 | 7 | 53 | 15 | 53 | 16 | Here it should be noted that, although these are idealized experiments in the models, they are in principally representative of putting mirrors in space (especially at the Lagrange Point). [Mark Lawrence, Germany] | Accepted. appended a phrase "similar, in principle to the effect produced by introducing mirrors in space to attenuate sunlight (Early, 1989)" |
| 7-1392 | 7 | 53 | 16 | 53 | 16 | Briefly re-define "climate efficacy" here. [Mark Lawrence, Germany] | accepted-- inserted a parenthetical phrase (see section 7.3.5.5) where it is already discussed. |
| 7-1393 | 7 | 53 | 17 | 53 | 17 | Parentheses misplaced. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | accepted |
| 7-1394 | 7 | 53 | 17 | 53 | 18 | The paper by Kravitz et al. (2011) only describes the GeoMIP experiments, it does NOT show any results. The first results are described in Schmidt et al. (2012), which should be cited here, and which is also the source of Figure 7.14 (NOT Kravitz et al., 2011); Schmidt et al. is also a nice source of discussion about the physics behind the changes described in my comments 11 and 12. Citation: Schmidt, H., Alterskjær, K., Bou Karam, D., Boucher, O., Jones, A., Kristjansson, J. E., Niemeier, U., Schulz, M., Aaheim, A., Benduhn, F., Lawrence, M., and Timmreck, C.: Can a reduction of solar irradiance counteract CO2-induced climate change? – Results from four Earth system models, Earth Syst. Dynam. Discuss., 3, 31-72, doi:10.5194/esdd-3-31-2012, 2012. [Mark Lawrence, Germany] | Accepted: We will update these references to reflect the appropriate papers as they appear |

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| 7-1395 | 7 | 53 | 17 | 53 | 20 | The reduction in global mean precipitation is mentioned twice (redundant), and the explanation that is given ("arguments on the energy budget of the atmosphere") is wishy-washy; this points to the mistaken belief that global total precipitation can be influenced by atmospheric processes, which is wrong, because with a 1-week lifetime of water vapor, the total precipitation is almost exactly the same as the total evapotranspiration; thus the primary reason for this is a difference in how long-wave and short-wave radiation affect evapotranspiration, with the reduction in short-wave causing a more direct impact and reduction on evapotranspiration, which should be discussed better here. [Mark Lawrence, Germany] | Taken into Account. We are engaging in discussions with the reviewer to arrive at mutally agreeable language |
| 7-1396 | 7 | 53 | 18 | 53 | 18 | "exactly balanced" - this is not correct; the balance is within about +/- 0.1 W/m2, but an exact balance is not achieved (or even attempted, since it is not really sensible due to the natural interannual variability). [Mark Lawrence, Germany] | accepted -- changed exactly to closely (within 0.1 W/m2) |
| 7-1397 | 7 | 53 | 19 | 53 | 19 | Also, the latitudinal differences in the temperature changes should be described better: "compared to the reference (pre-industrial) simulation, the temperatures still increase at mid and high latitudes, and decrease in the tropics" [Mark Lawrence, Germany] | accepted |
| 7-1398 | 7 | 53 | 23 | 53 | 23 | Here the reference should be to IMPLICC (http://implicc.zmaw.de) and Schmidt et al. (2012) rather than to GeoMIP and Kravitz et al. (2011) [Mark Lawrence, Germany] | accepted |
| 7-1399 | 7 | 53 | 23 | 53 | 31 | This figure is in fact not from Kravitz et al. (2011), but from Schmidt et al. (2012: doi:10.5194/esdd-3-31-2012)! [Jón Egill Kristjánsson, Norway] | accepted |
| 7-1400 | 7 | 53 | 25 | 53 | 25 | Also land-ice should be discussed in the revised version [Mark Lawrence, Germany] | accepted |
| 7-1401 | 7 | 53 | 26 | 53 | 26 | Also, where appropriate, the impacts on ecosystems and vegetation should be discussed (some of this is in Chapter 6 already, so a link to there should be made). [Mark Lawrence, Germany] | accepted. |
| 7-1402 | 7 | 53 | 29 | 53 | 31 | The figure caption does not identify the different curves in Figure 7.14. [Steven Ghan, USA] | accepted |
| 7-1403 | 7 | 53 | 29 | 53 | 31 | About “Figure 7.14: Multi-model mean of the residual surface temperature and precipitation changes from GeoMIP simulations with a simultaneous fourfold increase in CO 2 and a reduction in solar forcing which has been adjusted in each model to maintain the top of atmosphere net flux imbalance within ±0.1 W m –2 (Kravitz et al., 2011)” . Please explain the different curves, since normally when one of them is made in a continuous (solid) way, it means that it is the mean of the others (which is not the case) or the most important one. [Rubén D Piacentini, Argentina] | accepted Same as comment 1402. |
| 7-1404 | 7 | 53 | 30 | 53 | 31 | "adjusted in each model" -> "adjusted once at the beginning of the simulation in each model" [Mark Lawrence, Germany] | accepted |
| 7-1405 | 7 | 53 | 31 | 53 | 31 | Here the reference should be to Schmidt et al. (2012) rather than to Kravitz et al. (2011) [Mark Lawrence, Germany] | accepted (if we do not use a GEOMIP figure) |
| 7-1406 | 7 | 53 | 34 | 53 | 34 | "…to mimic the effects of marine cloud seeding" - this is really not accurate, since only some regions over the ocean are appropriately cloudy on a regular basis and would be susceptible to having significant impacts from cloud seeding [Mark Lawrence, Germany] | accepted We will change "mimic" to "approximate" |
| 7-1407 | 7 | 53 | 37 | 53 | 37 | "plant albedo and desert regions" -> "plant albedo or brightening desert regions' [Mark Lawrence, Germany] | Accepted |
| 7-1408 | 7 | 53 | 40 | | | Stratospheric aerosol enhancement as an SRM option has a growing literature that reflects growing understanding of the associated processes and their tradeoffs. This section falls short of doing justice to the available material beyond citations. No figures, tables, or synthesis is included for this important topic. As a minimum, I suggest adding 1 or 2 salient graphs from the sulfate aerosol SRM results that would expand the discussion and add salient detail. One suggestion is a table that would be similar to the one Alan Robock uses in his presentations that lists physical aspects that would be significant from the climate response to injected aerosols. Some of these aspects are included in the text p54 ln 20 but would be more readily assimilated as a complete list in a table. [David Fahey, USA] | Accepted |
| 7-1409 | 7 | 53 | 40 | | | Section 7.3.5: It should be mentioned that a termination of the method would produce a reappearance of most | accepted -- See response to comment 1390.We |

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| | | | | | | of the avoided global warming within a very short time period with all the negative implications of a rapid warming of climate. [Claudia Mäder, Germany] | discuss this point in a new synthesis subsection |
| 7-1410 | 7 | 53 | 42 | 53 | 42 | "like Pinatubo demonstrate" -> "like Pinatubo (June 15, 1991) demonstrate" [Mark Lawrence, Germany] | Accepted |
| 7-1411 | 7 | 53 | 42 | 53 | 42 | "that increasing stratospheric aerosols will cool..." -> "that an increase in the amount of stratospheric aerosol particles cools..." [Mark Lawrence, Germany] | Accepted. |
| 7-1412 | 7 | 53 | 44 | 53 | 44 | "continuous" - this only fits for a tower or tethered balloon; aircraft or bursting balloon or rocket injections would be "intermittant", "regular", "periodic" or similar [Mark Lawrence, Germany] | Accepted. |
| 7-1413 | 7 | 53 | 44 | 53 | 45 | "to mimic this" - the word "mimic" here is a bit of a misnomer, because there is a vast difference between the sudden, vast, singular volcanic injection and the way climate engineering would be done; "to produce a similar overall effect on the stratospheric aerosol layer" would be more accurate [Mark Lawrence, Germany] | Accepted |
| 7-1414 | 7 | 53 | 46 | 53 | 46 | "in the stratosphere" should be "into the stratosphere" [Jón Egill Kristjánsson, Norway] | accepted |
| 7-1415 | 7 | 53 | 46 | 53 | 46 | "injecting…gases in the stratosphere" -> "injecting…gases into the stratosphere" [Mark Lawrence, Germany] | accepted |
| 7-1416 | 7 | 53 | 49 | 54 | 4 | Did anyone look at the effects an increase in the sulfuric acid flux to the troposphere and the surface would have? Would the precipitating material be spread uniformly over the globe, or concentrate in certain regions? Would it significantly change the pH regionally or globally over a period of decades? At least these questions can probably be addressed with more certainty than what the more subtle, unintended impacts on stratospheric and troposhperic chemistry, and on cloud processes, might be. [Ralph Kahn, United States of America] | Taken into Account: Yes,a sentence and reference has been inserted in the discussion |
| 7-1417 | 7 | 53 | 52 | 53 | 52 | "recognized this fact but prescribed…" -> "recognized the importance of this fact, but nevertheless for the sake of a first rough estimate prescribed…" [Mark Lawrence, Germany] | accepted |
| 7-1418 | 7 | 53 | 54 | 53 | 55 | "particle size…could be very inefficient" - a size cannot be inefficient, please revise wording to what is really meant [Mark Lawrence, Germany] | accepted, corrected. Thanks. The sentence reads correctly if "particle size" is replaced by "geoengineering" |
| 7-1419 | 7 | 53 | 54 | 53 | 55 | How can "particle size… be very inefficient"? Something is missing in this sentence. [JOHN OGREN, USA] | see respose to comment 1419 |
| 7-1420 | 7 | 53 | 54 | | | Also recent paper by English, Toon et al that shows the same thing. Not sure if this is out. [Andrew Gettelman, USA] | the English paper is now cited. |
| 7-1421 | 7 | 53 | 56 | 53 | 56 | "emissions" -> "injections" [Mark Lawrence, Germany] | taken into account, but replaced "emissions" with "sources" |
| 7-1422 | 7 | 54 | 3 | 54 | 3 | "immediately' -> "extremely rapidly' [Mark Lawrence, Germany] | Accepted. |
| 7-1423 | 7 | 54 | 6 | 54 | 6 | Please cite the several studies. [Steven Ghan, USA] | Accepted. The early studies mentioned in later sentences are moved to this point in the narrative |
| 7-1424 | 7 | 54 | 6 | 54 | 6 | "Several modelling studies" - please give references [Mark Lawrence, Germany] | See response to comment 1423 |
| 7-1425 | 7 | 54 | 6 | 54 | 7 | "at least through a doubling of CO2 concentrations" is very confusing. Better: "even in the case of at least a doubling of CO2 concentrations", or something like that. [Jón Egill Kristjánsson, Norway] | Accepted |
| 7-1426 | 7 | 54 | 8 | 54 | 8 | "idealized studies" -> "idealized studies discussed above" [Mark Lawrence, Germany] | Accepted |
| 7-1427 | 7 | 54 | 9 | 54 | 9 | "more or less" - make this more precise [Mark Lawrence, Germany] | Accepted |
| 7-1428 | 7 | 54 | 9 | 54 | 10 | state relative to what the reduction occurs. Relative to present day conditions or relative to future conditions without SRM. [Rolf Mueller, Germany] | accepted.added Relative to present day conditions |
| 7-1429 | 7 | 54 | 12 | 54 | 18 | The influence of extended volcanic outbreaks on the atmosphere best is documented by times series of | rejected. These papers are interesting, but not needed |

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| | | | | | | atmospheric turbidity. 100 year records of atmospheric turbidity on a global basis are available, but never used to correlate to global temperature. Helmes, L., R. Jaenicke (1986) Atmospheric Turbidity Determined From Sunshine Records. J. Aerosol Science 17, 261-263 or Jaenicke, R. (1988) Aerosol Physics and Chemistry. Landolt-Börnstein Numerical Data and Functional Relationships in Science and Technology New Series "Group V: Geophysics and Space Research, 4 Meteorology, Subvolume b [Ruprecht Jaenicke, Germany] | to make the point. The assessment is not a review |
| 7-1430 | 7 | 54 | 12 | 54 | 18 | Too much detail here [Daniel Murphy, United States of America] | section completely rewritten |
| 7-1431 | 7 | 54 | 13 | 54 | 13 | "less effective" - add references (e.g., Niemeier et al., 2010; Niemeier U, Schmidt H, Timmreck C. 2010. The dependency of geoengineered sulfate aerosol on the emission strategy, Atmos. Sci. Let.,DOI: 10.1002/asl.304) [Mark Lawrence, Germany] | See response to comment 1430 |
| 7-1432 | 7 | 54 | 14 | 54 | 14 | "will produce" -> "are simulated to produce" [Mark Lawrence, Germany] | see response to comment 1430 |
| 7-1433 | 7 | 54 | 14 | 54 | 14 | imprecise use of the term "aerosol' here, better AOD or similar measurable term [Mark Lawrence, Germany] | see response to comment 1430 |
| 7-1434 | 7 | 54 | 14 | 54 | 14 | "sizable fraction" - could this be made more precise, e.g., in terms of AOD? Also, it would be helpful to contrast this with the coverage from a tropical injection [Mark Lawrence, Germany] | Taken into account. -- There is some conflict between some reviewers wanting less detail in this text, and others urging more precise language. We have tried to accommodate both. |
| 7-1435 | 7 | 54 | 17 | 54 | 17 | Replace "protocol" by "design" [Jón Egill Kristjánsson, Norway] | accepted |
| 7-1436 | 7 | 54 | 20 | 54 | 32 | Given all the cloud modeling uncertainties described in the first four sections of this chapter, what level of confidence do you assign to these predictions, qualitatively, and more importntly, quantitatively? I realize it is beyond the scope of this chapter to suggest whether the associated risks would be worth taking... [Ralph Kahn, United States of America] | Accepted -- see revisions. |
| 7-1437 | 7 | 54 | 27 | 54 | 27 | "is ozone" -> "in ozone" [Mark Lawrence, Germany] | accepted |
| 7-1438 | 7 | 54 | 27 | | | "…This change is ozone might have discernable…". Probably meant is: "…This change in ozone might have discernable …" Please adjust sentence. [Birgit Nabbefeld, Germany] | same as comment 1438 |
| 7-1439 | 7 | 54 | 27 | | | "change is" ---> "change in" [Manfred Wendisch, Germany] | same as comment 1438 |
| 7-1440 | 7 | 54 | 28 | 54 | 28 | "some degree" -> of course there is *some* degree, the question is whether it is a significant degree [Mark Lawrence, Germany] | accepted -- changed "is some degree" to "may be an approximate". This needs more study just like all the other issues discussed in the geoengineering section |
| 7-1441 | 7 | 54 | 42 | | | "Wang et al., 2011a" in the reference list belong to here (with Latham et al., 2008). [Hailong Wang, USA] | accepted |
| 7-1442 | 7 | 54 | 43 | | | It is useful to repeat here the finding that at a larger (i.e., climatically-relevant) scale, such effects from ship emissions are not detectable in satellite data (Peters et al., 2011; details see above) [Johannes Quaas, Germany] | accepted -- we agree with the point, but phrase it somewhat differently, |
| 7-1443 | 7 | 54 | 46 | 54 | 47 | "changing low-liquid water clouds to high liquid water clouds" -> "changing clouds with low liquid water contents to high liquid water contents" [Mark Lawrence, Germany] | accepted |
| 7-1444 | 7 | 54 | 46 | 54 | 47 | "low-liquid" and "high liquid" are inconsistent. [Hailong Wang, USA] | see response to comment 1445 |
| 7-1445 | 7 | 55 | 5 | 55 | 5 | Also for the SOD: note some of the rough considerations and calculations for the delivery modalities [Mark Lawrence, Germany] | accepted -- added a sentence discussing consequences of injection strategies |
| 7-1446 | 7 | 55 | 7 | 55 | 12 | A useful comparison for the difficulty of intentionally changing marine cloud brightness is that all current global ship tracks don't have a large enough impact so any intervention would have to be 1) much better targeted and 2) some fraction of global ship traffic. [Daniel Murphy, United States of America] | accepted. We have added some sentences about this. |
| 7-1447 | 7 | 55 | 8 | 55 | 11 | Some caveats should be included surrounding these studies - they aessentially assume all clouds in certain | accepted -- see last paragraph of 7.5.4 |

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| | | | | | | areas have a maximum CCN. They do not assess any of the practicalities of elevating CCN to such levels nor are they able to capture some important physical processes (documented in the LES modelling sections) or the evaporative cooling of sea-salt/water mixtures. [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | |
| 7-1448 | 7 | 55 | 8 | 55 | 11 | there's nothing here on the rate of removal of geoengineering (which could be rapid) and subsequent rapid return of climate system to unmask the avoided warming. The “lifetime” of GE techniques relative to the lifetime of CO2 are important to discuss. The Exec Summary mentions this but I couldn't spot the details in the text [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland] | accepted -- we now discuss this in the synthesis section |
| 7-1449 | 7 | 55 | 11 | 55 | 11 | Here would be a natural place to mention some of the results from the very recent modeling studies of Partanen et al. (2012, doi:10.1029/2011JD016428) and Alterskjær et al. (2012: doi:10.5194/acpd-11-29527-2011). [Jón Egill Kristjánsson, Norway] | accepted |
| 7-1450 | 7 | 55 | 13 | 55 | 48 | White roofs are a form of geoengineering surface albedo. A recent study found that, whereas white roofs cause local cooling on average, they increase stability, reducing larger scale cloudiness, increasing sunlght to the surface. They also increase reflective sunlight in uban areas being absorbed by brown and black carbon. The combination of these effects resulted in net global warming due to white roofs (Jacobson, M.Z., and J.E. Ten Hoeve, Effects of urban surfaces and white roofs on global and regional climate, J. Climate, 25, 1028-1044, doi:10.1175/JCLI-D-11-00032.1, 2012). The same effect does not apply to snow or sea ice because the types of clouds and their impact due to stability changes differ and because not so much BC or BrC exists over high latitudes. [Mark Z. Jacobson, U.S.A.] | It is interesting that there could be some negative feedback to increasing surface albedo. Such a negative feedback can limit the cooling effect from surface brightening. However it is unclear and there is little evidence provided in this paper as to why the climate response can be so large as to overwhem the initial radiative perturbation. A simple RT model can be used to show that the the albedo of the surface-atmosphere increases when surface albedo increases, even in the presence of absorbing aerosols. A warming in response to roof whitening would imply a negative climate efficacy. The text has been modified to mention possible negative feedback to white roofs. |
| 7-1451 | 7 | 55 | 22 | 55 | 22 | "less negative values" -> "smaller magnitudes of cooling" [Mark Lawrence, Germany] | agreed |
| 7-1452 | 7 | 55 | 24 | 55 | 34 | I appreciate that if *only* the albedo of the surface is changed by genetically engineered grass species, the impacts can at least assesessed to some extent. But is there an assessment of uncertainties in the broader ecological impacts on everything from the water cycle to insect and bacterial populations? Also, most of the energy budget impacts are given as "global mean" values, whereas the actual impacts will be regional, and the magnitudes in some (possibly large) regions might be much greater. [Ralph Kahn, United States of America] | We have tried to do a better job highlighted unanticipated, and unexplored consequences |
| 7-1453 | 7 | 55 | 32 | 55 | 32 | out to -> to [Jim Haywood, United Kingdom of Great Britain & Northern Ireland] | Changed to "pointed to " |
| 7-1454 | 7 | 55 | 32 | 55 | 32 | "out to potential effects" -> "out potential feedbacks and side effects" [Mark Lawrence, Germany] | Changed to"pointed to". "Feedbacks" is more neutral here as the effects could be of either sign. |
| 7-1455 | 7 | 55 | 36 | 55 | 36 | "HadCM3 model." -> "HadCM3 model, a relatively simple GCM by contemporary standards." [Mark Lawrence, Germany] | This is irrelevant for the statement which is made here. |
| 7-1456 | 7 | 55 | 41 | 55 | 41 | This is poorly worded. Please replace "albedo of ocean surfaces and large areal extent mean only" with "albedo and large areal extent of ocean surfaces mean that only" [Jón Egill Kristjánsson, Norway] | accepted |
| 7-1457 | 7 | 55 | 52 | 55 | 52 | "Cirrus clouds…radiation. Thin high cirrus" -> "Clouds…radiation. The effect of low clouds on OLR is very small. In contrast, thin high cirrus" [Mark Lawrence, Germany] | editorial: See response to 7-1458 |
| 7-1458 | 7 | 55 | 56 | 55 | 56 | "coldest cirrus ice crystals could fall out and reduce" -> "coldest cirrus, ice crystals would be expected to fall out more rapidly and thus reduce" [Mark Lawrence, Germany] | This paragraph has been substantialyl revised |
| 7-1459 | 7 | 56 | 1 | 56 | 1 | "not yet well enough understood" - marine cloud microphysics is also not well understood, as was recently demonstrated in the E-PEACE campaign (http://aerosols.ucsd.edu/E_PEACE.html), which should also definitely be mentioned somewhere in the previous section. [Mark Lawrence, Germany] | taken into account,, but a paper needs to be published before it can be cited. |

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| 7-1460 | 7 | 56 | 5 | 57 | 11 | FAQ 7.1: I think that overall this FAQ is pitched at a good level for a general reader, and the figures are clear and helpful. [David Wratt, New Zealand] | Thanks. The FAQ has been substantially rewritten however. |
| 7-1461 | 7 | 56 | 12 | 56 | 12 | I suggest that you avoid the word "belief" and stick with the words used elsewhere in this Assessment, like "very likely". [JOHN OGREN, USA] | This has been changed 'it seems very likely that ..;" |
| 7-1462 | 7 | 56 | 12 | 56 | 12 | change "have" to "has" [Richard Somerville, USA] | Done |
| 7-1463 | 7 | 56 | 12 | 56 | 13 | Here location (i.e., Earth's surface) where the cooling and warming occur should be indicated. [Chien Wang, United States of America] | The first paragraph is an introduction to the FAQ and cannot go into too much details. Location of cooling and warming in now discussed in the figure. |
| 7-1464 | 7 | 56 | 12 | 56 | 13 | I suggest this "initial summary answer" paragraph be italicised, in line with the standard WG1 FAQ style. [David Wratt, New Zealand] | done. |
| 7-1465 | 7 | 56 | 12 | 59 | 13 | Fails to mention that aerosols behave differently at night when they reduce radiation loss from the earth and restors half it back [VINCENT GRAY, NEW ZEALAND] | this is too much detail for an FAQ. The greenhouse effect due to antropogenic aerosols is usually included in climate models but it is small compared to their shortwave effects. |
| 7-1466 | 7 | 56 | 12 | | | "responsible for a cooling"; NO. Earth has _warmed_, not cooled. "exerted a cooling influence" or similar. Same "masked" warming. NO. diminished the warming that would otherwise have occurred, or similar. Line 31 is better. [Stephen E Schwartz, USA] | change done. |
| 7-1467 | 7 | 56 | 12 | | | Please replace "a cooling which have" by "a cooling which has". [David Wratt, New Zealand] | done |
| 7-1468 | 7 | 56 | 12 | | | "Believed" is a loaded word and not very scientific. Could "It is believed" be replaced by something like "There is evidence that" ? [David Wratt, New Zealand] | changed. |
| 7-1469 | 7 | 56 | 13 | 56 | 13 | Add, at end, "if any" [VINCENT GRAY, NEW ZEALAND] | the warming from anthropogenic greenhouse effect is well established, see AR4 and other chapters in AR5. |
| 7-1470 | 7 | 56 | 13 | | | “partially masked”: some of the iRF estimates that were considered in the assessment overcompensate the greenhouse-gas forcing. [Johannes Quaas, Germany] | this is correct, but the FAQ draws on material located in other chapters, eg on D&A, which shows that the warming from anthropogenic greenhouse effect is strongly than the cooling from aerosols. |
| 7-1471 | 7 | 56 | 15 | 56 | 21 | Aerosol definitions should be concentrated at one location in the chapter. A table is needed (similiar to earlier IPCC reports) with updated global source strenghts of the various aerosol sources. That would give the opportunity to value the importance of the different sources. It could limit itself to the radiative important aerosol sizes as well. Please keep in mind that mineral dust, sea-salt, biological particles are documented to be present even under 100 nm in diameter in considerable amounts. K. KANDLER, L. SCHÜTZ, C. DEUTSCHER, M. EBERT, H. HOFMANN,S. JÄCKEL, R. JAENICKE, P. KNIPPERTZ, K. LIEKE, A. MASSLING, A. PETZOLD, A. SCHLADITZ, B. WEINZIERL, A. WIEDENSOHLER, S. ZORN and S. WEINBRUCH (2009) Size distribution, mass concentration, chemical and mineralogical composition and derived optical parameters of the boundary layer aerosol at Tinfou, Morocco, during SAMUM 2006. Tellus 61B, 32-50; K. KANDLER, L. SCHÜTZ, S. JÄCKEL, K. LIEKE, C. EMMEL, D. MÜLLER-EBERT, M. EBERT, D. SCHEUVENS, A. SCHLADITZ, B. SEGVIC , A. WIEDENSOHLER and S. WEINBRUCH (2011) Ground-based off-line aerosol measurements at Praia, Cape Verde, during the Saharan Mineral Dust Experiment: microphysical properties and mineralogy. Tellus 63B, 459-474 ("The size distribution measurements show number maxima around 50 to 70 nm diameter. Four modes can be identified in the spectra, of which the smallest [that with 50 to 70 nm] belongs to sea-generated and sulfate aerosol."). See also Mészáros, A., K. Vissy (1974) Concentration, Size Distribution and Chemical Nature of Atmospheric Aerosol Particles in Remote Oceanic Areas. Aerosol Sci. 5, 101-109 [Ruprecht Jaenicke, Germany] | it is not clear how this comment relates to the FAQ on page 56, lines 15 to 21. Note that Tables 7.1 and 7.2 will be modified to address this and similar comments. |
| 7-1472 | 7 | 56 | 15 | | | 1 week is not a lower bound on the aerosol lifetime, not even a good approximation to this. [Johannes Quaas, Germany] | changed to 1 day to 2 weeks. |

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| 7-1473 | 7 | 56 | 17 | 56 | 17 | "man-made" has just been used above instead of anthropogenic. It may be useful to harmonise the wording. [Nicolas Bellouin, United Kingdom] | anthropogenic used throughout now. |
| 7-1474 | 7 | 56 | 24 | 56 | 24 | FAQ.7.1, Figure 1, the lower panel on the left has such statement of "Atmospheric heating leading to surface warming", this is not absolutely correct. Please note that the absorbing aerosol actually cools the surface quite significantly, combining both scattering and ansorption, hence, whether a warming to the surface would occur really depends on the heat transport and heating effeciency among many other factors, especially when local effect is concerned. [Chien Wang, United States of America] | the figure will be redrawn with two panels to highlight the difference between the local instantaneous cooling and the large-scale warming due to absorbing aerosols. |
| 7-1475 | 7 | 56 | 24 | 56 | 26 | Proposal to change: Aerosol scattering generally results in a more reflective planet an tends to cool climate, while absorption results in a less reflective planet and tends to warm climate. [Claudia Mäder, Germany] | changed. |
| 7-1476 | 7 | 56 | 31 | 56 | 31 | Suggest replacing "been" with "contributed" [Jón Egill Kristjánsson, Norway] | unchanged. |
| 7-1477 | 7 | 56 | 35 | 56 | 49 | Studies on the influence of stratospheric processes, particulalrly via dynamics, on tropspospoheric weather and climate change are sparse and need to be undertaken on priority basis. [Panuganti China Sattilingam Devara, India] | the stratosphere is encapsulated in this sentence. The IPCC does not make statement on the prioritisation of research. |
| 7-1478 | 7 | 56 | 36 | 56 | 36 | The "subtle" does not go well with the "significant" in the following sentence. [Nicolas Bellouin, United Kingdom] | point taken, but small shift in rain patterns can result in large changes locally. Subtle also means "difficult to understand". Unchanged. |
| 7-1479 | 7 | 56 | 41 | 56 | 41 | "more condensation nuclei" should be "more cloud condensation nuclei" [Jón Egill Kristjánsson, Norway] | done. |
| 7-1480 | 7 | 56 | 43 | | | “tend to” seems overly cautious, “produce clouds”, however, may lead to misunderstanding. A better wording could be “A robust result is that typical anthropogenic aerosols lead to more numerous, but smaller, droplets in liquid clouds, all else being equal.” [Johannes Quaas, Germany] | changed to "A robust result is that more aerosols generally produce liquid clouds which are brighter because of more numerous and smaller cloud droplets" |
| 7-1481 | 7 | 56 | 46 | | | supposedly, “many other pathways” [Johannes Quaas, Germany] | changed. |
| 7-1482 | 7 | 56 | 49 | 56 | 49 | "enhancing" should be "adding to", because the direct and indirect effect are not the same (but they are additive). [Jón Egill Kristjánsson, Norway] | removed. |
| 7-1483 | 7 | 56 | 51 | | 56 | You might want to look at the recent Nature paper by solomon et al regarding the impact of small volcanic perturbations of the stratosphere and its potential to impact global warming. [Larry Thomason, United States of America] | this is covered in Chapter 8 but it is too detailed for the FAQ. |
| 7-1484 | 7 | 56 | | | | Is informal language deliberate? "While", "since", "last couple of decades". I suggest at least consider whether this is appropriate for a report of the WMO and UNEP. At line 37 "predict" is surely used informally but at variance with the labored distinction given in chapter 11 ; better "represent" .<br><br>Similarly FAQ 2: "reflect a lot of solar radiation" [Stephen E Schwartz, USA] | the language has been tied up. |
| 7-1485 | 7 | 56 | | | | FAQ 7.1: Swap FAQ numbering to be consistent with ordering given in Chapter, i.e., Clouds before Aerosols. [Thomas Stocker/ WGI TSU, Switzerland] | FAQ will be swapped. |
| 7-1486 | 7 | 56 | | | | FAQ 7.1: Chapeau needs to be revised. 'It is believed' does not sound like the result of a comprehensive assessment. [Thomas Stocker/ WGI TSU, Switzerland] | revised. |
| 7-1487 | 7 | 56 | | | | FAQ 7.1: Page 57, lines 3-4. Replace 'augment' by 'stop off-setting' or similar. [Thomas Stocker/ WGI TSU, Switzerland] | "augment" means "to make (something already developed or well under way) greater", which seems appropriate here. |
| 7-1488 | 7 | 56 | | | | FAQ 7.1, Fig 1: We suggest the messages of this figure will be much clearer if you include disturbed vs. undisturbed in each of the three instances. Also we suggest adding LW and SW contributions. [Thomas Stocker/ WGI TSU, Switzerland] | it would be too complicated to include the effects of LW radiation. We now include two panels to highlight the difference between changes in radiative fluxes and climate response. |

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| 7-1489 | 7 | 57 | 2 | 57 | 4 | The sentence is unclear. Does it intend to mean that decreasing aerosol emissions will progressively less balance greenhouse gas-induced warming ? [François GERVAIS, France] | yes, "augment" means "to make (something already developed or well under way) greater", which seems appropriate here. |
| 7-1490 | 7 | 57 | 4 | | | As a clarification, it would be good to add “augment greenhouse-gas induced warming relative to the present day” (they would continue to cool relative to pre-industrial) [Johannes Quaas, Germany] | "augment" means "to make (something already developed or well under way) greater", which seems appropriate here. |
| 7-1491 | 7 | 57 | 14 | 58 | 20 | FAQ 7.2: This FAQ covers the relevant issues in language which is suitable for the general reader. [David Wratt, New Zealand] | Acknowledged. |
| 7-1492 | 7 | 57 | 16 | 57 | 26 | I suggest this "initial summary answer" paragraph be italicised, in line with the standard WG1 FAQ style. [David Wratt, New Zealand] | Done. |
| 7-1493 | 7 | 57 | 18 | 58 | 18 | Fails to mention that clouds warm the atmosphere when they form after the evaporation of water cooled the earth. Also it does not mention their different role at night when they return some of the energy radiated from the earth by condensation. [VINCENT GRAY, NEW ZEALAND] | Added a mention of warming as water vapor condenses. |
| 7-1494 | 7 | 57 | 20 | | | Whether water precipitation is essential for life in general is questionable. [Johannes Quaas, Germany] | Modified the text to refer to life on land. |
| 7-1495 | 7 | 57 | 32 | 57 | 32 | This sentence should be updated with findings of AR5 [Andrew Ferrone, Germany] | The sentence is consistent with the AR5 results. |
| 7-1496 | 7 | 57 | 32 | 57 | 32 | For an FAQ, it would be appropriate to explain what "a positive or near-neutral cloud feedback" means in the context of climate change. [JOHN OGREN, USA] | The wording has been changed. |
| 7-1497 | 7 | 57 | 36 | 57 | 36 | "in the present-climate": it is likely that clouds have "cooled the Earth" in a pre-industrial climate as well. As currently written, the statement confuses the distinction between "current contribution to the Earth's budget" and "cloud forcing since pre-industrial times", which are two very different notions. [Nicolas Bellouin, United Kingdom] | No change has been made. We have no observations of the cloud forcing in pre-industrial times. |
| 7-1498 | 7 | 57 | 40 | 57 | 46 | I realize that the intent of the FAQs is to provide simple, digestible answers to fundamental questions. But the explanation of how high clouds warm the Earth in this paragraph is just fundamentally incorrect. High cold clouds do not primarily absorb IR radiation from the surface. Most of the surface upwelling LW (at least in places with high clouds) is absorbed by water vapor in the boundary layer (and to a lesser extent by CO2 and other GHGs). Very little of it (a few percent) ever gets as far as the base of a cirrus cloud. What cirrus clouds do absorb a lot of, however, is the upwelling LW emitted from the mid-troposphere, which is where the atmosphere would radiate to space in the absence of clouds (i.e., what determines most of the clear sky OLR measured by a satellite instrument such as ERBE or CERES). Cirrus clouds warm the Earth because they lie above the clear sky emission-to-space level and are colder than the temperatures at the clear-sky emission level (which is really shorthand for a distribution of altitudes over which the Earth emits to space, but peaking in mid-troposphere). Cirrus absorb this radiation and emit to space at their colder temperatures, which means they emit less to space than clear sky regions do. This emission reduction is only communicated to a surface warming after one includes a convective adjustment of the lapse rate. Without the basic statement that in the absence of clouds Earth radiates to space mostly from the middle troposphere (because of GHGs) and that cirrus warm the Earth by causing it to radiate to space at colder temperatures than that because they lie above the clear sky emission level, there is no way for the reader to understand the unexplained statement in the previous paragraph that low clouds have only a weak effect on IR emitted to space by Earth. After all, if I am a reader of this FAQ trying to understand why high and low clouds have different IR effects, I might ask: if wimpy cirrus absorb radiation from the surface, why don't those bad boy thick low clouds, which are so much closer to the surface, absorb lots of that IR (in fact they do) and warm the Earth too? It's because one lies below the clear sky emission level and the other lies above - the IR absorbed by low clouds doesn't matter for the TOA energy balance, but the IR absorbed by cirrus does. [Anthony Del Genio, USA] | The wording has been changed. |
| 7-1499 | 7 | 57 | 42 | 57 | 42 | Insert "that the" between "energy" and "Earth" [Jón Egill Kristjánsson, Norway] | The wording has been changed. |
| 7-1500 | 7 | 57 | 48 | 57 | 48 | Insert "that" after "sunlight" [Jón Egill Kristjánsson, Norway] | The wording has been changed. |

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| 7-1501 | 7 | 57 | 48 | 57 | 48 | "could be different" is unclear. Different from what? [Jón Egill Kristjánsson, Norway] | The wording has been changed. |
| 7-1502 | 7 | 57 | | | | FAQ 7.2: Chapeau - 'Recognized as a key factor since 1970's - Unclear if this refers to the availability of observations since 1970s, or was it not relevant before the 1970's.? [Thomas Stocker/ WGI TSU, Switzerland] | The wording has been changed. |
| 7-1503 | 7 | 58 | 2 | | 3 | Might use this sentence to suggest that the understanding gained from this research is the basis for understanding the role of clouds in climate change and representing cloud processes in climate models. Might also continue in third person plural: Climate scientists have also... [Stephen E Schwartz, USA] | The text has been deleted. |
| 7-1504 | 7 | 58 | 3 | 58 | 6 | About the sentence: “We have also been working to improve the simulation of clouds in climate models. Many current models predict a moderately positive net cloud feedback, in which both low and high clouds feed back positively. Work continues to further evaluate and refine these results.” It is the first time I see the word “we” in this and the first two chapters of AR5-WGI, that I analyzed. Please, explain if this word (we) refers to the Authors of the chapter or to climate scientists (as indicated in the same page 58, line 1). [Rubén D Piacentini, Argentina] | The wording has been changed. |
| 7-1505 | 7 | 58 | 5 | | | "feed back positively"; might better say "increase the warming that would result from an increase in CO2 beyond the direct effect of the CO2 itself" or otherwise elaborate. Careful on "predict" here and also next para. 5 times. [Stephen E Schwartz, USA] | The wording has been changed. |
| 7-1506 | 7 | 58 | 8 | 58 | 9 | We would like to support the idea to add a discussion of the confidence in current models. [Andrew Ferrone, Germany] | We don't think that such a statement belongs in an FAQ, at least not at this stage. |
| 7-1507 | 7 | 58 | 17 | 57 | 18 | I know what the authors are trying to convey here, but it is stated very clumsily and deserves to be re-written. Large cloud changes could go in either direction, but the sentence sort of implies that any large cloud change would be an increase that causes a negative feedback. This unfortunately reinforces one of the biggest misconceptions that is out there, namely that in a warmer climate more water evaporates and this causes clouds to increase, causing a negative feedback that could eliminate climate change. The last time I checked, there was still a video online at the NASA education web site claiming that this is what climate models predict. So what is really needed here is a statement that it is sometimes assumed outside the scientific community that more evaporation will cause an increase in cloud cover with warming, but that this is not what climate models predict thus far, and in fact, nearly all current models predict that clouds will actually do the opposite. [Anthony Del Genio, USA] | The wording has been changed. |
| 7-1508 | 7 | 58 | 23 | 60 | 16 | FAQ 7.3: I think this FAQ does a good job of covering the necessary scope, but could benefit from some simplification or clarification of some of the wording for the the general reader. [David Wratt, New Zealand] | The FAQ has been substantially revised. will adjust again based on subsequent specific comments |
| 7-1509 | 7 | 58 | 26 | | | I recommend insertion of a one paragraph high-level summary answer at the beginning (in italics), in line with the standard WG1 FAQ style. [David Wratt, New Zealand] | Accepted |
| 7-1510 | 7 | 58 | 31 | | | Section FAQ 7.3: I think that the FAQ "Geoengineering" should be split and that the answer concerning carbon dioxide removal should be moved to Chapter 6, becasue the CDR itself is discussed in Chapter 6. It should be made clear that geoengineering is not exclusively related to aerosol and clouds. [Johannes Schneider, Germany] | Rejected based on consultation with other chapters and the TSU |
| 7-1511 | 7 | 58 | 40 | 58 | 40 | About “FAQ 7.3, Figure 1: Overview of carbon dioxide removal methods”. The geometrical representation of CO2 (carbon dioxide) is not correct, since it is a linear molecule (without molecular electric dipole, as the water molecule has, for example, since it is of the angular type).So the image of CO2 must be as follows: O=C=O (see for example, http://www.rsmas.miami.edu/groups/coral-lab/faq/). [Rubén D Piacentini, Argentina] | Accepted |
| 7-1512 | 7 | 58 | 42 | 58 | 42 | insert "artificial acceleration of" before natural carbon cycle processes. [Daniel Murphy, United States of America] | Agreed - text revised |
| 7-1513 | 7 | 58 | 42 | 58 | 44 | The meaning of the numbers (1) – (4) is not clear. [Claudia Mäder, Germany] | Agreed - text revised |
| 7-1514 | 7 | 58 | 47 | 59 | 55 | What if carbon dioxide turns out to have little influence on the climate? [VINCENT GRAY, NEW ZEALAND] | Rejected - Section is consistent with contents in other chapters in this WG1 report |

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| 7-1515 | 7 | 58 | 49 | | | I suggest replacing "PgC" with "billion metric tons of carbon" for the benefit of the general reader. [David Wratt, New Zealand] | Taken into account - PgC expanded |
| 7-1516 | 7 | 58 | 51 | 58 | 54 | Could this sentence be reworded in a way which is more easily understandable for the general reader ? [David Wratt, New Zealand] | Editorial - text revised |
| 7-1517 | 7 | 58 | 54 | 58 | 54 | Useful to state this directly "Since about half of the CO2 emitted from burning fossil fuels remains in the atmosphere, it is expected that sequestering a certain amount of carbon will only remove about half that much from the atmosphere. [Daniel Murphy, United States of America] | Agreed - text revised |
| 7-1518 | 7 | 58 | 54 | 58 | 54 | Also useful to state directly that ocean carbon storage in the upper ocean is not effective. It must go into the deep ocean or else it will rebound back into the atmosphere. [Daniel Murphy, United States of America] | Taken into account - Discussion of "permanence of reservoir" in this paragraph |
| 7-1519 | 7 | 58 | 56 | 58 | 56 | The fact that CO2 stocking solutions might fail and release large quantities of stocked CO2 in the atmosphere during a very short time should be acknowledged here, although it may be a "low probability, high impact" event. [Andrew Ferrone, Germany] | Taken into account - in the previous paragraph where the permanence of the reservoir is discussed |
| 7-1520 | 7 | 58 | 56 | 58 | 57 | The "low risk" assessment breaks down if CO2 were to leak out from the reservoirs where it is stored. This is clearly a risk to be reckoned with. [Jón Egill Kristjánsson, Norway] | Taken into account - in the previous paragraph where the permanence of the reservoir is discussed |
| 7-1521 | 7 | 59 | 4 | | | "respond for decades or centuries to the original increases in CO2 even after CDR is applied." Contradicts recent work (Held, 2010) and other modeling studies that speak to rapid response of upper compartment of climate system to forcings.<br><br>Held IM, Winton M, Takahashi K, Delworth T, Zeng F, Vallis GK (2010) Probing the Fast and Slow Components of Global Warming by Returning Abruptly to Preindustrial Forcing. J Climate 23:2418-2427. doi:10.1175/2009JCLI3466.1<br><br>Rapid response is evidenced also in model calculations in which emissions of aerosols are halted and temperature responds on time scale of 5 years).<br><br>Brasseur GP, Roeckner E (2005) Impact of improved air quality on the future evolution of climate. Geophys Res Lett 32:L23704. doi:10.1029/2005GL023902<br><br>Knutti R, Krähenmann S, Frame DJ, Allen MR (2008) Comment on ‘‘Heat capacity, time constant, and sensitivity of Earth’s climate system’’ by S. E. Schwartz. J Geophys Res 113:D15103. doi:10.1029/2007JD009473<br><br>Knutti R., and G.-K. Plattner, 2012: Comment on “Why Hasn't Earth Warmed as Much as Expected?” by Schwartz et al. 2010. J. Climate. In press, http://dx.doi.org/10.1175/2011JCLI4038.1<br><br>Matthews HD, Caldeira K (2007) Transient climate-carbon simulations of planetary geoengineering. Proc Natl Acad Sci USA 104:9949-9954 [Stephen E Schwartz, USA] | Agreed - The sentence "Therefore, decreases in surface temperature would lag CDR-induced decreases in atmospheric CO2 concentrations" is removed |
| 7-1522 | 7 | 59 | 8 | 59 | 11 | The first two "side effects" are really losing the side effects of increasing CO2; it is uncomfortable listing them in this way. [Daniel Murphy, United States of America] | Accepted. Section extensively revised |
| 7-1523 | 7 | 59 | 11 | 59 | 11 | The fact that higher CO2 levels always leads to an increase in plant productivity is not correct. In regions were certain plant species are on the edge of temperature resilience today, the temperature increase associated with higher CO2 levels, might lead to a reduction of plant productivity (see AR4 WGII). This should be relfected in the present sentence.. [Andrew Ferrone, Germany] | Taken into account. Sentence revised |
| 7-1524 | 7 | 59 | 14 | 59 | 14 | Need a reference to the afforestation statement. [Daniel Murphy, United States of America] | Rejected - references not recommened in FAQ |
| 7-1525 | 7 | 59 | 41 | 59 | 41 | About “FAQ 7.3, Figure 2: Overview of solar radiation management methods”. There are six arrows and only five of them have an explanation about the effect that they will produce on incident solar radiation. In particular, the arrow lacking explanation is the one pointing to the ocean surface. Please, provide an | Agreed - Figure revised |

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| | | | | | | explanation. [Rubén D Piacentini, Argentina] | |
| 7-1526 | 7 | 60 | 2 | 60 | 2 | "average" on two occurences is not very clear. Better: "globally averaged" [Jón Egill Kristjánsson, Norway] | Accepted |
| 7-1527 | 7 | 60 | | | | discussion on geo-engineering is incomplete. What about; 1. Acidification by precipitation and 2. Long-term sustenance, its cost implications and side effects? [K KRISHNA MOORTHY, INDIA] | Taken into account. acidification is now dealt with (see response to comment 1470). A sentence covering the sustanance point will also appear in the "synthesis section" |
| 7-1528 | 7 | 61 | 3 | 99 | 34 | Cheng, A., and K.-M. Xu, 2006: Simulation of shallow cumuli and their transition to deep convective clouds by cloud-resolving models with different third-order turbulence closures. Q. J. Roy. Meteor. Soc., 132, 359-382.<br>Cheng, A., and K.-M. Xu, 2008: Simulation of boundary-layer cumulus and stratocumulus clouds using a cloud-resolving model with low and third-order turbulence closures. J. Meteor. Soc. Japan, 86A, 67-86.<br>Cheng, A., and K.-M. Xu, 2011: Improved low-cloud simulation from a multiscale modeling framework with a third-order turbulence closure in its cloud-resolving model component. J. Geophys. Res., 116, D14101, doi:10.1029/2010JD015362.<br><br>Sun, F., Hall, A., and Qu, X.: On the relationship between low cloud variability and lower tropospheric stability in the Southeast Pacific, Atmos. Chem. Phys., 11, 9053-9065, doi:10.5194/acp-11-9053-2011, 2011.<br><br>Wielicki, B. A., E. F. Harrison, R. D. Cess, M. D. King, and D. A. Randall, 1995: Mission to Planet Earth: Role of Clouds and Radiation in Climate. Bull. Amer. Meteor. Soc., 76, 2125–2153. doi: 10.1175/1520-0477(1995)076<2125:MTPERO>2.0.CO;2<br>Xu, K.-M., T. Wong, B. A. Wielicki, L. Parker, B. Lin, Z. A. Eitzen, and M. Branson, 2007: Statistical analyses of satellite cloud object data from CERES. Part II: Tropical convective cloud objects during 1998 El Nino and evidence for supporting the fixed anvil temperature hypothesis. J. Climate, 20, 819-842.<br><br>Zhang, Y., B. Stevens, B. Medeiros, and M. Ghil, 2009: Low-cloud fraction, lower-tropospheric stability, and large-scale divergence. J. Climate, 22, 4827–4844. Doi: 10.1175/2009JCLI2891.1<br><br>Zhou, Y. P., K.-M. Xu, Y. C. Sud, and A. K. Betts (2011), Recent trends of the tropical hydrological cycle inferred<br>from Global Precipitation Climatology Project and International Satellite Cloud Climatology Project data, J. Geophys. Res., 116, D09101, doi:10.1029/2010JD015197.<br>[Kuan-Man Xu, USA] | you need to make explicit suggestion as to where in the chapter these references are relevant |
| 7-1529 | 7 | 63 | 12 | | | The title of this paper is incorrect. [Drew Shindell, USA] | corrected |
| 7-1530 | 7 | 66 | 41 | 66 | 42 | The Reference "DeMott et al., 2003" is missing in the text. [Panuganti China Sattilingam Devara, India] | this is correct but will be removed from the reference list dring the final editing |
| 7-1531 | 7 | 72 | 52 | 72 | 53 | Journal missing in Huneeus reference [Meinrat O. Andreae, Germany] | corrected |
| 7-1532 | 7 | 74 | 47 | 74 | 48 | The Reference "Karcher and Lohmann, 2003" is missing in the text. [Panuganti China Sattilingam Devara, India] | this is correct but will be removed from the reference list dring the final editing |
| 7-1533 | 7 | 75 | 54 | 75 | 55 | The Reference "Khvorostyanov and Curry, 2005" is missing in the text. [Panuganti China Sattilingam Devara, India] | this is correct but will be removed from the reference list dring the final editing |
| 7-1534 | 7 | 83 | 1 | 83 | 2 | The Reference "Meyers et al., 1992" is missing in the text. [Panuganti China Sattilingam Devara, India] | this is correct but will be removed from the reference list dring the final editing |
| 7-1535 | 7 | 90 | 52 | 90 | 53 | The Reference "Shaw et al., 2005" is missing in the text. [Panuganti China Sattilingam Devara, India] | this is correct but will be removed from the reference list dring the final editing |
| 7-1536 | 7 | 94 | 46 | 94 | 47 | The Refrence "Vali, 2008" is missing in the text. [Panuganti China Sattilingam Devara, India] | this is correct but will be removed from the reference |

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| | | | | | | | list dring the final editing |
| 7-1537 | 7 | 95 | 41 | 95 | 43 | change "2009" to "2009b"; and add "Wang, H., and G. Feingold, 2009a: Modeling open cellular structures and drizzle in marine stratocumulus. Part I: Impact of drizzle on the formation and evolution of open cells. J. Atmos. Sci., 66, 3237-3256." to the list; see comment #13. [Hailong Wang, USA] | See reply to other comment. |
| 7-1538 | 7 | 99 | 29 | 99 | 30 | The Reference "Zuberi et al., 2002" is missing in the text. [Panuganti China Sattilingam Devara, India] | this is correct but will be removed from the reference list dring the final editing |
| 7-1539 | 7 | 100 | 5 | 100 | 6 | Inaccurate units "Tg/year" are not needed in the table. [Hailong Wang, USA] | Units are explicitly called out in table. There is some variation across species and these are now called out |
| 7-1540 | 7 | 100 | | | | Table7.1: Please specify the inventories used [Andrew Ferrone, Germany] | Different inventories are used for different emission numbers. The cited paper has these details, so this is not needed here |
| 7-1541 | 7 | 100 | | | | Table7.1: Units for NMVOCs, BC and OC should be clarified as is done for NOx (e.g. Tg C yr^-1) [Andrew Ferrone, Germany] | Agreed, Units now explicitly called out |
| 7-1542 | 7 | 101 | | | | Marine biogenic POA are largely in the Aitken mode [Timothy Bates, USA] | Agreed, table corrected |
| 7-1543 | 7 | 101 | | | | The coarse mode sea salt mass size distribution tales into the accumulation mode range but I do not believe there is any evidence of an accumulation mode. [Timothy Bates, USA] | Agreed, table corrected |
| 7-1544 | 7 | 103 | 1 | 103 | 9 | Need to add another black arrow from GHG to clouds. This is the so called cloud 'rapid response' that comes from cloud changes from (rapid) stability changes from CO2 forcing (e.g. Gregory et al, Colman and McAvaney 2011.) [Robert Colman, Australia] | The reviewer is incorrect, as the rapid response must arise from the CO2 affecting the state variables (T, q, winds in the atmosphere) which then affect cloud cover. Clouds do not respond directly to photons. The correct interaction is shown in the "adjustment" loop. |
| 7-1545 | 7 | 103 | 2 | 103 | 2 | The overview of figure 7.1 should be improved. [Hua Zhang, China] | yes. |
| 7-1546 | 7 | 103 | | | | Fig7.1. As drawn 'rapid' adjustments affecting 'Clouds and Precip' must be in your red loop. However, the list of 'Other climate variables' should then also have heating response for completeness, for example. The listed effects are really of a different nature, esp with respect to time scale, than response effects which are considered 'rapid'. So I suggest perhaps adding a separate subloop of feedback to 'Cloud and precip' for rapid adjustments. This is perhaps a refinement that can be omitted if you expand the variables list. [David Fahey, USA] | The "other variables" list is meant to include all state variables orthogonal to global mean surface temperature, but it is impossible to list them all. It is not clear what the reviewer means by "heating response;" radiative heating is a process included in the "radiation" box, and temperature changes are included in the two boxes at right (one for global mean, one for local departures from teh global mean) |
| 7-1547 | 7 | 103 | | | | Figure7.1: We suggest adding an additional arrow from "Clouds&Precip" to "Other climate variables" that is not passing through the radiation box, as also non-radiative processes exist between these two boxes (e.g. effect of condensation on moisture and temperature profile) [Andrew Ferrone, Germany] | While this comment may technically be correct, the prevailing view of cloud and precipitation behaviour is that it is primarily a response or adjustment process to large-scale motions (except through its impact on radiation); adding more arrows would further obfuscate the key interactions shown. |
| 7-1548 | 7 | 103 | | | | Figure7.1: The arrows between the box "global surface temperature" and "other climate variables" should be in both ways and not one-way, as these two quantities are interacting. [Andrew Ferrone, Germany] | No, because the manner in which the other climate variables affect global mean temperature must act through radiation. |
| 7-1549 | 7 | 103 | | | | In the figure 7.1 schematic, why doesn't radiation affect clouds? We know that direct CO2 forcing of clouds is important (or probably important), but it is not represented. Or is thie represented by the red unlabeled arrow going from climate variables to clouds and precip. I'm not convinced the CO2 forcing needs to go through the climate variables (i.e., T,q) to affect clouds. [Robert Wood, USA] | The CO2 radiative effect definitely does need to go through state variables to affect clouds, since the photons are not directly altering the clouds (at least not in any of our models). |

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| 7-1550 | 7 | 104 | 1 | | | Panel b in Figure 7.2 can be improved. [Hailong Wang, USA] | panel has been redrawn. |
| 7-1551 | 7 | 104 | | | | Fig7.2 The addition of line representing the tropopause would give the reader a useful reference. [David Fahey, USA] | Will be added to panels 7. b and c. |
| 7-1552 | 7 | 104 | | | | Hand drawn figure…. [Larry Thomason, United States of America] | Has been redrafted |
| 7-1553 | 7 | 104 | | | | Figures 7.2b and 7.2c should be improved graphically. [Manfred Wendisch, Germany] | Fig. 7.2b has been redrafted. Vertical scale added to Fig. 7.2c. |
| 7-1554 | 7 | 105 | 4 | | | Fig. 7.3: COSP does not play a role here, these are retrievals, not model results. [Johannes Quaas, Germany] | The COSP in the caption was a preliminary reminder unintentionally left in the final draft to update a reference to a paper (Kay et al. 2012) from which this figure came. The figure uses only satellite data and not COSP output. |
| 7-1555 | 7 | 105 | 5 | | | check reference to Kay 2011; doesnt seem correct or approp; paper seems limited to Arctic. Is color scale same for a and b? Suggest if possible all figs that have latitude as indep variable transform to sin(lat) as indep variable so as not to present misleading picture. [Stephen E Schwartz, USA] | Ref should have been to Kay et al. (2012) which is a comparison of CAM5 COSP results and global satellite cloud obs. Figures are being redrafted - but sin transformation of latitude coordinate on right panel, while a worthy idea, is not used in most other figures in this chapter or the IPCC report.. |
| 7-1556 | 7 | 105 | | | | Labels are too small on left figure and scale [Larry Thomason, United States of America] | Right figure, maybe? This figure is being redrafted, which should address these concerns. |
| 7-1557 | 7 | 105 | | | | Figure 7.3. The caption is not clear, but it looks to me as if 7.3(b) is COSP output. Since simulators are now covered in a different chapter, perhaps a Cloudsat figure would be more appropriate here? Bodas et al (2011) (already cited) has several suitable figures. [Mark Webb, United Kingdom of Great Britain & Northern Ireland] | The COSP in the caption was a preliminary reminder unintentionally left in the final draft to update a reference to a paper (Kay et al. 2012) from which this figure came. The figure uses only satellite data and not COSP output. |
| 7-1558 | 7 | 106 | 1 | | | need to add panel numbers to Figure 7.4, which are referred to in the text; the current color scheme should be improved to better distinguish between positive and negative values in the top two panels. [Hailong Wang, USA] | done |
| 7-1559 | 7 | 106 | 4 | | | Fig. 7.4: It is necessary to clarify that this is the distribution for the present day; the averaging time should be indicated. I think it would be useful to have the same colour code for the three radiation panels. [Johannes Quaas, Germany] | done. We already tried using the same color for net CRE as for the LW and SW component, but the dynamic range is too small for this to be effective. Instead we note the different scale in the revised caption. |
| 7-1560 | 7 | 106 | | | | Figure 7.4: Please indicate a, b, c, d (as referred to in the text) to distinguish the four panels. [Manfred Wendisch, Germany] | done |
| 7-1561 | 7 | 106 | | | | Fig 7.4: What time period is shown in this figure? [Thomas Stocker/ WGI TSU, Switzerland] | added to caption |
| 7-1562 | 7 | 107 | | | | Fig 7.5: We note inconsistency in the way in which forcing/ feedback figures are plotted in the different chapters (i.e., swapping of the X and Y axis) See for example chapters 6 and 8. We suggest chapters coordinate and use a consistent layout. [Thomas Stocker/ WGI TSU, Switzerland] | suggestion noted |
| 7-1563 | 7 | 107 | | | | Fig. 7.5. The chapter could use some new ways to show different components of cloud feedbacks, or is the plan to only include an updated version of the Colman figure? I was hoping that in AR5 cloud feedbacks from the multi-model ensembles will be broken down into LW/SW, high/low clouds, direct CO2 vs surface T responses, etc. [Robert Wood, USA] | yes this will be done |
| 7-1564 | 7 | 108 | 1 | | | "Clouds" in this conceptual diagram can be better illustrated. [Hailong Wang, USA] | we will try |

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| 7-1565 | 7 | 108 | | | | Figure7.6: An indicative scale of altitude and latitude on the two plots could help the interested reader to better understand them. [Andrew Ferrone, Germany] | yes we will do this |
| 7-1566 | 7 | 108 | | | | Fig 7.6: Currently the responses illustrated here are rather subtle and difficult to indentify. We suggest some further work to this schematic to clearly highlight the important responses you want the reader to identify. [Thomas Stocker/ WGI TSU, Switzerland] | suggestion noted |
| 7-1567 | 7 | 108 | | | | Fig 7.6. Optically-relevant clouds do not top out at the tropical tropopause as shown in the figure. FAT isn't necessarily dependent upon a rising tropopause as far as I know. Ozone and CO2 radiative effects are also a large part of the tropopause change story and that has nothing to do with FAT. [Robert Wood, USA] | To a first approximation the rise in tropopause and cloud-top height are of similar magnitude and both driven by increases in atmospheric opacity associated with the climate change, though the reviewer is correct that the relative importances of the different gases vary with altitude in the TTL region. The text discusses this a bit more precisely but there is not room in the chapter to discuss all the sublteties to the degree one might ideally like. |
| 7-1568 | 7 | 109 | | | | Fig7.7 This is a useful figure. Suggest changing 'volatile' to 'volatility' in 2 places. The 'Particulate' box is awkward because 'Secondary particles' and 'Primary Particles' are 'Atmospheric particles'. Suggest replacing 'Atmospheric particles' with 'Processed aerosols' and having all 3 lines from particle types merge into one line and connect to Optical properties and Cloud condensation on the right. This recognizes that 'Secondary particles' and 'Primary Particles' need not be further processed to affect the atmosphere. [David Fahey, USA] | very useful comments. Figure changed accordingly. |
| 7-1569 | 7 | 109 | | | | Figure7.7: The aim of this plot is to highlight the aerosol processes and their influence on the meteorological variables. However as the interactions with the atmosphere as a whole and in particular mutual interactions are of high importance this should be indicated. In particular the "junctions" on the right side of the plots where certain atmospheric variables are influencing the aerosol properties to lead to the semi-direct and indirect effect hide a large part of complexity and uncertainty. This should be better highlighted in the plot. [Andrew Ferrone, Germany] | this is correct. Rather than modifying the plot, we have added a caveat in the figure caption to say that this is a coupled system. |
| 7-1570 | 7 | 110 | 1 | 110 | 1 | The figure should be complimented by other major components of the atmospheric aerosol (sea salt, primary biological particles). Desert aerosol is included, but concentrations over the oceans are missing, see Prospero, J.M. (2006): Saharan Dust Impacts and Climate Change. Oceanography 19 (2), 60-61 and references therein [Ruprecht Jaenicke, Germany] | We assume this comment refers to page 111 (figure 7.9) rather than page 110. We have now added two panels for marine aerosols. PBAP is discussed in the text, but there is no sufficient data (ie longer than 1 year dataset) for inclusion in this plot. |
| 7-1571 | 7 | 110 | | | | What is the solid curve in Fig. 7.8 (which is otherwise a very nice synthesis figure)? [Anthony Del Genio, USA] | solid line shows supersaturation with respect to liquid water. Will be added to caption. |
| 7-1572 | 7 | 110 | | | | <Figure 7-8> "Bioaerosol" appears only in this figure, and not in the text. Is this equivalent for "bacteria" in the text? [Yoko Yokouchi, Japan] | bio-aerosols is synonymous to PBAP here and include "bacteria, fungi spores and pollen". |
| 7-1573 | 7 | 111 | | | | Figure 7.9: Would it be possible to also indicate how the models used for the forcing estimates reproduce these concentrations? [Michael Schulz, Norway] | that would be nice but far too much work! |
| 7-1574 | 7 | 111 | | | | Purpose of figure not clear; why presenting this? Lots of information; but it's PM10, not all that relevant; and no assessment, eg of whether the measurements are suitable for a given purpose (empirical determination of forcings; model eval); or of their accuracy, or adequacy of spatial coverage; or the like. just graphs of mean mass concentrations. [Stephen E Schwartz, USA] | These kind of data syntheszed in this figure can be used to validate the output of climate model, and constrain the uncertainties of the forcing estimation. |
| 7-1575 | 7 | 111 | | | | Fig 7.9: A very useful figure, but consider using a large (full page) map, with individual regional panels positioned appropriately over top of the map. Explain uncertainties and the year used for the analyses. Were all measurements normalized to a common year? [Thomas Stocker/ WGI TSU, Switzerland] | We will consider using a large map. Because not all observations were conducted in the same year, it is difficult at this moment to normalize all measurements to a common year |
| 7-1576 | 7 | 112 | | | | Fig 7.10: Please explain the one-sided error bar weight [Thomas Stocker/ WGI TSU, Switzerland] | 1 s.d. |
| 7-1577 | 7 | 113 | 1 | | | "CAM5" line seems to be missiong from in the Figure 7.11; Has it been accounted in the "Model mean"? | new AeroCOM II figure |

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| | | | | | | [Hailong Wang, USA] | |
| 7-1578 | 7 | 113 | 5 | 113 | 5 | References on these models should be given here. [Hua Zhang, China] | models now referenced in Myhre et al 2012 |
| 7-1579 | 7 | 113 | | | | Suggest all zonal mean plots be plotted versus sin(lat) so as not to present distorted picture; suggest all line plot data such as these be provided online. [Stephen E Schwartz, USA] | sine(lat) now used |
| 7-1580 | 7 | 114 | 1 | 114 | 7 | Table 7.12 and Chapter 7 in general. Except for the discussion with respect to the BC-snow-albedo forcing, this chapter and Table 7.12 do not discuss the effects of biofuel soot on climate at all. It discusses only fossil-fuel and open biomass-burning soot. The overall effects of fossil fuel soot and biofuel soot are each discussd in Jacobson, M.Z., Short-term effects of controlling fossil-fuel soot, biofuel soot and gases, and methane on climate, Arctic ice, and air pollution health, J. Geophys. Res., 115, D14209, doi:10.1029/2009JD013795, 2010. The paper shows in Figure 5g that the TOA irradiance changes (close to Adjusted Forcings) due to both fossil-fuel soot alone and fossil-fuel soot plus biofuel soot are strongly positive (the difference between the FSBSG and FS curves indicates that biofuel soot irradiance change is positive). The climate response from that study indicates that both also cause strong surface warming. [Mark Z. Jacobson, U.S.A.] | biofuel is lumped with fossil-fuel in both AEROCOM and our estimates. This will be made clear in the SOD. |
| 7-1581 | 7 | 114 | 4 | 114 | 6 | The term anthropogenic must be included here. [Ruprecht Jaenicke, Germany] | yes, caption will be revised |
| 7-1582 | 7 | 114 | | | | Figure 7.12: this is essentially same with Fig.8.18, but uncertainty ranges are slightly different. [Shigeki KOBAYASHI, Japan] | plots will be made consistent |
| 7-1583 | 7 | 114 | | | | Given the new appreciation of the importance of SOA since AR4 and the finding that much SOA is anthropogenically mediated (see comment on chapter 7, page 23 line 8) I would have expected SOA to exert much stronger forcing than is shown here; suggest discuss. [Stephen E Schwartz, USA] | this is still uncertain as discussed in section 7.3. SOA also somewhat absorbing. |
| 7-1584 | 7 | 114 | | | | Fig 7.12: We note inconsistency in the way in which forcing/ feedback figures are plotted in the different chapters (i.e., swapping of the X and Y axis) See for example chapters 6 and 8. We suggest chapters coordinate and use a consistent layout. [Thomas Stocker/ WGI TSU, Switzerland] | it is more important to be consistent within each chapter. |
| 7-1585 | 7 | 115 | 4 | | | Fig. 7.13: In AR4, a table was provided on the different estimates of the iRF/iAF that contributed to the overall assessment. Such a table, where the references are connected to the individual numbers, and from which it becomes more clear which model and method lead to which estimate, would be more transparent that the currently-provided figure. In Fig. 7.13b there seems to be a problem with the statistics: E.g. for “iAF-satellites” the distribution seems unskewed, yet the mean value is very different from the median. [Johannes Quaas, Germany] | Good point about the table. We will consider adding it. The iAF-satellite is correct. The mean is -0.7 W/m2, whereas the median is -0.5 W/m2. |
| 7-1586 | 7 | 115 | | | | Figure7.13a: The reading would be clearer if a zero reference line would be added to the plot [Andrew Ferrone, Germany] | not necessary as the plot it bound by 0. |
| 7-1587 | 7 | 115 | | | | Figure7.13a: It would be interesting to add the number of studies considered for each category on the plot [Andrew Ferrone, Germany] | the number of studies equals the number of symbols in each category |
| 7-1588 | 7 | 115 | | | | Figure7.13a + legend: Please eliminate the inconsistency between the plot and the legend concerning the "warm" and "liquid" designations [Andrew Ferrone, Germany] | Figure revised |
| 7-1589 | 7 | 115 | | | | It would be a lot of value added if some device could be found to better associate individual points (line segments) on the graph with the responsible studies. It is tedious to try to match them from the caption, and I never quite succeeded with the first set, counting 9 in the caption and 10 in the figure. Maybe letters of the alphabet or shorthand references, eg BL95, in the figure itself. They are evidently in alpha order by groups; might be more value added to do in date order as in Lohman's paper. But within groups, as here. Caption might be in tabular form rather than para form for ease in reading, possibly using color coding. I would like to quickly locate who had the -5 value at the far left of the lower figure, place that in date context with the others. Ordering by date also allows to see if any convergence. But all that said, its a very rich figure, and with a little more effort could be a lot richer. Not sure figure b adds anything. [Stephen E Schwartz, USA] | The figure has been revised so that a and b give different messages. We think about a way to make the Figure caption more readable. In each group, the papers are in chronological order according to their year of publication. |
| 7-1590 | 7 | 115 | | | | I really do not think it possible to estimate aerosol indirect RF from satellites. How does one figure out what the clouds in region X or region Y were like in preindustrial times? I understand in a broad sense how these | In the most studies satellite data were used in combination with GCMs. Please look at the references |

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| | | | | | | estimates are made, but it is not a well-posed problem without control (preindustrial) information. It would seem like a relatively simple exercise to use climate model simulations to assess whether the methodology is fundamentally sound. Has this been done? I'm aware that Joyce Penner is looking at this with her model and she argues that the satellite estimates are fundamentally biased. I'm not sure if this is published result. To my mind, it is questionable whether we can use the satellite data to argue that the models produce AIEs that are too strong. There appears to be no discussion of this huge elephant in the room in the chapter other than saying that it might be "difficult to separate iRF from cloud fast feedbacks" in observations. [Robert Wood, USA] | (mostly from Quaas et al) for more detail. Yes, Yoyce's study has been published and is included in the revised plot. |
| 7-1591 | 7 | 116 | 2 | 116 | 4 | For the "inverse estimate" the iRF contains only a single estimate and an indication of a best estimate is not possible, however there are several studies indicated for the iAF, so here a best estimate for this category could be indicated. [Andrew Ferrone, Germany] | we avoid giving a best estimate of the inverse estimates because their goal is to provide bounds and that's what we show in the figure. |
| 7-1592 | 7 | 117 | | | | Fig7.13b  Suggest harmonizing the terminology and values between this figure and Fig. 8.20. [David Fahey, USA] | taken into account |
| 7-1593 | 7 | 117 | | | | Figure7.13b: Eliminate inconsistency of "liquid" and "warm" in accordance with Figure7.13a and correct "AIF-inverse" to "iAF-inverse" on the right hand-side of the plot [Andrew Ferrone, Germany] | Figure has been revised and is now more consistent |
| 7-1594 | 7 | 117 | | | | Fig 7.13b: Not clear to us what additional information this figure provides relative to Fig 7.13a., suggest to combine into one figure [Thomas Stocker/ WGI TSU, Switzerland] | Figure has been revised so that now Fig b provides a different message from Fig a |
| 7-1595 | 7 | 118 | 6 | 118 | 6 | "Kravitz et al., 2011" is wrong. The correct citation is: "Schmidt et al. (2012), cf. doi:10.5194/esdd-3-31-2012 [Jón Egill Kristjánsson, Norway] | a new figure has been produced and the caption will be adapted accordingly. |
| 7-1596 | 7 | 118 | 6 | 118 | 6 | Here the reference should be to Schmidt et al. (2012) rather than to Kravitz et al. (2011) [Mark Lawrence, Germany] | a new figure has been produced and the caption will be adapted accordingly. |
| 7-1597 | 7 | 118 | 6 | 118 | 6 | The meaning of different lines in the figure should be given here. [Hua Zhang, China] | a new figure has been produced where the model names are labelled. |
| 7-1598 | 7 | 118 | | | | Figure7.14: Please provide a legend for the different lines plotted [Andrew Ferrone, Germany] | a new figure has been produced where the model names are labelled. |
| 7-1599 | 7 | 118 | | | | Labelsare too small [Larry Thomason, United  States of America] | a new figure has been produced where the model names are labelled. |
| 7-1600 | 7 | 118 | | | | Fig 7.14: We suggest that you add global mean to these figures, thereby allowing a comparison with the assessment given in Chapter 12. [Thomas Stocker/ WGI TSU, Switzerland] | global means are not relevant because different degrees of titration between GHG and solar forcing is possible. |
| 7-1601 | 7 | 119 | 1 | 119 | 1 | Lower left: "leading to surface warming" is wrong, unless climate response is included. The radiative forcing of absorbing aerosols is strongly negative at the surface. However, if the TOA forcing is positive, then eventually, there will tend to be a warming at the surface, especially if cloud formation is suppressed due to reduced evaporation. [Jón Egill Kristjánsson, Norway] | it was our intent to include the climate response. The figure has been redrawn to make this clear. |
| 7-1602 | 7 | 119 | 1 | 119 | 1 | Right panel: "cloud nuclei" is ambiguous. Replace by "cloud condensation nuclei or ice nuclei" [Jón Egill Kristjánsson, Norway] | done. |
| 7-1603 | 7 | 119 | 1 | 119 | 1 | The figure for absorbing aerosols are misleading. Absorbing aerosols produces negative surface forcing first. Considering feedback, it may be correct. The figure for the indirect effect is only for water cloud. [Toshihiko Takemura, Japan] | it was our intent to include the climate response. The figure has been redrawn to make this clear. |
| 7-1604 | 7 | 119 | 1 | 119 | 4 | The right two panels are misleading. As a global mean, this figure is correct. But regionally, the surface cooling near anthropogenic aerosol sources is mainly caused by absorbing aerosols, so it might be better to add "as global mean" in the caption. Actually this figure is very important, but is difficult to understand for public (even scholors without professional background in this field) who sometimes say that absorbing aerosols strongly cool the surface so that GHG warming is easily cancelled by aerosols; of course, which is wrong. So we need a bit more explanation for clear understanding of this figure. Also use of "absorbing aerosol" and "scattering | it was our intent to include the climate response. The figure has been redrawn to make this clear. |

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| | | | | | | aerosol" will be too ambiguous, because there are no such distinct differences in aerosol species; "aerosol scattering" and "aerosol absorption" might be OK. [Teruyuki Nakajima, Japan] | |
| 7-1605 | 7 | 119 | 1 | | | I think the schematic for "absorbing aerosol" is misleading (in terms of "surface warming"). [Hailong Wang, USA] | it is correct if it includes the climate response. The figure has been modified. |
| 7-1606 | 7 | 121 | 1 | | | need some words to explain the third method (clockwise) in Figure 2. [Hailong Wang, USA] | full caption has been added. |
| 7-1607 | 7 | 121 | | | | FAQ7.3,Figure2: Please add a description to the "foam technique" indicated in the ocean. [Andrew Ferrone, Germany] | full caption has been added. |