Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7-1	7	0	0	0	0	A novel and correct attempt to address clouds and aerosols together. In general, the chapter is well-written and up-to-date. There still is some scope to improve the document. My suggestions to follow, basically focus on South Asian region [K KRISHNA MOORTHY, INDIA]	Noted, no action needed.
7-2	7	0	0	0	0	Overall I found this to be an impressive chapter, covering many key topics in climate science, for both feedbacks and forcing. As I will note below, there were a few places where I felt that the reasoning in reaching particular conclusions was not compelling and either the evidence needs to be better presented or the conclusion modified. I also query the chapter title. I did not expect to find information on either the water vapour feedback or precipitation in this chapter. On another wider issue, I believe that the ari and aci split generally works well and is a helpful advance, but I think there is really some haziness about where the semi-direct should fall - I think this should be acknowledged [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Noted. Individual comments will be addressed below. The semi-direct belongs to AFari as it is initially caused by the impact of aerosols on radiation. It can of course interact with aci.
7-3	7	0	0	0	0	Better links and more consistency between Chapter 6, specifically Section 6.5.4 and Chapter 7, specifically Section 7.7.3 would improve the quality of both sections and chapters. [Naomi Vaughan, United Kingdom]	Taken into account. We have added cross-references to chapter 6 in the introduction of Section 7.7 and FAQ7.3. FAQ7.3 has been rewritten together with Chapter 6 LA and CLAs.
7-4	7	0	0			I commend the authors on putting together a thorough and well-written summary and review of the role of clouds and aerosols in climate and climate change, especially given the challenge of this task. In particular, I like the use of “radiative forcing due to aerosol-cloud interactions”, RFaci, and “adjusted forcing due to cloud-aerosol interactions”, AFaci, to describe the effects of aerosol-cloud-radiation interactions. In my opinion this is much better than trying to analyze specific effects in isolation as has been done frequently in the past, since as stated on p. 7-35 there is evidence for significant interaction and compensation between effects. However, I think this nomenclature is confusing in a few places; specific instances of this are noted below. The authors have also done a very good job highlighting the distinction between aerosol influences on individual clouds, versus the aerosol-cloud-precipitation system as a whole (e.g., lines 16-21 on p. 7-4 of the Executive Summary). I also appreciate the authors’ focus on how effects vary in different regimes (lines 48-52 on p. 7-40 state this point very nicely), and how this diversity of interactions and underlying complexity has important consequences for modeling overall global-scale effects. I think the regime-dependent context could be emphasized even further; specific suggestions are given in comments below. Several other specific minor comments are also detailed below. Finally, while I have read through the entire chapter, most of my comments focus on section 7.4 since this is the section I was specifically asked to review. Nonetheless, I do have several comments pertaining to other sections as well, including the figures. [Hugh Morrison, United States]	Noted, specific comments will be addressed below.
7-5	7	0	0			Reference list for comments: Bogegnschutz, P. A., A. Gettelman, H. Morrison, V. E. Larson, D. P. Schanen, N. R. Meyer, and C. Craig, 2012: Unified parameterization of the planetary boundary layer and shallow convection with a higher-order turbulence closure in the Community Atmosphere Model: Single column experiments. Geosci. Model Dev., 5, 1407-1423. de Boer, G., H. Morrison, M. D. Shupe, and R. Hildner, 2011: Evidence of liquid dependent ice nucleation in high-latitude stratiform clouds from surface remote sensors. Geophys. Res. Lett., 38, L01803, doi:10.1029/2010GL046016. Fan, J., L. R. Leung, Z. Li, H. Morrison, Y. Qian, Y. Zhou, and H. Chen, 2012: Aerosol impacts on clouds and precipitation in southeast China - Results from bin and bulk microphysics for the 2008 AMF-China field campaign. J. Geophys. Res., 117, D00K36, doi:10.1029/2011JD016537. Golaz, J.-C., Larson, V. E., and Cotton, W. R.: A pdf-based model for boundary layer clouds part I: method and model descrip- tion, J. Atmos. Sci., 59, 3540–3551, 2002. Grabowski, W. W.: Indirect impact of atmospheric aerosols in idealized simulations of convective-radiative equilibrium, J. Climate, 19, 4664-4682, 2006. Guo, H., Golaz, J.-C., Donner, L. J., Larson, V. E., Schanen, D. P., and Griffin, B. M.: Multi-variate probability density func- tions with dynamics for cloud droplet activation in large-scale models: single column tests, Geosci. Model Dev., 3, 475–486, doi:10.5194/gmd-3-475-2010, 2010. Hill, A. A., G. Feingold, and H. Jiang, 2009: The influence of entrainment and mixing assumption on aerosol-cloud interactions in marine stratocumulus. J. Atmos. Sci., 66, 1450-1464. Khairoutdinov, M., and Kogan, Y., 2000. A new cloud physics parameterization in a large-ddy simulation	Noted, specific comments will be addressed below and references will be considered.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						model of marine stratocumulus. Monthly Weather Review, 128(1): 229-243. Larson, V. E., and B. M. Griffin, 2012: Analytic upscaling of a local microphysics scheme. Part I: Derivation, Quart. J. Roy. Meteor. Soc., DOI: 10.1002/qj.1967. (in press, available through early online release) Lebo, Z. J., and Seinfold, J. H.: Theoretical basis for convective invigoration due to increased aerosol concentration, Atmos. Chem. Phys., 11, 5407-5429, 2011. Lohmann, U., 2008. Global anthropogenic aerosol effects on convective clouds in ECHAM5-HAM. Atmospheric Chemistry and Physics, 8: 2115-2131. Morrison, H., and W. W. Grabowski, 2011: Cloud system-resolving model simulations of aerosol indirect effects on tropical deep convection and its thermodynamic environment. Atmos. Chem. Phys., 11, 10503-10523, doi:10.5194/acp-11-10503-2011. Posselt, R., and Lohmann, U., 2009. Sensitivity of the total anthropogenic aerosol effect to the treatment of rain in a global climate model. Geophysical Research Letters, 36: L02805. Slawinska, J., W. W. Grabowski, H. Pawlowska, and H. Morrison, 2012: Droplet activation and mixing in large-eddy simulation of a shallow cumulus field. J. Atmos. Sci., 69, 444-462. van den Heever, S. C., G. L. Stephens, and N. B. Wood, 2011: Aerosol indirect effects on tropical convective characteristics under conditions of radiative-convective equilibrium. J. Atmos. Sci., 68, 699-718. Wang, M., S. Ghan, M. Ovchinnikov, X. Liu, R. Easter, E. Kassianov, Y. Qian, and H. Morrison, 2011: Aerosol indirect effects in a multi-scale aerosol-climate model PNNL-MMF. Atmos. Phys. Chem., 11, 5431-5455, doi:10.5194/acp-11-5431-2011. Wang, M., S. Ghan, X. Liu, T. L'Ecuyer, Kai Zhang, H. Morrison, M. Ovchinnikov, R. Easter, R. Marchand, D. Chand, Y. Qian, and J. E. Penner, 2012: Constraining cloud lifetime effects of aerosols using A-Train satellite observations. Geophys. Res. Lett., 39, L15709, doi:10.1029/2012GL052204, 7 pp. Yang, Q., W. I. Gustafson, Jr., J. D. Fast, H. Wang, R. C. Easter, M. Wang, S. J. Ghan, L. K. Berg, L. R. Leung, and H. Morrison, 2012: Impact of natural and anthropogenic aerosols on marine stratocumulus and precipitation in the Southeast Pacific: A regional modeling study using WRF-Chem. Atmos. Chem. Phys., 12, 8777-8796. [Hugh Morrison, United States]	
7-6	7	0	1			Consistency in assessment numbers: Because chapter assessments continue to be refined, please check carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Errors in the numbering of Tables were introduced by the TSU when handling the SOD.
7-7	7	0	2			Treatment of Uncertainty: please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. [Thomas Stocker/ WGI TSU, Switzerland]	likelihood statements have been edited for consistency or removed within the chapter in agreement with IPCC guidance note.
7-8	7	0	3			Format of Executive Summary (ES): As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
7-9	7	0	4			Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	Done. Sections have been cross-references except in places when the cross-reference is general to the chapter.
7-10	7	0	5			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any	Noted. There are cases where a simple reference to

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	AR4 is actually useful, eg in sentence such as "Studies since AR4 have focused on…"
7-11	7	0	6			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
7-12	7	0	7			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
7-13	7	0	8			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II - Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
7-14	7	0				Congratulations for the chapter as a whole ! I enjoyed reading it, appreciate the quality of the argumentation underlying the assessment, and was impressed by the amount of information conveyed and summarized by the conceptual figures. This chapter represents a key progress in our assessment of the role of cloud and aerosol processes in climate change compared to previous IPCC assessments. I identified just a few gaps and I have many comments. All of them are meant to be constructive, and I hope they will be considered as such. [Sandrine BONY, France]	Noted. Thanks. Specific comments will be addressed below.
7-15	7	0				Using the concepts of radiative forcing, adjusted forcing and feedbacks to discuss the complex interactions between aerosols, clouds, radiation and climate makes a lot of sense. Organizing the IPCC assessment along this line is an excellent initiative. [Sandrine BONY, France]	Noted. Thanks.
7-16	7	0				The chapter is significantly improved from the FOD version and now appears balanced among all the subject areas. As such I no longer have overarching comments about the chapter organization and structure, only a moderate number of specific comments, as detailed below. I congratulate the authors on their efforts to put this increasingly complex subject into publishable form. [Anthony Del Genio, United States of America]	Noted. Thanks.
7-17	7	0				The 7th Chapter essentially deals with a comprehensive study of 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. A novel approach has been followed, in which the individual forcings involving instanataneous direct changes, and the adjusted ones from feedbacks that occur indirectly through atmospheric and surface changes, and resultant forcing from their interactions. Better understanding of rapid adjustments could narrow the uncertainty in climate projections. It also provides an overview on rapid integrated response of aerosols and clouds of all varieties to the precipitation in changing climate through their modulation of solar radiation. The state-of-the-art multi-scale, process / climate models and associated parameterization schemes, data assimilation techniques and sensitivity studies against the existing in-situ and remote sensing observations have been highlighted in this Chapter. The deviations are attributed to the existing theories and new discoveries. This Chapter concludes with a discussion on merits / demerits and future scope of some proposed geoengineering methods like Solar Radiation Management (SRM) etc. to counter undesirable impacts of climate change on the planet. [Panuganti, C.S. Devara, India]	Noted.
7-18	7	0				There is a large difference of quality between the cloud and aerosol sections. The cloud section is generally getting lost in small details, often incoherent, and lack synthesis of major scientific progresses since AR4. Its seems to have been an operation of copy-and-paste from papers of close friends. [Paul Ginoux, United States of America]	Noted.
7-19	7	0				It is better to avoid the use of Organic Carbon and OC in this chapter unless it is absolutely necessary (e.g., measurements of OC). Right now it is mainly confusing the audience and in a few cases it is actually wrong. [European Union]	Partly taken into account. We do use other terms when appropriate (organic matter, primary organic matter, biological particles, …) but there are cases when OC is meant (measurements, emission inventories).
7-20	7	0				The authors of the chapter have decided to use new forcing terminology in this chapter. However, in the	Partly taken into account. The SPM and TS have to

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Summary for Policymakers the old terminology (Direct effect and Impact of aerosols in clouds) is used for example in Figure SPM.3. I agree with the choice of not changing the terminology for the policymakers. However, this creates a discontinuity between this chapter and the summary. It is not clear what is included in the direct effect reported in the summary and what is in the impact of aerosols in clouds. A bridging section is probably needed. [European Union]	rely on the chapter contents. In principle no bridging section is required. The SPM and TS will use the same forcing concept as in Chapters 7 and 8. It has been renamed effective RF to this purpose.
7-21	7	0				The order in the chapter is in my point of view not the best possible. The more logical order would be first aerosols then clouds [European Union]	Rejected. The ordering of the sections follows that of the chapter title, which is a given.
7-22	7	0				Particularly the cloud part of the chapter is too much focusing on models. It is important to remember that besides models observations and process understanding need to be improved [European Union]	Taken into account. Cloud observations are discussed upfront and there are several figures to highlight observations. Process understanding is also largely discussed in both the cloud and the aerosol-cloud sections. IPCC does not however make statement as to what needs to be improved.
7-23	7	0				Overall comment: It is concerning that in several places the report talks about SRM being able to offset radiative forcing, and only later brings in regional residual effects on temperature and precipitation. More prominence should be given to the regional issues within this chapter. [European Union]	Partly taken into account. More prominence is given in discussing uncertainties and risks. We have kept however the two-stage discussion, which first focuses on the potential of several SRM techniques to produce a RF of a given magnitude, and then treat about the climate response and the residual effects.
7-24	7	0				From our point of view, the explanations with regard to SRM sound relatively positive in consideration of the negative side effects which could be connected with SRM-methods. [Government of Germany]	taken into account. More prominence is given to negative side effects and the limitations of available modelling is highlighted.
7-25	7	0				We take note that there is no mention of short lived climate forcers in this chapter. It would be relevant to mention them several places. One example is page 28, line 47-49. "BC and organics have received increasing attention". [Government of NORWAY]	Rejected. We discuss "aerosols" which are indeed short-lived forcings, but the terminology of short- vs long-lived forcings is introduced in Chapter 8.
7-26	7	0				We suggest that this chapter also reflect on the relation between BC and EC. The reason for this is that BC measurements e.g. to improve emission inventories are scarce, hence measurements of EC is applied instead. The reader should learn at least in which direction the substitution of EC emissions factors for BC factors will alter the magnitude of emissions. [Government of NORWAY]	Detailed descriptions of EC are given in Section 2 of Bond et al. [2013]. EC is measured by thermal method in which BC (on-volatile or refractory) and OC (volatile) are separately quantified in the form of CO_2, which is produced by combustion of BC and OC. BC measured by this analytical method is operationally defined as EC. The uncertainty of BC measured by this technique depends on details of the method, namely, how accurately BC and OC are separated. There is no need to discuss EC separately, because 1) here BC is clearly defined as an entity being uninfluenced by co-existence of non-BC compounds and 2) EC is basically identical to BC, except for this uncertainty. If EC emission inventory is based on the thermal method, it may be influenced by OC but it is very difficult to assess the uncertainty quantitatively. It can be overestimatde or underestimated as compared with the BC emissions, which is more rigorously defined here. Considering this, it is reasonable to assume that EC emissions and BC emissions are the same. We do mention EC in the context of Fig 7.13 though.
7-27	7	0				Speaking about climate engineering (or geoengineering) we should remember that such measure should be implemented to avoid global climatic catastrophe when other mitigation measures will fail. Under such	Rejected. The chapter has to rely on existing literature and there is yet little if any literature on SRM use in

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						condition it is reasonable to compare effects of geoengineering with the most aggressive (but realistic) scenario of greenhouse global warming (for example, A2 or A1FI). Another aspect is in the fact that geoengineering technologies must be developed and tested in limited experiments as soon as possible because nobody can say when we can approach to beginning of climatic catastrophe. I am sure that such idea must be reflected in the text of the report. [Government of Russian Federation]	RCP8.5 scenarios (GeoMIP relies on RCP4.5 scenario). This Chapter relies on more generic idealised experiment. Furthermore any recommendation on technology development and testing would be policy-prescriptive and are therefore not appropriate for this report.
7-28	7	0				In the discussion of different SRM approaches the idea of practical realization should be reflected. Otherwise reader would not have a right understanding about possibility of implementation of different technics. I understood that implementation and cost issues are discussed in detail in Chapter 6 WGIII, but in order to give a full impression of available technics, a few sentences should be added in this part of report also. [Government of Russian Federation]	Rejected. This is out of scope for this report. We discuss "geoengineering methods that have the potential to influence components of the energy budget by at least a few tenths of a W m–2 in the global mean without presuming their technological feasibility. "
7-29	7	0				D. With my regret I discovered that there are no references to works in the field of geoengineering published by Russian scientists. I understand that publications in Russian may be not easily available but there are several publications in English which could be appropriately included into the reference list. Below I enumerate several publications (translated into English and published in the USA) and give short characteristics of their contents. Besides, I would like to draw your attention to the fact that in Russia in the course of some years a series of limited natural experiments to study interaction of solar radiation with artificial aerosols has been conducted. [Government of Russian Federation]	Taken into account. Eliseev et al (2009) and Izrael et al (2009) are now cited in Section 7.7
7-30	7	0				Russian publications in the field of SRM geoengineering: Izrael Yu.A., 2005. An efficient way to regulate the global climate is the main objective of the solution of the climate problem. Russian Meteorology and Hydrology, No 10, pp. 1-4. In this work it was shown that Kioto methods are unrealistic and that the climate change problem can be solved by geoengineering with stratospheric aerosols. [Government of Russian Federation]	see reply to comment #7.30.
7-31	7	0				Russian publications in the field of SRM geoengineering: Izrael Yu.A. and Semenov S.M., 2006. Critical Levels of Greenhouse Gases, Stabilization Scenarios, and Implications for the Global Decisions. In: Avoiding Dangerous Climate Change, Schellnhuber, H J., Cramer, W., Nakicenovic, N., Wigley, T. and Yohe, G (Eds). Cambridge University Press, pp. 73 - 79. It this work a concept of acceptable temperature rise threshold (+2.5C) was suggested. [Government of Russian Federation]	see reply to comment #7.30.
7-32	7	0				Russian publications in the field of SRM geoengineering: Izrael Yu.A., Borzenkova I.I., Severov D.A.., 2007. Role of Stratospheric Aerosols in the Maintenance of Present-Day Climate. Russian Meteorology and Hydrology 32: 1-7. In this work some quantitative evaluations of implementation of stratospheric aerosol method were done. [Government of Russian Federation]	see reply to comment #7.30.
7-33	7	0				Russian publications in the field of SRM geoengineering: Izrael Yu.A., 2008. About modern climate state and suggestion on action to counteract climate change. Russian Meteorology and Hydrology, Vol. 33, No 10, pp. 611-613. In this work philosophy of geoengineering was considered. [Government of Russian Federation]	see reply to comment #7.30.
7-34	7	0				Russian publications in the field of SRM geoengineering:	see reply to comment #7.30.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Izrael Yu.A., Ryaboshapko A.G. and Petrov N.N., 2009. Comparative analysis of Geo-Engineering Approaches to Climate Stabilization. Russian Meteorology and Hydrology, Vol 34, No 6, pp. 335-347. In this work different geoengineering methods were considered using a set of criteria. [Government of Russian Federation]	
7-35	7	0				Russian publications in the field of SRM geoengineering: Izrael Yu. A., Zakharov V. M., Petrov N. N., Ryaboshapko A. G., Ivanov V. N., Savchenko A. V., Andreev Yu. V., Puzov Yu. A., Danelyan B. G., and Kulyapin V. P., 2009b. Field Experiment on Studying Solar Radiation Passing through Aerosol Layers. Russian Meteorology and Hydrology, Vol. 34, No 5, pp. 265-273. Izrael Yu.A., Zakharov V.M., Petrov N.N., Ryaboshapko A.G., Ivanov V.N., A.V.Savchenko, Yu.V.Andreev, Eran'kov V.G., Puzov Yu.A., B.G.Danilyan, Kulyapin V.P. and Gulevskii V.A., 2009c. Field Studies of a Geo-engineering Method of Maintaining a Modern Climate with Aerosol Particles. Russian Meteorology and Hydrology, Vol. 34, No 10, pp. 635-638. Izrael Yu.A., Zakharov V.M., V.N.Ivanov, Petrov N.N., Andreev Yu.V., Gulevskii V.A., Danilyan B.G., Eran'kov V.G., Kirin D.V., Kulyapin V.P., Rusakov Yu.S., Savchenko A.V., Svirkunov P.N., Severov D.A. and Folomeev V.V., 2011. A Field Experiment on Modeling the Impact of Aerosol Layers on the Variability of Solar Insolation and Meteorological Characteristics of the Surface Layer. Russian Meteorology and Hydrology, Vol. 36, No 11, pp. 705-711. In this series of publications the first attempts to develop technology of aerosol clouds creation in the free atmosphere are described. Assessments of parameters of interaction of solar radiation with aerosols of different composition are presented. [Government of Russian Federation]	see reply to comment #7.30.
7-36	7	0				Russian publications in the field of SRM geoengineering: Eliseev A.V., Chernokulsky A.V., Karpenko A.A. and Mokhov I.I., 2010. Global warming mitigation by sulphur loading in the stratosphere: dependence of required emissions on allowable residual warming rate. Theor Appl Climatol., 101:67–81 In this work relations between mass of stratospheric aerosol and climate response were investigated. [Government of Russian Federation]	see reply to comment #7.30.
7-37	7	0				Russian publications in the field of SRM geoengineering: Volodin E.M., Kostrykin S.V. and Ryaboshapko A.G., 2011. Climate response to aerosol injection at different stratospheric locations. Atmospheric Science Letters, doi: 10.1002/asl.351. This work was devoted to finding the optimal location of the stratosphere for injection of aerosol gaseous precursors. [Government of Russian Federation]	see reply to comment #7.30.
7-38	7	0				The Likelihood Table (Table 1.1) and Confidence figure (1.12) should be repeated in the SPM, TS and each Chapter and the terminology should be applied consistently. As an alternative to repeating the complete table/figure the material should be restated briefly in the SPM, TS, and each chapter. [Government of United States of America]	Taken into account: the likelihood and confidence language is applied consistently with the definitions provided in Chapter 1. It cannot however be restated or repeated in each chapter. We have added a cross-reference to Chapter 1.
7-39	7	0				A table of acronyms for each Chapter would be useful. Whule they may all be defined in one place elsewhere in the volume, it would be useful to have Chapter-specific acronym tables, as well, since most people will not peruse the entire report. Many acronyms are never defined or are defined after they have already appeared in the text. [Government of United States of America]	A list of acronyms and abbreviations will be provided as an Annex to the published Report (one list for the entire report, not chapter specific). All acronyms and abbreviations are now spelled out at the first usage.
7-40	7	0				Summary paragraphs at the end of each main section would be a useful addition. There were some sections	Partly taken into account. We have synthesis

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						that had good summaries (i.e. Feedback Synthesis) and others that had essentially no summary. This would help reiterate many of the main points in an otherwise very detailed/complex discussion. [Government of United States of America]	paragraphs where appropriate (cloud feedbacks, cosmic ray, geoengineering), but decided against having a synthesis paragraph in shorter sections or in sections which span a large amount of material but do not contribute important conclusions.
7-41	7	0				There is no discussion of stratospheric aerosol and its role in global warming. According to the recent Solomon et al. (2010) Nature paper, satellite aerosol observations imply a small negative forcing which should not be neglected in the GCMs. Also suggests there may be a trend in the background that could make this forcing variable. Is this discussed elsewhere? [Government of United States of America]	Stratospheric aerosols are treated in Chapter 8, as was mentioned on page 24, lines 13-14 of the SOD of Chapter 7.
7-42	7	0				Recommended important topics of the atmospheric aerosol have been added improving the Report. However, they just have been added. No consequence has been drawn, no reordering, no sorting hand can be seen in this chapter and/or in the whole Report. If uncertainties are increasing because certain parts of the aerosol had been left out earlier and added now, such uncertainties must mirror in other chapters as well. All details are sitting side by side and nobody cares. In my comments No. 3, my point has been made. [Ruprecht Jaenicke, Germany]	Noted. More specific comments from this reviewer will be addressed below.
7-43	7	0				I can't believe that Paulo Artaxo, Graham Feingold, Veli-Matti Kerminen, Ulrike Lohmann, Philip Rasch, Bjorn Stevens, Steven Ghan, Corinna Hoose, Colin O'Dowd, Alan Robock, Sandro Fuzzi, Joyce Penner sign responsible for this chapter. They should know the atmospheric aerosol much better. [Ruprecht Jaenicke, Germany]	Noted. More specific comments from this reviewer will be addressed below.
7-44	7	0				Please replace “species” with compounds or constituents. Specie belong to the world of animals (e.g. phytoplankton) not to chemistry. This should also apply to Tables or Figures (incl. Legends) whenever used. [Caroline Leck, Sweden]	Rejected. The taxonomic vocabulary used in biology (species, genus, etc…) is commonly used in other scientific fields.
7-45	7	0				Plesae make sure all abbrevations in use are defined. [Caroline Leck, Sweden]	A list of acronyms and abbreviations will be provided as an Annex to the published Report (one list for the entire report, not chapter specific). All acronyms and abbreviations are now spelled out at the first usage.
7-46	7	0				Review on the IPCC Fifth Assessment Report – Chapter 7 Clouds and aerosols [Kuo-Nan Liou, U.S.A.]	Noted. Not a comment.
7-47	7	0				Preface: Per your request, I have carefully studied Section 7.4 Aerosol-Cloud Interactions in Chapter 7 Clouds and Aerosols. This section is an integral part of Clouds and Aerosols and as such I have read the entire Chapter. I would like to commend you for providing a thorough review and update of the contemporary and critical subject of aerosol-cloud interactions, which constitute perhaps the largest uncertainties in the understanding of climate change and global warming. Below are my comments and concerns. [Kuo-Nan Liou, U.S.A.]	Noted. Thanks. More specific comments from this reviewer will be addressed below.
7-48	7	0				Comments on “Executive Summary (ES)” [Kuo-Nan Liou, U.S.A.]	Noted. Not a comment.
7-49	7	0				(1) First, I respectfully submit that the 24 items contained in the ES are simply too many for any reader to have a comprehensive overview of the report. As discussed, clouds are generally formed from the aerosols serving as condensation or ice nuclei (heterogeneous nucleation) and aerosols and clouds interact microscopically and share intimately the sources of radiative energy emitted from the sun and the Earth and the atmosphere. Except for item 1, it appears that the ES can be grouped with reference to RF with sub-titles as follows: [Kuo-Nan Liou, U.S.A.]	Taken into account. the number of paragraphs (items) in the executive summary have been reduced to a number of 13. Sub-headings have been added to four different groupings.
7-50	7	0				(a) Clouds – Items 2-5 [Kuo-Nan Liou, U.S.A.]	see reply to comment #7.49.
7-51	7	0				(b) Aerosols – Items 6-9, 12- 15 [Kuo-Nan Liou, U.S.A.]	see reply to comment #7.49.
7-52	7	0				(c) Cloud-aerosol interaction – Items 11, 15, 18, 21 [Kuo-Nan Liou, U.S.A.]	see reply to comment #7.49.
7-53	7	0				(d) Aerosol-ice/snow interaction – Items 16-17 [Kuo-Nan Liou, U.S.A.]	see reply to comment #7.49.
7-54	7	0				(e) Cloud-aerosol-precipitation – Items 10, 19-20 [Kuo-Nan Liou, U.S.A.]	see reply to comment #7.49.

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7-55	7	0				(f) Solar Radiation Management and related techniques – Items 22-24 [Kuo-Nan Liou, U.S.A.]	see reply to comment #7.49.
7-56	7	0				Please check for the proper use of umlaut and other special characters in citations and references. [Ottmar Möhler, Germany]	Noted.
7-57	7	0				In general, the chapter is in good shape, especially for clouds [Daniel Murphy, United States of America]	Noted.
7-58	7	0				I like the use of adjusted forcing instead of lists of hemi-demi-semi-indirect effects. [Daniel Murphy, United States of America]	Noted. Thanks.
7-59	7	0				One subtle need is be very clear on a consistent definition of DRE. First, I think some authors have used instantaneous values and some a 24-hr average. For example for page 7-32 line 13 I think Loeb and Manalo-Smith use daytime whereas Myhre et al. use 24-hr average. Second and more important: is the clear-sky DRE the 24-hr value on an average clear day or is it an annual average effect including the fraction of time which is clear? These are about a factor of 2 apart. The clear-day value is more intuitive but multiplying by the fraction of clear sky is important for comparing the clear-sky DRE to other forcings. Otherwise it looks anomalously large. [Daniel Murphy, United States of America]	Taken into acount. All cited DRE, RF and ERF estimates are 24-hr values.
7-60	7	0				I think you need to discuss circulation changes from differential heating due to aerosol gradients, especially absorbing aerosols and the Indian monsoon. Aerosols have also been proposed for why rainfall patterns in China appear to have changed. Both examples are a contentious literature, perhaps without definite conclusions. But hundreds of millions of people are affected, so you need to take it on some. [Daniel Murphy, United States of America]	Rejected. This discussion belongs to Chapter 14 on climate phenomena. A sentence has been added to section 7.6 to point to the discussion in Chapter 14: "The effect of processes discussed in this section on specific precipitation systems, such as the monsoon, the inter-tropical convergence zones, or tropical cyclones are presented in Chapter 14."
7-61	7	0				The whole chapter is highly improved since FOD! A few sections still need some work to be as quantitaive and concise as the general content. A few parts may also have somewhat too much textbook style. [Gunnar Myhre, Norway]	Noted. Thanks.
7-62	7	0				I appreciate that considerable effort has gone into organizing cloud and aersol ineractions into RFari, AFari, Rfaci, and Afaci. The point of this organization, though, is to aid understanding of the interactions themselves, while the very heavy emphasis placed on this terminology, and the frequent comparisions with "older" or "former" technology are more heavy-handed than is necessary. It is not clear that the chapter's exectuive summary needs to lead off with this, for example - the ideas might simply be used in the summary and defined in the text. This would be in keeping with the an assessment rather than a journal sythesis article. [Robert Pincus, United States of America]	Partly taken into account. The exec summary starts now with a more general statement, but it is necessary to introduce the terminology in the exec summary as well, or it would be difficult for the reader to understand the exec summary without reading the whole report. The chapter refers back to Figure 7.3 in key locations (of eg sections 7.4 and 7.5).
7-63	7	0				The organization of the chapter has changed significantly since the First Order Draft, and the reorangization has improved the chapter. [Robert Pincus, United States of America]	Noted. Thanks.
7-64	7	0				I saw the authors stating that they cannot cite further publications because of space constraints. I suggest checking whether all references cited are really necessary. For example, there are 7 references to Jacobson et al., but I doubt that all of them are necessary. [Ulrich Schumann, Germany]	Partly taken into account. The reviewer does not say which one(s) of the Jacobson reference is not necessary. A reference to Jacobson (2001) was removed.
7-65	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, United States of America]	Noted. Thanks.
7-66	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, United States of America]	Noted. Thanks.
7-67	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, United States of America]	Noted. The SOD devoted 16 pages to clouds and cloud feedbacks (7.2), 10 pages to aerosols (7.3), 10 pages to aerosol-cloud interactions (7.4), 6 pages to aerosol forcing estimates (7.5), 3 pages on

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							precipitation (7.6), and 4 pages on SRM (7.7). As sections 7.4, 7.5, 7.6 and 7.7 are relevant to both clouds and aerosols, there is a good balance between clouds and aerosols. This balance is maintained in the final draft.
7-68	7	1	1	63	1	acronyms and jargon: too widely used. Reduce and/or refer to explanations [Andrea Flossmann, France]	A list of acronyms and abbreviations will be provided as an Annex to the published Report (one list for the entire report, not chapter specific). All acronyms and abbreviations are now spelled out at the first usage and have been eliminated when possible.
7-69	7	1	1	63	1	overall comment: there are several inconsistencies between the chapters. The evolution of water vapor is not very clear and its influence on RFari and RFaci not well described. However, the water vapor will in fact determine clouds and aerosol evolution. The chapter is not at all adressing cloudiness. Is it varying? This should impact RFaci, right? Maybe also RFari, as particles swell? [Andrea Flossmann, France]	Rejected. Water vapour was discussed in sections 7.2.4.1 and 7.2.4.2. It is not clear what the reviewer means by saying "this chapter is not at all addressing cloudiness". Observed changes in cloudiness are discussed in Chapter 2 and there are several cross-references in the Chapter.
7-70	7	1	1	63	1	I did read only the chapter 7 and the TS. The TS is much better written, much clearer. [Andrea Flossmann, France]	Noted. Specific comments from this reviewer are addressed below.
7-71	7	1	1	63	4	This version is vastly better than the first draft in my opinion. There is a great deal of material here, much of it covering topics that were absent from AR4. This is excellent work. My suggestions fall mainly in two categories: (1) adding some material about what satellites contribute to aerosol remote sensing, especially the new contributions since the 2006 deadline for AR4, and (2) reflecting the uncertainties in climate modeling when geoengineering is discussed. (These uncertainties seem to me to be much better expressed in the aerosol-cloud interaction section, yet, if this material is taken seriously, given the potential risks it seems much more important to present the prediction uncertainties as they relate to geoengineering ideas.). In any case, my thoughts are included below. [Ralph Kahn, United States of America]	Thanks. Partly taken into account. How satellite contribute to aerosol remote sensing and quantification of aerosol forcings is already mentioned in several places in sections 7.3, 7.4 and 7.5. The SRM section has been modified to reflect better the uncertainties, including those coming from climate models.
7-72	7	1	1	139	1	I reviewed the aerosol-cloud interction part of Chapter 7. Overall, I think the second draft is very well-written, and has also been improved a lot compared to the first draft. [Minghuai Wang, United States of America]	Noted. Thanks.
7-73	7	1	1			Assessment Report Second Order Draft. Review Chapter 7 (formerly 6) Carbon and Biogeochemical Cycles IPCC WG1 – The Physical Science Basis The fact that other nutrients than nitrogen might act as main drivers of the global carbon cycling by limiting the primary production, is not attended enough. Without doubt, this is true for phosphorus in many aquatic environments (Wu et al. 2000, Blomqvist et al. 2004, Mather 2008), but applies significantly also to other elements, for instance, silica (Leynaerta et al. 2001, Brzezinski et al. 2011). This is a most essential aspect when dealing with the global biogeochemistry of carbon cycling and nutrient dynamics. Actually, to my opinion, this research field is so important that it deserves a thorough paragraph on its own, and I do recommend elaboration the present state-of-the-art. Not least when modeling the global carbon cycling, reliable knowledge on limiting nutrients is of great importance. Therefore, I strongly urge you to include this aspect in the Fifth Assessment Report of IPCC. References Blomqvist, S., Gunnars, A. & Elmgren, R., 2004. Why the limiting nutrient differs between temperate coastal seas and freshwater lakes: A matter of salt. Limnology and Oceanography 49: 2236-2241. Brzezinski, M.A., Baines, S.B., Balch, W.M., Beucher, C.P., Chai, F.; Dugdale, RC,.Krause, J., Landry, MR., Marchi, A., Measures, C.I., Nelson, D.M., Parker, A.E., Poulton, A.J., Selph, K.E., Strutton, P.G., Taylor, A.G. & Twining, B.S., 2011. Co-limitation of diatoms by iron and silicic acid in the equatorial Pacific. Deep-Sea	Rejected. This comment does not seem to belong to Chapter 7. It has been passed on to CLAs of Chapter 6.

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						Research II 58: 493-511 Leynaerta, A., Tréguera,P., Lancelot, C. & Rodierc, M., 2001. Silicon limitation of biogenic silica production in the Equatorial Pacific. Deep-Sea Res. 48: 639-660. Mather, R.L., Reynolds S.E., ,Wolff, G.A., Richard, G., Williams, R.G., Torres-Valdes, S., Woodward, E.M.S., Landolfi, A., Pan, X.I., Sanders, R. & Achterberg, E.P., 2008. Phosphorus cycling in the North and South Atlantic Ocean subtropical gyres. Nature Geoscience 1: 439-443. Wu, J.F., Sunda, W., Boyle, E.A. & Karl, D.M., 2000. Phosphate depletion in the western North Atlantic Ocean. Science 289: 759-762. [Sven Blomqvist, Sweden]	
7-74	7	1	12	1	12	Correct spelling of my given name is Anthony. [Anthony Del Genio, United States of America]	taken into account.
7-75	7	1	12	1	12	"Anthony" Del Genio (not Antony) [Robert Pincus, United States of America]	taken into acount.
7-76	7	1	14			Please could my name appear as 'Benjamin Laken' instead of 'Ben Laken' [Benjamin Laken, Spain]	taken into acount.
7-77	7	1	40	1	40	"Progresses" should be "Progress" [Peter Irvine, Germany]	Corrected.
7-78	7	1		139		Please decide in the entire text: aerosol or aerosols. This should be uniform throughout the text [Government of Poland]	Partly taken into account. We have added a footnote in Section 7.1 as to why we take "aerosols" to mean "aerosol particles". With that caveat it is appropriate to discuss about aerosol (generically as the aerosol system) and aerosols.
7-79	7	1		139		The chapter also assesses the new radiative forcing aspects such as adjusted forcing, and classifying them as dri and cri. These new concepts are well addressed. Please correct the typos and sentence structures in the chapter. [Ramachandran Srikanthan, India]	Noted. Typos and sentence structures corrected when flagged by the review comments.
7-80	7	1		200		14. This paragraph refers to the entire Chapter 7. Chapter 7 reviews some of the published information on the topic "Clouds and Aerosols". However, the motivation for the reviewed research effort and the logic behind it is more often fraudulent than not, as the respective research frequently follows the pseudo-scientific reasoning that "more corroborating evidence produces a stronger case for the AGW hypothesis". In fact, nothing can be further from the truth, as shown in my Paragraph 3. Indeed, no amount of corroborating evidence can prove a hypothesis, while a single piece of contradictory evidence is sufficient to reject a hypothesis. In effect, the only (dubiously) useful result of this research effort is the "general progress of science", resulting from wasteful usage of public money on climate studies, where no real problem requiring study may be found. Even the PhD degrees earned as a result of such research are of dubious (in the very least) value, as we are producing more pseudo-scientists certified as scientists, in addition to the already existing pseudo-scientists. Research based on the AGW hypothesis, known to be wrong, may provide no valid scientific results, as its conclusions are already known before the research even began - these conclusions being "AGW is happening, and we are to blame for it". Additionally, the data interpretation in the publications is frequently done based on the same climate models, which are demonstrably wrong (as shown in my Paragraphs 2 to 8), and therefore constitutes a fraud. [Igor Khmelinskii, Portugal]	Rejected. The comment is not specific enough to be further considered.
7-81	7	1				Please convey to all of the authors my pleasure with the entire chapter. Aside from some minor issues—mostly small, typographical errors, I find nothing wrong with what is said. The chapter does an excellent job of conveying current knowledge as reflected in the published literature, to the extent that I know it. It has expressed well-deserved skepticism concerning space-based observations of aerosols, clouds, and their interactions. It has correctly identified most of the progress in understanding during the past decade, again to the extent that I'm aware of it. It recognizes the weakest link in assessing climate change: the distribution of water vapor, its sources and sinks, and clouds. [James Coakley, United States of America]	Noted. Thanks.
7-82	7	1				The most annoying aspect of this chapter is the heavy reliance on abbreviations: AF, +CNV, POA, RFari, and so on. Most of these are defined the first time they are used, although I did find a few exceptions, usage	A list of acronyms and regional abbreviations will be provided as an Annex to the published Report (one list

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						defined after first use. ALL SHOULD BE DEFINED THE FIRST TIME THEY ARE USED. In addition, because some readers will jump to sections in the middle of the chapter, there should be a list of abbreviations used in the chapter, right at the beginning, perhaps following the table of contents for the chapter, so that readers can easily track down what is being said. Having read the chapter from start to finish, I can assure the authors that by the end of the chapter, I wasn't entirely sure that I correctly remembered what each of these abbreviations meant. [James Coakley, United States of America]	for the entire report, not chapter specific). All acronyms and abbreviations are now spelled out at the first usage.
7-83	7	1				The part on clouds is very interesting, both in terms of new observations and new model developments. I am however a little bit upset to see no illustration on the way those new model developments impact the representation of clouds in the models. This point is only very inderectly mentioned in chapter 9, which shows an evaluation of cloud radiative effect in CMIP3 models. There may not be enough work available in the literature so far, or may be only individual model evaluation (as in our team Hourdin, et al, 2012, LMDZ5B... already in the bibliography). I know at least 3 papers on multi-model comparisons with Calipso lidar cloud observations : Nam et al., 2012, GEOPHYSICAL RESEARCH LETTERS, VOL. 39, L21801, 7 PP., 2012 doi:10.1029/2012GL053421, Cezana and Chepfer, 2012, GEOPHYSICAL RESEARCH LETTERS, VOL. 39, L20803, 6 PP., 2012 doi:10.1029/2012GL053153, or a specific process oriented evaluation on the West African region by Roehrig et al., in revision for climate Dynamics, ftp://cnrm-ftp.meteo.fr/pub-moana/roehrig/CMIP5_WAM/rev1/Roehrig_etal2012_JClim_rev1.pdf). Even if it is to conclude that this point is not mature enough for the AR5, I feel that this discussion should deserve a stronger emphasize in the report either in ch 7 or in ch 9. [Frédéric HOURDIN, France]	Rejected. It is not appropriate to single out particular models in figures. We have concentrated on figures that bring a multi-model perspective. Note that model evaluation belongs to chapter 9.
7-84	7	1				The chapter is much improved over the FOD, and I am pleased to see you have taken a number of my suggestions on the FOD. [Stephen E Schwartz, United States of America]	Noted, thanks.
7-85	7	1				Throughout. Attention needs to be paid to specifying which aerosol forcing: TOA vs Sfc; Total aerosol vs anthro vs secular over some time period. All sky vs cloud-free sky; instantaneous vs 24 hr avg; [Stephen E Schwartz, United States of America]	Noted. Unless otherwise specified, forcing estimates are TOA, 24-hr averages.
7-86	7	1				Unfortunately, I do not have time to review the remainer of this chapter. [Gunilla Svensson, Sweden]	Noted.
7-87	7	1				As a whole: You have done a great job! Congratulations. [Erik Swietlicki, Sweden]	Noted, thanks.
7-88	7	2	7	2	7	Chapter 3 concludes with a Synthesis section for the whole chapter. Does this chapter not have a Synthesis section for the whole chapter--but just for section 7.7. This seems strange. [Michael MacCracken, United States of America]	Noted but no change is made. The chapter discusses various topics, which do not necessarily need to be brought back together. Section 7.5 brings 7.3 and 7.4 together. The title of section 7.7.4 now explicitly refers to SRM.
7-89	7	2	23			Obscure sentence staring with "a valid alternative..." [Jost Heintzenberg, Germany]	this comment must refer to page 3. The sentence has been removed from the ES.
7-90	7	2		62		This chapter is well written. My issues with inconsisten terminaology are corrected. [Larry Thomason, United States of America]	Noted, thanks.
7-91	7	3	0	6	0	Executive summary: Crisp, well-covered and quantified. [K KRISHNA MOORTHY, INDIA]	no action needed
7-92	7	3	1	7	6	Excellent executive summary. [Robert Kandel, France]	no action needed
7-93	7	3	1			ES: Although obvious to authors and many readers, I think it should be stated in the ES that RF and AF are given relative to pre-industrial levels (e.g. like ch8, page 3, lines 13-14) [Jan Fuglestvedt, Norway]	accepted. Now mentioned in a footnote.
7-94	7	3	3	3	3	ES : I would suggest not to start the Executive Summary with such a methodological statement, but by something more general (e.g. about the overall aim of the chapter). [Sandrine BONY, France]	agreed. 1st para graph now does this
7-95	7	3	3	3	6	Please specify is this sepration is done throughout AR5 or just for this chapter. [Andrew Ferrone, Germany]	framework is throughout report unless specified differently. This has to be clarified in chapter 1, rather than here.
7-96	7	3	3	3	9	I do not think it is the place of IPCC to introduce a new framework to replace "direct" and "indirect" forcing,	this new framework does come from the peer review

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						though as a whole I feel that the framework would be a good idea coming from the peer-reviewed community. I am especially concerned that this new framework will diminish the really substantial strides we have made in the scientific understanding of aerosol processes since the 4th assessment. Moreover, the balance between introducing new information and emphasizing the steady progress competes with the discussion of solar radiation management at the end of the chapter. Taken together, I fear that the net effect will be to enhance the attention paid to the SRM part and to dismiss the aerosol part as confusing. That would be a very bad outcome. [Neil Donahue, United States of America]	community. Text reworded for clarity. SRM is not as developed as aerosol science. This is clarified. The revised text adresses any misunderstanding
7-97	7	3	3	3	9	The framework obviously involves also "cooling influence " and "feedbacks". This should be introduced in the first paragraph to better prepare for the cloud conclusions [Michael Schulz, Norway]	This comment is unclear. Feedbacks are indeed mentioned here.
7-98	7	3	3			the executive summary starts with "a new framework is used which separates..". This is bizarre for a first sentence. Should be detailed what kind of framework. Presentation concept? Modeling concept? Analysis concept? [Andrea Flossmann, France]	Taken into account. The framework arises from the peer reviewed literature. The ES now starts with a more general paragraph and framework is discussed further below.
7-99	7	3	3			the number of studies is always "limited", admit how many "limited" means [Jost Heintzenberg, Germany]	does this comment refer to page 4? "limited" is somewhat subjective but carries the message that there is a low level of confidence.
7-100	7	3	3			It is suggested to qualify the framework, e.g. by wording such as: A new conceptual framework or A new framework for analysis is used, .. [Klaus Radunsky, Austria]	agreed, qualification added
7-101	7	3	3			I applaud the new framework (terminology) of forcing and adjusted forcing; perhaps better "distinguishes" than "separates". I see you use "distinction" and "distinguish" later in the para. I think that this is entirely parallel to the adjusted forcing phenomenon that has been recognized in the ghg forcing. [Stephen E Schwartz, United States of America]	agreed. Wording adopted
7-102	7	3	4	3	4	Line 4: First, the meaning of “fast” is unclear. It would appear to be a time scale and if so does it imply nanosecond, second, or minute? The definition of a time scale is critical and cannot be ambiguous in the discussion of aerosol-cloud interactions. Later, another time scale is introduced, namely “lifetime.” It must be defined as well in terms of model integration. [Kuo-Nan Liou, U.S.A.]	agreed. "Fast" no longer used, "rapid" is used instead and more discussion is provided in Section 7.1.
7-103	7	3	5	3	6	Lines 5-6: Second, I would argue that “surface temperature” is not entirely correct. This term should be changed to “surface and atmospheric temperatures.” [Kuo-Nan Liou, U.S.A.]	rejected. Concept here is general.
7-104	7	3	6	3	6	Is it best to stipulate that feedbacks are mediated by change in surface temperature, as opposed to temperatures at the surface and in the atmosphere? [Leo Donner, United States of America]	rejected. Surface temperature is meant here.
7-105	7	3	7	3	7	Even though the term aerosols is defined p. 7 line 19, it would be more correct to replace aerosols with aerosol particles at this early stage for the reader. This to be perfectly clear on which type of aerosol that is considered. [Caroline Leck, Sweden]	rejected. Clarification unnecessary here. A footnote was added in Section 7.1 on this issue.
7-106	7	3	7	3	8	The concepts need to be related to more traditional measures in the literature. Direct effects = forcing. Aerosol indirect effects? Semi direct effects? If not here than in sections. The executive summary is not understandable if the terms are not common. [Andrew Gettelman, United States of America]	The new terms explained in chapter 1, Section 7.1, figure 7.3 and in the appendix III (glossary). They are used throughout the report.
7-107	7	3	11	3	11	ES : At the beginning of the sentence or after -17 W/m2, I suggest to add : in the present climate. [Sandrine BONY, France]	This paragraph was deleted as not a key finding.
7-108	7	3	11	3	11	Please specify that the indicated forcing is valid for the present climate, as it might be different for different climatic states. [Andrew Ferrone, Germany]	This paragraph was deleted as not a key finding.
7-109	7	3	11	3	12	The cooling influence of clouds is given, but there is no mention of error bars (later it suggests that this value was accurate within 10%). Given the importance of clouds, please add up a confidence level and uncertainty estimate here. [Government of Australia]	This paragraph was deleted as not a key finding.
7-110	7	3	11	3	13	The net cooling effect is infact about 30% larger than -17W.m-2 It comes from the larger SW effect than is deduced from the usual clear minus all sky flux difference inferred from TOA fluxes. This difference fails to	This paragraph was deleted as not a key finding.

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						account for that portion of the surface reflection masked by clouds which overestimates the clear-sky reflection - this masking is about 8Wm-2. I have quantitfied this in a recent study that I have completed but not yet publsihed. This masking effect is also discussed in Soden et al (2008) although I dont think it is not explicitly quantified. [Graeme Stephens, United States of America]	
7-111	7	3	11	3	13	It is pedagogically correct to emphasize that clouds exert an average cooling influence on Earth of about −17 W m–2 (+30 and -47 m–2). This must be kept in mind when scrutinizing the relatively small – in comparison – aerosols radiative forcing effects. This number was rather well hidden in AR4. [Erik Swietlicki, Sweden]	Noted. This number is still in Section 7.2, but is no longer highlighted in the ES as it is not a key finding since AR4.
7-112	7	3	11	3	15	I wonder if it would be useful to include that clouds do not exert a "forcing". Otherwise the wording cooling influence comes as a surprise after the first paragraph. Also reference could be made to chapter 8. … or see my comment on first paragraph [Michael Schulz, Norway]	The cloud radiative effects are no longer highlighted in the ES as it is not a key finding since AR4. The fact that clouds do not exert a forcing in the IPCC sense is mentioned in the glossary.
7-113	7	3	11			Is 'influence' a forcing? Or an ' adjustment' in the framework? [Andrew Gettelman, United States of America]	Neither one nor the other. The cloud radiative effects are no longer highlighted in the ES as it is not a key finding since AR4. The fact that clouds do not exert a forcing in the IPCC sense is mentioned in the glossary.
7-114	7	3	11			It is suggested to add some language in order to clarify that this effect is is part of the natural greenhouse gas effect. The wording could read as follows: Clouds exert an average cooling influence on Earth, which is mainly natural, of about -17 W m-2. [Klaus Radunsky, Austria]	The cloud radiative effects are no longer highlighted in the ES as it is not a key finding since AR4. The fact that clouds do not exert a forcing in the IPCC sense is mentioned in the glossary.
7-115	7	3	12			inverse order of words; better "mainly due to" [Andrea Flossmann, France]	This paragraph was deleted as not a key finding.
7-116	7	3	13	3	13	Indeed it is not new even relative to FAR, as the Ramanathan et al. (Science 1989) values are almost identical to the ones quoted here – Table 3.1 of FAR lists these values, although naughtily it doesn't properly attribute them. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account. This paragraph was deleted as not a key finding.
7-117	7	3	13			replace "this results is not knew since.." with "this result is known.." [Andrea Flossmann, France]	This paragraph was deleted as not a key finding.
7-118	7	3	16			The Executive Summary is lacking a summary of what seems to be a fairly important concept - that of cloud parameterizations, the topic of section 7.2.3. [Government of United States of America]	Partly taken into account. The "first "cloud" paragraph of the ES mentions the fact that models disagree on the cloud feedbacks.
7-119	7	3	17	3	17	Please define "clear-sky" in the executive summary. [Government of Australia]	Partly taken into account. "clear-sky" is now deleted from the ES when discussing the water vapour feedback..
7-120	7	3	17	3	17	As with SPM, suggest that "clear-sky" be better explained in main findings of executive summary, as the meaning of this term is not known to non-experts. [Government of Canada]	Partly taken into account. "clear-sky" is now deleted from the ES when discussing the water vapour feedback..
7-121	7	3	17	3	17	Line 17: The statement "… feedback from water vapour and lapse rate changes together is very likely positive" is confusing. My understanding is that water vapor feedback is positive and lapse rate feedback is negative. But it is not clear under what specific conditions one eclipses the other. The words "very likely" appear to be meaningless in the context of these sentences. [Kuo-Nan Liou, U.S.A.]	The point is that these two feedbacks need to be looked at together rather than separately. See also the corresponding section for more discussion on the framework.
7-122	7	3	17	3	19	Why is the feedback only very likely positive when the range given is 0.91-1.27 indicating that the 1 percentile could well be > 0 ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The water vapour feedback is now deemed to be "extremely likely" positive in the traditional framework.
7-123	7	3	17	3	24	The confidence level of the combined water vapour/lapse rate feedback being positive should surely be higher than 'very likely'. The strength of the accumulated evidence would strongly indicate this, with multiple lines of evidence further reinforcing the (already very strong) evidence available at the time of the AR4. In addition the 'very likely' range of values 0.91 to 1.27W/m2/K has a lower bound much above zero, which would imply a positive value being virtually certain. [Government of Australia]	Taken into account. The water vapour feedback is now deemed to be "extremely likely" positive in the traditional framework.

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7-124	7	3	17	3	24	It is surprising that clear-sky feedbacks are covered in a chapter about clouds, since they apply to the opposite situation! I don't think the reader would expect to find them in this chapter. I feel it would be more logical to move this material to 9.7, where there is already some material on this subject (and no reference to ch7). [Jonathan Gregory, United Kingdom]	Rejected. Other chapters will point to text here. There is an obvious connection between water vapour and clouds.
7-125	7	3	17	3	24	It is unclear to me why one would assess the water vapor and lapse rate feedbacks in the chapter devoted to clouds and aerosols, especially by highlighting the conclusion, which is not new at all, in the executive summary. [Yi Ming, United States of America]	Rejected. Other chapters will point to text here. There is an obvious connection between water vapour and clouds.
7-126	7	3	17	3	24	The use of the phrase "clear-sky" could be confusing here. These are not the values when clouds are not present; I believe everywhere in this chapter uses the "all-sky" feedbacks in that they represent the changes to the all-sky fluxes, not the clear-sky fluxes. "clear-sky" is also confusing because climate models and satellites define this term different ways. "non-cloud" feedbacks is more accurate, though it is a less elegant term. For this particular line, you could just drop "clear-sky" from this sentence. You're not including the albedo feedbacks, which is another "non-cloud"/"clear-sky" feedback anyway. I suggest modifying this term in the rest of the chapter as well (or defining exactly what you mean by it). I've flagged some other occurances I've noticed. [Karen Shell, United States of America]	Partly taken into account. "clear-sky" is now deleted from the ES when discussing the water vapour feedback..
7-127	7	3	17			It is suggested to add some clarifying language as this paragraph addresses some climate change aspect: The net clear-sky feedback from additional water vapour evaporated into the atmosphere by a warmer climate and lapse rate … [Klaus Radunsky, Austria]	Partly taken into account. "clear-sky" is now deleted from the ES when discussing the water vapour feedback..
7-128	7	3	18	3	19	I don't understand how the conclusion can be that it is only very likely positive when the 90% limits given are very positive. For this feedback to be negative, there would have to be a very, very long tail that would contain a very small likelihood. Is not this feedback virtually certain to be poistive? Or even just say it is positive without qualification. [Michael MacCracken, United States of America]	Taken into account. The water vapour feedback is now deemed to be "extremely likely" positive in the traditional framework.
7-129	7	3	19			This line states: "...a very likely range...of 1.09" when it intends to state that 1.09 is the best guess or central estimate, with possible values ranging from 0.91 to 1.27. The authors should consider revising the text accordingly. [Government of United States of America]	Taken into account. Reworded.
7-130	7	3	21	3	24	this sentence is incomprehensible [Andrea Flossmann, France]	Taken into account. This sentence has been deleted from the ES.
7-131	7	3	21	3	24	Do you mean that a strong positive feedback is 1.27 Wm2K-1 and a weak net feedback is 0.91 Wm2-K-1 ? Could you clarify that the range in feedback parameter is also "used" in the analysis frameworks referred to in this sentence? [Michael Schulz, Norway]	Taken into account. The confusing sentence has been deleted from the ES.
7-132	7	3	26			The sentence that the feedback due to all cloud types is positive seems in contradiction to the statement above line 11 to 15 that the cloud effect is cooling. Maybe explain better. You probabaly want to stress that the amount of negative forcing is likely decreasing with time. [Andrea Flossmann, France]	This is now clarified, and we are more careful in distinguishing present-day effects from climate feedbacks.
7-133	7	3	26			It is suggested to add some clarifying language as this paragraph addresses some climate change aspect: The net radiative feedback of the additional water vapour that is present in various cloud types due to all those cloud types is likely … [Klaus Radunsky, Austria]	Rejected. This sentence is about cloud feedbacks, rather than water vapour feeback. The cloud feedback is due to changes in the distribution and properties of clouds, rather than water vapour.
7-134	7	3	28			How would one define a 'plausible range for unknown contributions by processes yet to be accounted for' ? This is confusing and potentially misleading wording, especially in the Executive summary. The authors should consider revising the text accordingly. [Government of United States of America]	Taken into account. The sentence has been reworded and does no longer refer to a "plausible range".
7-135	7	3	29			"considering a plausible range for unknown contributions by processes yet to be accounted for": "unknown" suggests no knowledge, in which case it is difficult to have a plausible range. Maybe "uncertain" contributions? [Karen Shell, United States of America]	Taken into account. The sentence has been reworded and does no longer refer to a "plausible range".
7-136	7	3	37	3	37	I do not believe this 20 mWm-2 is defensible. I will return to this in section 7.2. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Noted, see reply to corresponding comments in section 7.2.
7-137	7	3	37	3	40	Here, negative forcing due to aerosol emission by shipping should be added together with contrails from	Rejected. Shipping aerosol emissions are considered

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						aviation. [HUA ZHANG, China]	as part of the aerosol forcing estimates. There is no special reason to treat them separately in this chapter. Contrails are treated separately because they are responsible for a forcing mechanism that is distinct from other forcing mechanisms which are dealt with in this chapter.
7-138	7	3	39	3	39	Line 39: Again, the meaning of "very" in the context of this sentence is not clear and definitive. Additionally, conclusions stated here are not in line with the discussions presented in section 7.2.5, which covers the work derived from global models. For example, it was stated in Lines 29-31 that "Raps et al. (2010b) confirmed the assessment that aviation contrails are "very" unlikely, at current levels of coverage, to have an observable effect on surface temperature or diurnal temperature range." However, Raps et al. (2010b) used a global model (the UK Met Office climate model) and the results presented did not qualify for the discussion of "regional effects." Moreover, taking a single model run and making prejudicial speculations without firm scientific ground appear to be not in harmony with your excellent report and from the perspective of a constructive scientific spirit. [Kuo-Nan Liou, U.S.A.]	Taken into account. We have removed the calibrated likelihood language which was not adapted here. We replace this by a confidence level statement and rely on several studies. It is unclear however why the reviewer thinks that global climate models do not qualify to discuss regional effects. Note that Raps et al (2010b) have multiplied the contrail RF by a factor of 100 to investigate the regional climate response. It confirms previous results obtained by Ponater et al. (2005) and strenghtens the discussion made in the AR4 on this subject.
7-139	7	3	42	3	48	48 "Cloud-processed particles" need to be explicitly included. There are number of studies (Hoppel et al., 1994; Krämer et al., 2000; Jeong and Li, 2010) discussed the importance of cloud-processed particles, which normally have larger sizes than newly formed particles. Detailed discussion need to be added in section 7.3.2.2 in Page 27, Line34[References: a) Hoppel, W. A., G. M. Frick, J. W. Fitzgerald, and R. E. Larson (1994), Marine boundary layer measurements of new particle formation and the effects nonprecipitating clouds have on aerosol size distribution, J. Geophys. Res., 99(D7), 14,443–14,459, doi:10.1029/94JD00797. b) Krämer, M., N. Beltz, D. Schell, L. Schütz, C. Sprengard-Eichel, and S. Wurzler (2000), Cloud processing of continental aerosol particles: Experimental investigations for different drop sizes, J. Geophys. Res., 105(D9), 11,739–11,752, doi:10.1029/1999JD901061. c) 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]	rejected, too complex for this exec bullet
7-140	7	3	46	3	47	"sufficient accuracy" to do what ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text now reworded
7-141	7	3	47			"sufficient accuracy". This needs to be quantitatively specified. [Stephen E Schwartz, United States of America]	Taken into account. Text now reworded
7-142	7	3	50			Mention these are semi-direct effects. [Andrew Gettelman, United States of America]	partly taken into account. The sentence was modified but semi-direct effects are not mentioned in the ES. The new terminology is further explained in Section 7.1 and glossary
7-143	7	3	51	3	53	"Observations and detailed large eddy simulations show cloud cover decreases with absorbing aerosol embedded in the cloud layer, and increases when aerosols are above cloud." For clarity, I would suggest "...decreases with increasing/decreasing concentration of absorbing aerosol..." and "...increases when aerosols are present above cloud level." [Eimear Dunne, Finland]	partly taken into account. Text now shortened.
7-144	7	3	53	3	53	It remains unclear here if it is also only absorbing aerosols or all aerosols above clouds to increase the cloud cover. Please specify. [Ottmar Möhler, Germany]	partly taken into account. Text now shortened.
7-145	7	3		3		In the bold portions of the bulleted points, I counted 9 occurences of "clouds" or "contrails", and 18 occurrences of "aerosols". As previous IPCC reports have concluded, and I believe the current AR5 draft continues to assert, cloud feedbacks and their associated processes are the largest source of uncertainty in future projections of climate. Why then does it appear that aerosols are twice as important (from a word count perspective)? [Brian Kahn, United States of America]	Noted. The SOD devoted 16 pages to clouds and cloud feedbacks (7.2), 10 pages to aerosols (7.3), 10 pages to aerosol-cloud interactions (7.4), 6 pages to aerosol forcing estimates (7.5), 3 pages on precipitation (7.6), and 4 pages on SRM (7.7). As sections 7.4, 7.5, 7.6 and 7.7 are relevant to both clouds and aerosols, there is a good balance between clouds and aerosols. This balance is maintained in the

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							final draft.
7-146	7	3		3		I have further comments about this below. I think there is the appearance of too much emphasis on how aerosols modulate cloud structure and behavior rather than understanding how clouds work without aerosol-cloud interactions folded in so explicitly. There is plenty of work left to be done regarding clouds and associated atmospheric processes that have nothing to do with aerosols. [Brian Kahn, United States of America]	Noted. This is what Section 7.2 is about. Note that IPCC does not make recommendations for future research.
7-147	7	3		6		ES : As a whole, there is quite of an imbalance in the Executive Summary between the number of statements and paragraphs about aerosol influences or aerosol radiative forcings and the number of statements about the other issues addressed in the chapter. I would suggest to present cloud and aerosol issues with a similar level of synthesis and of conciseness or details (e.g. if RF estimates are given for absorbing aerosols on snow and ice...why not give cloud feedback and adjustment estimates to greenhouse gases?). A suggestion would be to summarize the assessments of the different aerosol RF and AF estimates in a Table including some of the comments currently written in the text, and mentionning the relevant chapter sections where more information can be found. Regarding future climate change (not the 20th century), I strongly recommend to assess the relative roles, contributions or importance of aerosol effects vs non-aerosol effects in cloud feedback uncertainties, as there is often some misunderstanding on this issue. [Sandrine BONY, France]	Partly taken into account. The ES now reflects the balance of the sections with 1, 3, 2, 1, 2, 1 and 2 paragraphs for sections 7.1 to 7.7. The ES has to follow a certain format, that does not allow to have tables. Regarding the relative roles of aerosol and non-aerosol effects in the future climate, this cannot be mentioned in the ES as not a subject for this chapter (see Chapter 12 instead).
7-148	7	3		6		In some places in the executive summary the likelihood statements are defined, eg p.3 line 18 very likely (90%) and p.3 line 26 Likely (>66% chance). Firstly it does not appear necessary to define these terms when they are used throughout the report without being defined each time. However, if they are to be defined it should be done with the exact language from Chapter 1, eg Very Likely (90−100% probability) Likely (66−100% probability). [Government of Australia]	rejected. These are not definitions of the likelihood statements, but clarifications of what they mean in the particular instance in question. The ranges given in the IPCC definitions are the ranges for which that term is appropriate, that is, anytihng with a probability from 90% to 100% could be stated to be "very likely", but the actual assessed probability could vary (within these limits) from one use of the term to another.
7-149	7	3		6		General comment: I would like to commend the authors for writing a very good draft for Chapter 7, and overall an excellent draft for all of AR5. This is a huge task as you know, and I appreciate your efforts immensely. This first point I want to make is more of a perception as a reader rather than a technical comment, but I got the impression that aerosols are a larger priority than clouds because they are mentioned more times than clouds in the summary points from p. 3-6. [Brian Kahn, United States of America]	Noted. The SOD devoted 16 pages to clouds and cloud feedbacks (7.2), 10 pages to aerosols (7.3), 10 pages to aerosol-cloud interactions (7.4), 6 pages to aerosol forcing estimates (7.5), 3 pages on precipitation (7.6), and 4 pages on SRM (7.7). As sections 7.4, 7.5, 7.6 and 7.7 are relevant to both clouds and aerosols, there is a good balance between clouds and aerosols. This balance is maintained in the final draft. The ES now reflects the balance of the sections with 1, 3, 2, 1, 2, 1 and 2 paragraphs for sections 7.1 to 7.7.
7-150	7	3				The format of the Executive summary varies significantly from chapter to chapter. In some chapters, like chapter 7, there is little discussion. The presentation is more of a list of parameters. It would be helpful to have some discussion that provides an integrated, high-level view of the chapter. [Government of United States of America]	Taken into account. The chapter is now introduced in the first couple of sentences.
7-151	7	3				The Executive Summary is difficult to digest initially due to all the unfamiliar jargon. Maybe it would be better to put this at the end of the Chapter, so those who read the entire Chapter will have become more familiar with the terminology before they read the Summary. In contrast, for example, the Executive Summary for Chapter 2 was excellent- included all the main points with just the right level of detail. [Government of United States of America]	Partly taken into account. We have tried to reduce jargon. However Chapter 7 is a technical process-based chapter so some jargon is inevitable.
7-152	7	4	5	4	5	Line 5: Abbreviations DMS-CCN should be spelled out. [Kuo-Nan Liou, U.S.A.]	Accepted. All acronyms are now defined.
7-153	7	4	5	4	6	DMS-CCN-cloud' and 'DMS' should be spelt out. [Government of Australia]	Accepted. All acronyms are now defined.
7-154	7	4	5	4	6	“There is medium confidence for a weak DMS-CCN-cloud albedo feedback due to a weak sensitivity of CCN population to changes in DMS emissions”. This sentence seems to belong to the next paragraph which	Taken into account. The text is now rearranged into a feedback heading.

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						discusses aerosol influence on clouds. [Lazaros Oreopoulos, United States of America]	
7-155	7	4	5			DMS & CNN should be defined here. [Government of United States of America]	Accepted. All acronyms are now defined.
7-156	7	4	5			Define DMS and CCN. [Reto Knutti, Switzerland]	Accepted. All acronyms are now defined.
7-157	7	4	7			Do you mean "regional CLIMATE effects OF the aerosol may be important." ? [Michael Schulz, Norway]	Taken into account. Now deleted
7-158	7	4	9	4	14	Obscure para. the reader does not understand what the "two types" are referring to [Jost Heintzenberg, Germany]	Taken into account. Paragraph now deleted and merged
7-159	7	4	9	4	14	Add at the end of the paragraph that we also do not have a good understanding of aerosol influences on the microphysical properties of ice clouds. [Lazaros Oreopoulos, United States of America]	this paragraph has been reworded. New paragraph says that aerosol-cloud interactions are uncertain and problematic to model.
7-160	7	4	11	4	11	"smaller net effects" than what ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Paragraph is now deleted and merged.
7-161	7	4	12			and throughout. This is a minority view, but I think it will ultimately prevail. I am increasingly uneasy about "cloud fraction". The quantity reported as cloud fraction is strongly dependent on observation method and threshold (Stevens and Schwartz 12). If the quantity cannot be measured, it cannot be modeled and compared with observation. Eliminating cloud fraction from models (and measurements) will be an uphill task; it has to be replaced with something else (I would prefer transmittance or reflectance, either of which is a continuous variable, instead of some average of 1's and 0's). I expect this conclusion is fairly premature, but you might mention that the concept and its utility have been questioned with a reference to the paper. Stevens B. and Schwartz S. E.: Observing and Modeling Earth's Energy Flows. Surveys Geophys. 33 779-816 (2012). DOI 10.1007/s10712-012-9184-0 Figure 11. [Stephen E Schwartz, United States of America]	Noted. Cloud fraction no longer used here. Maybe something for AR6!
7-162	7	4	18			omit "but"? [Michael Schulz, Norway]	Paragraph is now deleted and merged.
7-163	7	4	18			"multiple feedbacks". Not approp here; not dependent on GMST; better "multiple types of interactions" or the like. [Stephen E Schwartz, United States of America]	Taken into account. "feedbacks" is no longer mentioned in this respect.
7-164	7	4	23	4	28	What does this specific uncertainty imply for the overall robustness of model results? Does this continue to be an overwhelming contributor to uncertainty of climate projections? [Jochen Harnisch, Germany]	Partly taken into account as the uncertainty in ERFari+aci is discussed. This has not necessarily some impact on future projections as explained in Section 7.1.
7-165	7	4	23	4	28	Here is an example where I think multiple issues are convoluted. I agree that aerosols and clouds have structures that are much smaller than what a typical GCM can resolve (perhaps even LES fails for some of these problems). And it is true that aerosol-cloud interactions occur essentially at the smallest of scales, say, the size of an individual hydrometeor. The small scale structure in clouds is not necessarily onlt a result of small scale aerosol structure. What about the proabaility distribution functions of temperature? Water vapor? Wind speed and direction? Wind shear? Stratification? Etc. [Brian Kahn, United States of America]	taken into account, paragraph now altered
7-166	7	4	23	4	28	All of these non-aerosol physical quantities may be much more important than the role aerosols play. I am sure the authors know this much better than I, but I just want to stress that it looks like these things are getting confused and blended together uncecessarily in the text. [Brian Kahn, United States of America]	taken into account, paragraph now altered
7-167	7	4	34	4	34	May be better to say “related to solar cycle changes” rather than “changes in cosmic rays” – it seems of more importance whether there is a signal related to the solar cycle, rather than the precise cause (and it would be hard to separate out a cosmic ray cause from, say, a UV cause). The section in the chapter is clear on this point. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Rejected. Although section 7.4 is clear on this, we use "cosmic ray" here because this is what the studies have postulated.It would take us much further to discuss the impact of solar cycle changes on cloudiness.
7-168	7	4	35	4	35	Please replace "cloudiness have" by "cloudiness has". [Government of Australia]	accepted
7-169	7	4	38	5	28	ES : The period/climate associated with these RF assessments should be clarified (20C?). Idem for Table 7.1 [Sandrine BONY, France]	accepted, now done in a footnote.

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7-170	7	4	38			Direct effects of aerosols? [Andrew Gettelman, United States of America]	yes, as per new terminology introduced in the ES and section 7.1.
7-171	7	4	38			Notation: The notation RFari and the like is very awkward. I suggest a subscript: RF_ari, etc. [Stephen E Schwartz, United States of America]	Rejected. A subscript makes ari difficult to distinguish from aci.
7-172	7	4	43	4	48	This table seems to mainly relate to anthropogenic aerosols, correct? In any case, the heading and accompanying text should be very clear in what is considered by table and what is excluded why. [Jochen Harnisch, Germany]	Taken into account. Table no longer used. The text is now more explicit.
7-173	7	4	43	4	48	Are the other entries (besides dust) in Table 7.1, e.g., SOA, intended to represent purely anthropogenic forcings? [Larry Horowitz, United States of America]	Taken into account. Table no longer used. The text is now more explicit.
7-174	7	4	43	5	28	and anywhere throughout the report + the glossary (which I am not allowed to comment on directly) concerning the terms radiative forcing, adjusted forcing, rapid adjustement, and rapid response: There are no clearly defined concepts connected with the terms "rapid..." in this report. The range of "rapid ..." that now is allowed is arbitrary and seems to vary greatly from stratospheric temperature response to aerosol induced atmospheric heating. The only definition that I would find acceptable would have to make unambiguous quantitative statements about the range of time constants of the processes that are termed "rapid...". [Jost Heintzenberg, Germany]	Taken into account. The new terminology and framework are now discussed in Section 7.1 and 8.1. This includes a discussion of timescales.
7-175	7	4	45	4	45	Line 45 and the last line in Table 7.1: Do the dust RFari values include longwave radiation effects? If not, should be so stated. [Kuo-Nan Liou, U.S.A.]	Yes, but too much detail for here.
7-176	7	4	46			Would it be worth to state that large uncertainty is in Asian, African and South American contributions due to missing data on concentrations, aerosol absorption and sources? [Michael Schulz, Norway]	Rejected. This is too much detail for the ES.
7-177	7	4	46			It is essential to distinguish whether the forcings indicated are from preindustrial to present or some other definition such as anthropogenic or aerosol vs aerosol-free atmosphere; this issue is raised by the qualifier on dust, which states not necessarily anthropogenic. [Stephen E Schwartz, United States of America]	Accepted. A footnote was added.
7-178	7	4	48	4	48	Table 7.1 This needs consistency checking with Table 8.5 in Ch 8 (and radiative forcing figures in Ch 8). Some of the values are different and the uncertainty bounds must have been derived differently. Are the values in Table 7.1 evaluated over the same time period as those in Table 8.5? Is there any way to bring consistency across these two chapters to keep the messaging consistent? [European Union]	Taken into account. The table is no longer used, but consistency is fully established with chapter 8.
7-179	7	4	48			Why are contrails not included in this table? [Jochen Harnisch, Germany]	Taken into account. Table no longer used. Contrails are discussed in a separate paragraph.
7-180	7	4	48			It seems essential that the summation of the table entries be given. And uncertainties. I get for the summation -0.46 W m-2 and uncertainty range -0.03 to - 0.89 W m-2, (i.e., -0.46 ± 0.43 W m-2)) where the uncertainty assumes independence in the uncertainties of the several terms (addition in quadrature); this might not be correct if, for example, the removal terms of the several substances are the same; such correlations would increase the uncertainty over that obtained assuming the quantities are independent. The magnitudes of the sum and the uncertainty are both larger than stated at lines 38-39 (-0.4 ± 0.3) W m-2. The reasons for this need to be discussed. [Stephen E Schwartz, United States of America]	Taken into account. The table no longer used but sum is not exact. This is discussed in 7.5.1.
7-181	7	4	48			Executive summary should not contain tables, please remove. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Table no longer used.
7-182	7	4				Table 7.1 should be consistent with table 8.5 [Government of United States of America]	Taken into account. The table no longer used but sum is not exact. This is discussed in 7.5.1.
7-183	7	4				Table 7.1 seems to misrepresent estimates that have been made of RFari, and the inappropriate use of symmetric uncertainty bands may occur in other places and in other chapters. [Government of United States of America]	Taken into account. The table no longer used but sum is not exact. This is discussed in 7.5.1.
7-184	7	4				Table 7.1 containing Radiative forcing due to aerosol-radiation interaction, RFari. A central value and range are presented for 7 aerosol constituents. In each case, the central value is at the mid-point of the range. Does this derive from some general methodology on how uncertainties are to be	Rejected. There is no reason for RFari to be positive or negative, so symmetric uncertainties are justified. Note that we adopt asymmetric uncertainty ranges for

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						presented? An obvious difficulty arises for a quantity that that must be positive (or negative) but has a large uncertainty such that a representation by a symmetric function (i.e. Gaussian) gives results of the opposite, unphysical sign. For example, an uncertainty range for aerosol number concentration would never have a negative lower bound. There are many ways to insure positive results. If results from multiple studies were fitted to a function it could be a log normal. The particular problem in Table 7.1 is that Secondary organic aerosol (SOA) has an RFari central value of -0.08 W m-2 and a range of -0.28 to +0.12 W m-2. SOA in the form of brown carbon can absorb shortwave radiation but the absorption is in the blue end of the spectrum where radiative flux is low. Brown carbon is mentioned in the text (page 7-24 and 7-29) but only to say that it is poorly quantified. All discussions of SOA absorption that I'm aware of have placed it well below black carbon in W/m2 units. The absorption from black carbon can be enhanced by a lensing effect due to coating by SOA, sulfate, nitrate, and other aerosol constituents. This effect is poorly quantified and in any event should be included under Black carbon and biomass burning. The text correctly notes that reflectance depends on many variables, but with any degree of averaging over different conditions, non absorbing aerosol has a negative radiative forcing. My inference is that Table 7.1 misrepresents estimates that have been made of RFari, and that the inappropriate use of symmetric uncertainty bands may occur in other places and in other chapters. [Government of United States of America]	ERFari+aci.
7-185	7	4				Table 7.1: Is the "Primary organic matter" entry intended to represent the source from fossil fuel and biofuel (as for black carbon), but not biomass burning. If so, this needs to be stated explicitly. [Larry Horowitz, United States of America]	Taken into account. This has been clarified.
7-186	7	5	1	5	28	General comments: It appears to be a good idea to use the new framework which differentiates forcing, rapid adjustment (adjusted forcing), and feedback. For aerosols, the report introduces a number of new terminologies: aerosol-radiation interactions (ari), aerosol-cloud interactions (aci), and the associated terms RFari (aerosol direct effect), AFari (RFari + semi-direct effects), RFaci (cloud albedo effect or 1st indirect effect), and AFaci (cloud albedo effect (1st indirect effect) + lifetime effects (perhaps these can be called the 2nd indirect effect, which has been used in the past). However, I have fundamental difficulty in comprehending the rationale for creating so many new terms and complicated notations in the current IPCC report. [Kuo-Nan Liou, U.S.A.]	Taken into account. The rationale for the new terminology is explained in section 7.1, and stems from the literature.
7-187	7	5	1	5	28	(a) There is a question of time scale introduced which has not been defined in the scientific context: Fast, rapid, and lifetime. [Kuo-Nan Liou, U.S.A.]	Taken into account. The new terminology and framework are now discussed in Section 7.1 and 8.1. This includes a discussion of timescales.
7-188	7	5	1	5	28	(b) AFari, RFaci, and AFaci are all associated with changes in the cloud fields produced by aerosol effects. I submit that it would be very difficult, if not impossible, to distinctly separate RFaci and AFaci or AFari and AFaci from the framework of GCM and climate model output because of nonlinearity incurring in these processes. [Kuo-Nan Liou, U.S.A.]	Noted. ERFari and ERFaci are relatively easy to distinguish in models. We mention the difficulty in isolating and calculating Rfaci from ERFaci.
7-189	7	5	1	5	28	(c) AFaci is estimated from the residual between AFari+aci (this term has not been defined!) and AFari, but without much physical substantiation and explanation of the assumptions used in the analysis. [Kuo-Nan Liou, U.S.A.]	Taken into account. The text now mentions the assumption of linearity and the difficulties in doing so. For that reason ERFaci is no longer highlighted in the ES.
7-190	7	5	1	5	28	Finally, at the risk of being critical, I would submit that creating new concepts and introducing intricate terms to understand aerosol-cloud interactions would carry a danger of misrepresentation and might not be advantageous to the IPCC efforts within the context of serving the climate and atmospheric sciences communities as well as policymakers. [Kuo-Nan Liou, U.S.A.]	Noted but concepts are only new to IPCC report, they are firmly grounded in literature as noted in Section 7.1 and elsewhere in the chapter. Only the naming convention is new.
7-191	7	5	2			Semi direct effects? [Andrew Gettelman, United States of America]	Yes. The terminology is now further clarified in Section 7.1, Chapter 8 box and glossary.
7-192	7	5	4	5	5	Lines 4-5: "poorly" appears not to be the best choice of words in this context. I would say that "… these effects are multiple and not well-represented in climate models on the basis of the first principle …" [Kuo-Nan Liou, U.S.A.]	Accepted. The paragraph has been reworded.
7-193	7	5	4	5	8	I believe that the signifcant impact of SRM on the hydrological cycle has to be emphasized. Under SRM not	This comment refers to page 6. Accepted. The new

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						all regions will see "reduced climate differences compared to a world with the same GHG concentrations". There is no publication showing that SRM can be designed such that precipitation changes are small throughout the world. Recent work in GEOMIP and IMPLICC rather suggests that some aspects of the hydrological changes might be as big as in a GHG only scenario (eg Schmidt et al., 2012). [Michael Schulz, Norway]	paragraphs on SRM in the ES have clarified this.
7-194	7	5	6	5	6	"cloud drop inclusions". Not clear what this means. [Lazaros Oreopoulos, United States of America]	Taken into account. The term is no longer used in the ES.
7-195	7	5	9	5	13	The RF from absorbing aerosol on snow and ice is actually a very small fraction of total estimated aerosol RF. It is also evident that the overall concentration of principal absorbing aerosol (black carbon) has decreased on the surface of arctic region over the last two decades out to 1990, and the RF due to black carbon on snow has very likely weakened since 1980 {8.3.4.4}. Moreover, there continues to be insufficient observational evidences from multi-lines to support this effect to have robust and significant climatic influence. It is suggested to continue to assess this small and weakening effect in the chapter text, but removing this bullet from executive summary in Chapter 7 and Chapter 8 to make room to highlight those with more important result of a consensus. [Government of China]	Rejected. The term is discussed in the ES for completeness
7-196	7	5	9	5	13	The confidence level of '2-4 times" should be added here in case of misleading too. The sentence of "but that result is not well under stood, nor effectively constrained by observations, confidence in it is low." is suggested to be added at the end of this paragraph. Here, "the region covered by snow/ice" should take place of "the Arctic and over Tibet" due to the limited observation data in these areas and large uncertainties existed in the modelling studies. [HUA ZHANG, China]	Partly taken into account. The sentence for the 2 to 4 efficacy has been reworded, but no confidence level is indicated. Specific regions are not mentioned.
7-197	7	5	11			The sentence starting with "This RF..." has is unclear; it's not clear what it is a factor of 2-4 larger than. Perhaps a clearing phrasing would be: "The global mean surface temperature change per unit forcing from absorbing aerosol on snow and ice is 2-4 times larger than X..." and X needs to be clearly defined. [Government of United States of America]	Accepted. Agreed and reworded.
7-198	7	5	15	5	21	Levy et al. (2012, J. Geophys. Res., in revision) report AFari+aci of -1.8 W m-2 for GFDL CM3, outside assessed range. [Leo Donner, United States of America]	Taken into account. The new range is -1.9 to -0.1 Wm-2. Note however that this is a 90% uncertainty range so estimates outside the range are possible. We also make it clear that we use expert judgement so model estimates can be outside the range for that reason as well.
7-199	7	5	15	5	21	Estimates of AFari+aci appear to depend moderately on the method of calculation or the details of definition. For example, Shindell et al. (Atmos. Chem. Phys. Disc.) report values of -1.4 and -1.1 W m-2 for GFDL AM3 and NCAR CAM5.1, respectively. Levy et al. (2012, J. Geophys. Res., in revision) report -1.8 W m-2 for GFDL AM3, and Ghan et al. (2012, J. Climate) report -1.5 W m-2 for NCAR CAM5.1. [Leo Donner, United States of America]	Taken into account. Estimates have been checked and corrected. Table 7.5 refers to CMIP5 data in fixed SST experiments.
7-200	7	5	15		16	I am surprised that the uncertainty in aerosol forcing is so much less than AR4; the comparison should be noted and the basis for the reduction given. [Stephen E Schwartz, United States of America]	Accepted. The comparison is given in the ES and the reasons for it are explained in section 7.5.2.
7-201	7	5	15			There is a potential inconsistency between the estimated total forcing, climate sensitivity and the observed ocean and surface warming. A large part of this is related to the aerosol total forcing estimated to be much smaller than in AR4. If all the forcing numbers are taken at face value, the observed warming implies a climate sensitivity of 2K or less, which is inconsistent with CMIP5, and which would imply much smaller warming in the future. This potential inconsistency needs to be checked, and if real its implications need to be discussed in one of the chapters. [Reto Knutti, Switzerland]	Noted. RF estimates in Chapter 7 are from bottom up calculations and expert judgement only. The consistency or lack of with estimates of surface warming and climate sensitivity is discussed elsewhere in the chapter.
7-202	7	5	23	5	26	These should probably be related to the direct and indirect effects that are common in the literature. I think you mean these to be the indirect effects. [Andrew Gettelman, United States of America]	This is correct as per the new terminology explained in the ES, Section 7.1 and Figure 7.3.
7-203	7	5	23			This is not understandable in the executive summary first you state AFari+ AFaci, then you state. RFaci + AFaci. [Andrew Gettelman, United States of America]	taken into account. The focus is now on ERF, first ERFari and then ERFari+aci. ERFaci is not highlighted anymore in the ES.

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7-204	7	5	24			I cannot even tell if these are supposed to be additive? So is the 'indirect' forcing commonly reported in models then -0.7 wm-2? [Andrew Gettelman, United States of America]	Taken into account. Additivity is discussed in Section 7.5.3 but this is too much detail for the ES.
7-205	7	5	26			Should probably highlight what the observations are (energy budget calculations) [Andrew Gettelman, United States of America]	Taken into account. Satellite estimates are discussed in Section 7.5.2.
7-206	7	5	30	5	32	While I understand that it matters how much energy reaches the surface, is it not also dependent on how much is trapped in the atmosphere--that is, it woul dseem to me it relates to the relative change absorbed at the surface relative to the atmosphere, especially the amount absorbed in the upper troposphere. So, the CO2 increase tends to slightly stabilize the atmosphere, contributing to the need for precipitation to become more intense in order to overcome the tendency to stabilization. In any case, the phrasing here seems a bit one-sided. [Michael MacCracken, United States of America]	Taken into account. The paragraph was reworded. This is discussed in Section 7.6.
7-207	7	5	30	5	42	I would not expect precipitation changes to be covered in this chapter; although precipitation is related to clouds, it's a different subject. Changes in precipitation are dealt with in 12.4.5, and I think it would be logical to move this material to that section. [Jonathan Gregory, United Kingdom]	Rejected. There is an obvious link between clouds and precipitation.
7-208	7	5	30	7	32	I agree that multiple lines of evidence suggest that global mean precipitation increases in a warming climate. However the implication of the sentence is also that multiple lines of evidence suggest 'the rate of increase is limited by the increase in radiant energy absorbed by the surface rather than by the more rapid increase in atmospheric water vapour'. I do not think that there is a consensus on this latter point. Indeed, Chapter 12 (Page 25, starting Line 32) cites numerous studies over the years which argue that future changes in global precipitation are constrained by the latent heat release due condensation in the atmosphere, which is primarily constrained by the radiative energy balance of the atmosphere, which in turn depends not only on the surface radiative fluxes but also those at the top of the atmosphere. If the top-of atmosphere flux does not change (e.g. at equilibrium) the two views are equivalent. But during a transient warming, the climate system is not in equilibrium at the top of the atmosphere, and different atmospheric constituents have different relative impacts on the atmospheric and surface heating. So these two views are not generally equivalent. Physically speaking, as Mitchell et al 1987 and subsequent papers explain, an increase in the radiative cooling of the atmosphere will result in changes in convection and boundary layer humidity whereby a new balance is reached with increases in latent heat release in the atmosphere, precipitation and surface evaporation. Changes in surface sensible heat flux also contribute, but to a much smaller degree. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted. The revised bullet clearly states that the atmospheric radiative cooilng is what proximately controls the global hydrological cycle.
7-209	7	5	31	5	31	“limited by the increase of radiant energy absorbed by the surface” This statement neglects the well-known factor of the changing partition between latent and sensible surface fluxes that affects global evaporation/precipitation (e.g., Boer 1993). [Timothy Merlis, United States of America]	Taken into account. The revised phrasing addresses this point by noting that changes in precip are constrained by changes in the energy budget, rather than "limited".
7-210	7	5	37	5	37	It seems to me that this treating the shifts in dynamics as being sort of secondary importance, being a final phrase on a sentence, is not appropriate. In particular, a key impact would seem to be the expansion of the subtropics, where may important countries and activities are located. That wet regions become a bit wetter and dry a bit drier seems to me likely to induce smaller impacts than will occur due to the shifts in the boundaries (e.g., storm systems moving away from where there have been tremendous investments in reservoir and aqueduct systems, etc. Indeed, some small countries have end up having a shift go completely over their nations. Thus, I would urge a rephrasing. And this whole point should be covering shifts in the monsoons, etc., which will likely have major consequences--these supposedly small shifts are likely to imply very large impacts, and merit attention. [Michael MacCracken, United States of America]	Noted. The issues raised by the reviewer are covered in later chapters, notable chapter 14. We hope the revised wording of our summary is adequate and conveys the limits of what we are assessing in this chapter.
7-211	7	5	38			Will precipitation extremes increase in frequency or intensity? How are precipitation extremes defined - rainrates exceeding some threshold? [Government of United States of America]	Taken into account. ES has been clarified in that respect by indicating the timescale.
7-212	7	5	40	5	40	Add "aerosols," before "clouds". Projection of precipitation changes on the scale of catchements has also been hindered by environmental changes through aerosol-cloud-precipitation interactions. Although we don't have strong an evidences of aerosol's effect on total rainfall amount, there is increasing evidences of the impact of the PDF of rain [Zhanqing Li, United States of America]	Partly taken into account. The role of aerosol-cloud-precipitation interactions is now mentioned in the revised paragraph.

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7-213	7	5	44	5	47	ES : It there a corresponding statement about rapid responses to greenhouse gases ? [Sandrine BONY, France]	taken into account. The CO2 effect is now mentioned in the ES paragraph.
7-214	7	5	44	5	47	Is this also the case in China and India? Is air pollution really only having influences on the distribution of rainfall and not on its amount? I think it might be useful to have a bit more explanation here, and hope there was consideration of the major international review edited by Zev Levin. [Michael MacCracken, United States of America]	Partly taken into account. The role of aerosol-cloud-precipitation interactions is now mentioned in the revised paragraph.
7-215	7	5	46	5	46	total amont of precipitation I think refers to global precipitation - probably need the word global inserted approprirately [Graeme Stephens, United States of America]	Taken into account. Paragraph revised.
7-216	7	5	47	5	47	"except in so far as they modify globally" is prposed to change into" with the exception that so far they modify globally' [Saviz Sehat Kashani, Iran, Islamic Republic of]	Taken into account. Paragraph revised.
7-217	7	5	49	5	50	The heading of this paragraph seems to suggest more certainty than the actual assessment. Possibly change "could" to "might". [Jan Cermak, Germany]	Rejected. "could" is appropriate here.
7-218	7	5	49	5	50	This statement is way too optimistic because it disregards the model results that indicate that the possible compensations are largely regional, and may have serious negative side effects on the global water cycle as detailed further down [Jost Heintzenberg, Germany]	Taken into account. We now indicate that there would be regional effects in the first sentence. Side effects are discussed prominently in the paragraph below.
7-219	7	5	49	5	52	For an executive summary, the stated confidence in the results of the GeoMIP G1 experiment to assess the effect of stratospheric aerosol injection is over optimistic, considering the rather simple models used for the experiment. These models do not include interactive atmospheric chemistry but focus mainly on the complexity of the aerosol scheme. Therefore, while these models can provide hints on the level of counterbalancing effect of CO2 increase, they are not appropriate for evaluating the risks associated to the considered SRM. Extreme caution on the wording regarding SRM issues is therefore advisable in the executive summary, especially at chapter level. The first sentence mentioning "observations" is also not correct stricto sensu because the only observation so far is linked to the Pinatubo eruption, which occurred at a period characterized by a specific amount of CO2 and chlorine compounds in the atmosphere. Finally, the fact that some publications mentioned in the SRM sections are still only submitted decreases the confidence a reviewer can have on the stated confidence level. [Sophie Godin-Beekmann, France]	Partly taken into account. We have modified the introductory sentence to add a cautionary note. There are many historic examples of volcanoes cooling the planet. Pinatubo is not the only observation. Side effects, including those on stratospheric ozone in the case of stratospheric aerosol SRM, are discussed prominentely in the paragraph below.
7-220	7	5	49	5	55	What I'm clearly missing here are some words and warnings on possible negative effects and feedbacks of SRM with stratospheric aerosols and also other methods. The current statement suggests that SRM with stratospheric aerosols, if feasible, is the ideal way out of climate warming problem. I do not believe this is the current consensus among aerosol-cloud-climate researchers. This paragraph needs to be formulated more carefully, even though there follows another paragraph on SRM risks. I would recommend to merging the statements about SRM possibilities and risks in one paragraph, not to separate them, also with headlines adjusted like "Theory, model studies and observations evaluated the effects and risks of some SRM methods … [Ottmar Möhler, Germany]	Partly taken into account. We now indicate that there would be regional effects in the first sentence. Side effects are discussed prominently in the paragraph below. But we decided against merging the statements about SRM possibilities and risks as it would imply a too long paragraph.
7-221	7	5	49	6	16	Given the significant potential risks of Solar Radiation Management (SRM) and the uncertainty in exactly how it would work the first statement (page 5, lines 49-50) seems a little optimistic. It might be worth pointing to the caveats listed below within this first statement to prevent problems from this being read in isolation. [European Union]	Taken into account. We now indicate that there would be regional effects in the first sentence. Side effects are discussed prominently in the paragraph below. The first paragraph is not on risks or technological feasibility.
7-222	7	5	49	6	16	A link here to other chapters/sections discussing geoengineering would be very useful to the reader e.g. Carbon Dioxide Removal in Ch 6, near-term climate projections in Ch 11. Is SRM (or other) considered in any mitigation scenarios? Also, as SRM/geoengineering makes up a significant portion of this chapter and is highly important for policy makers it would seem appropriate to carefully bring out some of this into the SPM - particularly the point that although SRM can reduce surface temperature in theory this does not result in an exact reversal of GHG induced climate change - the knock on regional changes in precipitation regimes could be very different with a reduction in global average precipitation. [European Union]	Rejected. A link to other chapters (within WGI, II and III) is made in Section 7.7 but is not appropriate in the ES. Material on geo-engineering will be suggested for the SPM and TS.
7-223	7	5	49	6	16	Please consider adding the word "geoengineering" to the executive summary, and explain that SRM is one of two distinct groups. [Government of NORWAY]	Taken into account, implictly in the subheading title 'Geoengineering using SRM methods".

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7-224	7	5	50	5	51	Because there isn't enough evidence for the medium confidence, the sentence "There is medium confidence (medium evidence, medium agreement) that" etc should be deleted. [HUA ZHANG, China]	Rejected. The confidence assessment is appropriate for the statement as justified in Section 7.7.
7-225	7	5	50			maybe replace "realisable" by "feasible" [Andrea Flossmann, France]	Taken into account. "realisable" has been changed to "practicable".
7-226	7	5	52	5	52	“two-fold”? – not sure why the focus on two-fold, and in any case it would be for equivalent CO2 not CO2 probably. Could be written more generally [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account. We have revised the sentence to try to clarify the point that SRM can be scaled up to 4 Wm-2.
7-227	7	5	52	5	52	At the end of the sentence, after "concentration" add: ", however regional temperature changes of up to the order of a quarter of the original one from CO2 doubling can not be prevented." The reason for adding this phrase is that it should be made clear that regional effects remain when performing SRM, e.g.over northern midlatitude land and the polar regions (figure 7.23).! The current text suggests full compensation of CO2 doubling by SRM is possible, which is too optimistic. [Peter Van Velthoven, Netherlands]	Taken into account. We make the point that the compensation is imprecise in the next paragraph, and the sentence does not indicate that ALL effects are offset. It specifically says "some" effects.
7-228	7	6	2	6	2	The sentence "It should be noted that no technology for SRM has been fully developed and can be considered ready for large-scale deployment" (page 53, line 49-51), should be included in the executive summary, with a reference to 7.7.1. [Government of NORWAY]	Taken into account. We have inserted a phrase indicating that SRM is unimplemented and untested.
7-229	7	6	4	6	4	mention DTR, or in general the night – day asymmetry in SRM vs. greenhouse effect [Johannes Quaas, Germany]	Rejected. We have tried to provide an example of side effects, but cannot list them all in the executive summary. This is too much detail for the ES.
7-230	7	6	4	6	6	This is a very interesting statement given that the IPCC paradigm, in presenting the collective effects of forcing from many different forcings with many different latitudinal and vertical aspects, simply adds them up and even suggests that GWPs can be used to relate the collective effects of substances with very different lifetimes. While I agree with the statement, it seems to me it also needs to be evaluated (and stated, if possible) whether the deviations mentioned here are large or small as compared to those arising from how the various other anthropogenic factors are being treated and indeed how they compare to short-term climate variability, effects of volcanic eruptions or variations in soloar radiation, etc. Basically, this kind of statement seems to me needs to be stated relative to some other factor/measure that causes variations in the distribution of changes. [Michael MacCracken, United States of America]	Noted. But this is an issue that is much broader than for just SRM.
7-231	7	6	4	6	8	It is not clear from this summary point that these residual changes in climate could be of a different nature to the changes in response to global warming, e.g. Cooler but drier in some regions. I believe this difference is very important and should be made clear. [Peter Irvine, Germany]	Taken into account. We make the point that the compensation is imprecise. The text is not inconsistent with the point, but we cannot be comprehensive in the summary.
7-232	7	6	4	6	8	In my opinion, the fact that SRM would not offset the suppression of global precipitation due to elevated CO2 levels should be mentioned here as well in the body of the chapter. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted.
7-233	7	6	8	6	8	Please consider to include the content of this sentence in the executive summary (page 57, line 12-15). "If SRM were used to counter positive forcing, it would be needed as long as the CO2 concentrations were high. If greenhouse gas concentrations increase, then the scale of SRM to offset the resulting warming would need to increase" [Government of NORWAY]	Accepted. A similar sentence was added to the second SRM paragraph of the ES.
7-234	7	6	10	6	16	As shown by observations after the Pinatubo eruption, elevated amounts of stratospheric aerosols have an effect on ozone at global scale and not only in the polar regions, although the effect is stronger in these regions. Mentioned risks in the executive summary should include the decrease of precipitation in the tropical regions. [Sophie Godin-Beekmann, France]	Noted. Ozone will increase away from the poles but only slightly, and it seem less relevant to the ES. We have now also mentioned the effect on global precipitation (which covers the tropics)
7-235	7	6	10	6	16	With regard to the assessment of SRM side effect and risk, it is recommended to conduct a comprehensive review involving ecological and agricultural impacts, apart from climate radiative effect. [Government of China]	Noted. There were not enough studies to do a formal assessment of ecosystem and agricultural impacts, so we do not summarize them here. IPCC does not make recommendations on research that is needed.

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7-236	7	6	10	6	16	You might add that the *lifetime* of aerosols in the lower stratosphere is on order a year, so among the other consequences, the aerosol would have to be renewed periodically or on a continuing basis, and that the sulfuric acid aerosol would rain out in some unknown spatial pattern, likely having other adverse environmental impacts. [Ralph Kahn, United States of America]	Rejected. This is discussed in Section 7.7 but would make the ES too long. The second paragraph of the ES does mention the necessity to increase the SRM effort as GHG increase.
7-237	7	6	10	6	16	This statement really lacks context. If there is going to be any consideration of SRM, it would seem that what decisionmakers considering potential implementation would like to be provided is an indication of the relative impacts of GHG induced climate change without geoengineering versus GHG induced cliamte change with geoengineering. In addition to indicating this, it thus seems to me that significant care has to be used in making absolute statements rather than relative statements. The two specific points listed here are new and unique to SRM, so worthy of mention, but at the same time SRM would, presumably, be limiting overall global warming and so thus presumably not passing some of the possible thresholds (e.g., less ice loss from ice sheets and methane/CO2 release from thwaing permafrost). So, I'd suggest that it is more important to be indicating that what will be needed is a comparative risk analysis (in addition to all the various governance related issues) and that, while desired, this will be a challenge, than to be focusing on suggesting there are other and unexplored impacts--as there are many of these that will also be, presumably, alleviated, at least in part. [Michael MacCracken, United States of America]	Rejected There is considerable resistance to mention of the need for risk benefit analysis in the WG1 report. This should be discussed in the WGII and WGIII reports. In any case IPCC does not make recommendation on research that is needed.
7-238	7	6	11	6	14	“Moreover, if SRM were used to counter a large RF by greenhouse gases and then terminated, most of the warming that had been offset would become evident within a few decades, and the rate of climate change would exceed the rate that would have occurred in the absence of SRM.” The sentence that precedes provides a “high confidence” statements, and the way it stands right now it seems that the above sentence is also a high confidence statement. Is that the intention? [Lazaros Oreopoulos, United States of America]	Accepted. Yes, there is high confidence in this statement and we have clarified the confidence statements.
7-239	7	6	14	6	15	"there would be other unaticipated or unexplored impacts". This statemet can be viewed in two ways; first, it is of course true in the trivial sense that we cannot predict the exact details of the response to SRM geoengineering; second, it appears to be an indirect warning or a recommendation for caution as it invokes (substantial?) unanticipated harm being a certainty. If it is intended in the first sense then I'd recommend replacing it with a more qualified phrase such as: "but due to the untested nature of SRM significant unanticipated or unexplored impacts may be possible.". If a cautionary note is desired then I'd recommend including this explicitly, perhaps in a phrase such as: "SRM is a novel form of anthropogenic influence on the climate and its effects are highly uncertain, as such extreme caution is recommended.". [Peter Irvine, Germany]	Taken into account. We have revised these statement significantly to clarify the uncertainties and unanticipated consequences.
7-240	7	6	15			It is suggested to substitute "will" by "would" because SRM has not been deployed so far. [Klaus Radunsky, Austria]	Accepted
7-241	7	7	1	8	40	Section 7.1 : This whole section is clear and very well written. [Sandrine BONY, France]	Noted. Thanks.
7-242	7	7	3			Better: composed mostly of gases (placement of restrictive adverb) [Stephen E Schwartz, United States of America]	Editorial. Accepted.
7-243	7	7	5	7	5	"sedimentation velocity" is more jargony than might be desired in the opening paragaph. "Fall velocity"? [Robert Pincus, United States of America]	Editorial. Accepted.
7-244	7	7	10	7	11	Why “Until”??? This sentence is misleading. Something like: “Clouds consist of droplets and ice crystals resulting from nucleation of aerosol particles evolving in course of cloud lifetime. The most common thermodynamical process leading to nucleation is adiabatic cooling of rising air” [Government of Poland]	taken into account. Sentence has been changed to "Cloud formation usually takes place in rising air, which expands adiabatically and cools, thus permitting the activation of aerosol particles into cloud droplets and ice crystals in supersaturated air."
7-245	7	7	10	7	11	I suggest to replace "nucleation" by "(so-called) activation", to avoid confusion between cloud droplet nucleation (=activation of an aerosol particle) and aerosol nucleation (=formation of an aerosol particle from condensing gases). Similar dual use of the term nucleation occurs throuhout the chapter. [Bart Verheggen, Netherlands]	taken into account, "nucleation" has been replaced by "activation of aerosol particles into cloud droplets and ice crystals".
7-246	7	7	10			"Clouds usually form in rising air" might be read to be most of the time that there is rising air, clouds form; as opposed to "cloud formation usually takes place in rising air" which I believe is meant and which makes the	taken into account. Sentence has been changed to "Cloud formation usually takes place in rising air,

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						denominator cloud formation, not rising air. [Stephen E Schwartz, United States of America]	which expands adiabatically and cools, thus permitting the activation of aerosol particles into cloud droplets and ice crystals in supersaturated air."
7-247	7	7	11	7	11	I think what you mean here is "… usually through the cloud condensation or ice nucleation processes induced by aerosols." [Ottmar Möhler, Germany]	taken into account, "nucleation" has been replaced by "activation of aerosol particles into cloud droplets and ice crystals".
7-248	7	7	11			I think "freezing on aerosol particles" is meant? [Richard Allan, United Kingdom]	taken into account, "nucleation" has been replaced by "activation of aerosol particles into cloud droplets and ice crystals".
7-249	7	7	11			pay attention to use of term "nucleation" vs "activation". Aerosol particles are activated to form drops or crystals, while drops and crystals nucleate… the nucleation of aerosol particles refers to their formation in the nanometer size. Here, it should be activation of aerosol particle or nucleation of ice crystals... [Andrea Flossmann, France]	taken into account, "nucleation" has been replaced by "activation of aerosol particles into cloud droplets and ice crystals".
7-250	7	7	12	7	12	Or a mixture of liquid and ice at a wide range of scales. [Brian Kahn, United States of America]	Rejected. The sentence is specifically about individual "cloud particles" not about "clouds" which can indeed be composed of a mixture of liquid water and ice.
7-251	7	7	12	7	12	Line 12: I suggest that "mostly" be deleted, but use the following: "… composed of liquid water, ice or both." [Kuo-Nan Liou, U.S.A.]	Rejected. The sentence is specifically about individual "cloud particles" not about "clouds" which can indeed be composed of a mixture of liquid water and ice.
7-252	7	7	12			The time evolution of aerosols and …? The authors might consider using evolution, because evolution also includes spatial evolution, not just temporal. [Government of United States of America]	Rejected. As we mention dynamical, radiative and microphysical processes, "evolution" can be loosely taken here to indicate both the spatial and temporal evolution of the clouds.
7-253	7	7	12			better: composed mostly [Stephen E Schwartz, United States of America]	Editorial. Accepted.
7-254	7	7	15	7	15	Use another word for "comprehend" [Daniel Murphy, United States of America]	Editorial. "comprehend" changed to "seize".
7-255	7	7	15	7	15	Why is precipitation "difficult to comprehend"? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Editorial. "comprehend" changed to "seize".
7-256	7	7	15	7	17	It might also be noted that precipitation is influenced by atmospheric dynamics and thermodynamics, which change moisture distributions and cloudiness in ways that may or may not induce precipitation. [Adrian Simmons, United Kingdom]	Rejected. Although the reviewer is absolutely right, this would complicate the sentence in a way that is not needed for an introductory paragraph. It is already mentioned that cloud evolution is influenced by atmospheric dynamics and thermodynamics.
7-257	7	7	15			precipitation is not difficult to comprehend, but maybe difficult to forecast. Please change the word "comprehend" here and take out the comma behind. [Andrea Flossmann, France]	Editorial. "comprehend" changed to "seize". The manuscript will be copy-edited, should the comma be superflous.
7-258	7	7	15			Is precipitation difficult to quantify, perhaps? [Government of United States of America]	Editorial. "comprehend" changed to "seize".
7-259	7	7	19		20	Strictly, an aerosol is a suspension of particles in air; so better: "Atmospheric aerosol particles are small solid or liquid particles suspended in air; these particles can be of natural or anthropogenic origin." other edits for clarity. [Stephen E Schwartz, United States of America]	Editorial. Accepted.
7-260	7	7	19			and glossary: The definition of aerosols is in conflict with corresponding definition in physical chemistry that includes the carrier gas. [Jost Heintzenberg, Germany]	Partly take into account. The sentence has been modified and a footnote was added to justify the use of "aerosols" as a shortcut for "aerosol particles".
7-261	7	7	21	7	24	Reformulate to be comprehensible [Paul Ginoux, United States of America]	Rejected. Comment is not specific and it is not clear to the authors what is not comprehensible here.

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7-262	7	7	21			These particles can serve ...; please note I am not doing a full tech edit; lots of other, similar issues in these paras. [Stephen E Schwartz, United States of America]	Rejected. We have added a footnote to indicate that we take "aerosols" to mean "aerosol particles" in the Chapter for convenience.
7-263	7	7	22	7	24	Suggestion for an addition to this sentence: "… and by facilitating or speeding up chemical reactions" (e.g. ozone destruction in PSC's or aqueous phase SO2 oxidation) [Bart Verheggen, Netherlands]	Partly taken into account. The role of aerosols in atmospheric chemistry is now mentioned.
7-264	7	7	26	7	26	I am having trouble wrapping my head around this. Are the authors really saying that aerosols are as complex and time/space varying as clouds? I am not sure I buy that. Clouds are way more complex and variable from my experience. [Brian Kahn, United States of America]	Rejected. The sentence is correct as it and does not seek to rank the variability or complexity of aerosols and clouds.
7-265	7	7	27	7	28	"...relatively sharp edges and fine scale variations in-cloud properties..." citations should be added. E.g., from recent papers on fine-scale cloud structure: Haman KE, Malinowski SP, Kurowski MJ, Gerber H, Brenguier J-L. 2007. Small-scale mixing processes at the top of a marine stratocumulus – A case-study. Q. J. R. Meteorol. Soc. 133: 213–226. DOI: 10.1002/qj.5 Siebert H, Franke H, Lehmann K, Maser R, Saw EW, Schell D, Shaw RA, Wendisch M. 2006b. Probing finescale dynamics and microphysics of clouds with helicopter-borne measurements. Bull. Am. Meteorol. Soc.87: 1727–1738. doi: /10.1175/BAMS-87-12-1727 [Government of Poland]	Rejected. These are general statements that do not necessarily call for a reference.
7-266	7	7	32			The text begins discussing how changes in clouds will change the planet's radiation budget, but no information about their role in the climatology has yet been presented. It seems to skip over mentioning the important role of clouds in the mean-state, jumping right into how a change in clouds may act as a feedback. [Government of United States of America]	Rejected. The role of aerosols and clouds on the planet's radiation budget was mentioned at lines 6-8.
7-267	7	7	33			potency is a strange word here, maybe better "strength" [Andrea Flossmann, France]	Editorial. "potency" changed to "magnitude".
7-268	7	7	34			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, United States of America]	Rejected. It seems this comment refers to the wrong page or line number.
7-269	7	7	38			I would add fall speed of particles from cirrus, identified by Sanderson as a parameter that strongly influences climate sensy. Sanderson BM, C. Piani, W. J. Ingram, D. A. Stone, M. R. Allen; Towards constraining climate sensitivity by linear analysis of feedback patterns in thousands of perturbed-physics GCM simulations. Clim Dyn (2008) 30:175–190 DOI 10.1007/s00382-007-0280-7 [Stephen E Schwartz, United States of America]	Rejected. Sedimentation of ice particles is one of the cloud dissipation processes which are mentioned on line 38.
7-270	7	7	40	7	40	Plesae make sure all abbrevations in use are defined e.g. CMIP3. [Caroline Leck, Sweden]	partly taken into account. "CMIP3" has been deleted here.
7-271	7	7	40			What is CMIP3? The authors should probably assume that the reader is not familiar with all the acronyms and, therefore, they should be defined. [Government of United States of America]	partly taken into account. "CMIP3" has been deleted here.
7-272	7	7	41			The current wording is not clear. A possible rephrasing could be: "...simulate a cloud feedback that ranges from near-neutral to positive." Right now it sounds like there are two options: near-neutral or positive. [Government of United States of America]	Accepted.
7-273	7	7	44	7	45	The statement is true for 'atmospheric aerosols' in to-to, rather than only to the 'anthropogenic' subset; though the latter only is amenable to mitigation. Natural aerosols also exhibit large spatio-temporal changes, some of which, though , are triggered indirectly by human activities (forest fire, land use changes and related dust emission etc) [K KRISHNA MOORTHY, INDIA]	Partly taken into account. "Anthropogenic aerosols" has been replaced by "Aerosols of anthropogenic aerosols" to indicate that this does not exclude e.g. biomass burning aerosols which are partly natural. Pure natural aerosols do interact with radiation and clouds, but this does not result in a radiative forcing in the IPCC sense.
7-274	7	7	45			replace the first "and" by "but also" [Andrea Flossmann, France]	Editorial. Accepted.
7-275	7	7	46			Change while to whereas [Jost Heintzenberg, Germany]	Editorial. Accepted.

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7-276	7	7	47	7	47	add "of aerosols" after "the net anthropogenic radiative forcing" [Peter Irvine, Germany]	Partly taken into account. We meant the "net anthropogenic RF" as stated, but "negative" and "positive" were swapped.
7-277	7	7	51	7	52	About the sentence: "....even if we had a perfect knowledge of the temperature record (Stevens and Schwartz, 2012).", it is not possible to know “perfectly” the temperature variable, since the “uncertainty (or error)” of a continous function cannot be cero (even the Heisenberg principle put a lower limit to the uncertainty of any physical variable). [Rubén D Piacentini, Argentina]	Rejected. The meaning of the sentence is clear.
7-278	7	7	51		51	An additional key reference here is 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. doi: 10.1175/1520-0442(2002)015<3117:AOBEOT>2.0.CO;2; also Andreae et al cited in FOD. [Stephen E Schwartz, United States of America]	Particle taken into account. Current reference changed to Andreae et al. (2006).
7-279	7	7	52			Restore language from FOD: It was also found that the total anthropogenic forcing is inversely correlated to climate sensitivity in climate models used for CMIP3 (Kiehl, 2007). Add to text following that sentence. The variation in forcing and the resultant latitude in climate sensitivity in models that were able to accurately represent the temperature increase over the twentieth century was due in great part to variation in aerosol forcing among the models compared. That sentence goes a long way toward justifying the need for better constraint on aerosol forcing. [Stephen E Schwartz, United States of America]	Accepted (if space allows)
7-280	7	7	54	7	54	conceptualized -> illustrated [Robert Pincus, United States of America]	Rejected. "conceptualized" is what we mean here as the concepts of adjustments and feedbacks are introduced.
7-281	7	7	54	8	11	The explanations of the forcing terminology and the corresponding Figure 7.2 are not satisfactory. Even after reading this material several times still is not clear about the details of what is included where. [European Union]	Partly taken into account. A box has been added to the Chapter to explain the concept of rapid adjustments further.
7-282	7	7	54	8	11	Nice para; well stated. Especially definition of feedback and distinction from adjusted forcing. That said, you might qualify the definitions "as used here" because they are not universal, though perhaps they should be. [Stephen E Schwartz, United States of America]	Noted. Thanks.
7-283	7	7	54	8	16	Fig.1 should be redrawn. It is unclear and hard to follow. [Government of Poland]	Partly taken into account. The figure has been improved following comments received.
7-284	7	7	54	8	40	and Figure 7-2. Necessary and excellent discussion, clearing up confusion in previous terminology. [Robert Kandel, France]	Noted. Thanks.
7-285	7	7	56	7	56	You could add a sentence here saying something like: "An idealized study investigated the possibility of optimizing the latitudinal distribution of stratospheric sulfate aerosols to minimize the residual changes in surface temperature (Ban-Weiss and Caldeira, 2010). Ban-Weiss GA, Caldeira K, (2010) Geoengineering as an Optimization Problem. Environmental Research Letters, 5, 1-9. [George Ban-Weiss, United States of America]	presumably incorrect page and/or line number for this comment. There is a related statement made in Section 7.7.4 so we have not added this reference.
7-286	7	7	57			To me RFaci (cloud albedo effect) is an adjustment due to aerosol. The aerosol forcing is only direct, and everything else is an adjustment. I have never been certain how you separate lifetime and albedo aci effects, but trying to put lifetime effects into RF of aerosols does not seem appropriate. [Andrew Gettelman, United States of America]	Partly taken into account. Figure 7.2 makes it clear that the lifetime effect is part of AF and not RF. Whether the cloud albedo effect is part of RF or should be considered as an adjustment is open for debate. The choice made here is to define Rfaci as

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							the Twomey effect but say that this is a theoretical construct and we do not attempt to separate this effect from the other effects.
7-287	7	7	57			Please define 'global radiative energy budget.' ,i.e. the radiative budget of the earth-atmosphere system, that is, the top-of-atmosphere (TOA) radiative budget". Afterwards, be consistent and use the term 'global radiative budget' consistently. [Government of United States of America]	Rejected. TOA is not implied in this sentence, because the term forcing is used loosely here and does not mean "radiative forcing".
7-288	7	7		8		I'd just like to say that I think the Introductory section is excellent! [Richard Allan, United Kingdom]	Noted. Thanks.
7-289	7	8	4	8	4	“a few weeks” – a few weeks of what?! Needs clarifying that it is for the hypothetical case of a suddenly-imposed forcing. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account. More text on rapid adjustment has been added, which clarifies the timescales involved.
7-290	7	8	4			"...thought to occur within a few weeks." Seems that there should be some literature cited here concerning the timescale. Two studies for consideration include: Dong, B., J. M. Gregory, and R. T. Sutton, 2009: Understanding land-sea warming contrast in response to increasing greenhouse gases. Part I: Transient adjustment. J. Climate, 22 (11), 3079-3097. Cao, L., G. Bala, and K. Caldeira, 2012: Climate response to changes in atmospheric carbon dioxide and solar irradiance on the time scale of days to weeks. Environmental Research Letters, 7 (3), 034015. [Government of United States of America]	Taken into account. A box has been added that cites these one or both of two studies.
7-291	7	8	5			Better: "change in global mean surface temperature", not simply "temperature" [Stephen E Schwartz, United States of America]	Taken into account.
7-292	7	8	8	8	8	Adjusted forcing is said to be a new concept. Note that AR4 section 2.8.3 has a preliminary discussion. It might be more accurate to say the concept has been extended and made more central to aerosol and cloud forcing. [Daniel Murphy, United States of America]	Taken into account. We have added a box to provide the context of the AF (now renamed) concept and its rooting in previous studies.
7-293	7	8	9			The authors might consider specifically noting that AF includes RF as well as the response. [Government of United States of America]	Partly taken into account. We have added a box to better define the AF (now renamed ERF) concept.
7-294	7	8	10	8	10	I think it would be useful to acknowledge that there is some ambiguity as to where the semi-direct falls, even though I believe this new terminology is a great improvement on what has been used before. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Rejected. It is not clear to this reviewer why there is an ambiguity with respect to the semi-direct effect. This is clearly an adjustment to ari.
7-295	7	8	13			Figure 7.1 is confusing. Even looking at it a long time I cannot figure out what it is supposed to tell. Too many colors, arrows and not enough explanation [Andrea Flossmann, France]	Partly taken into account. The figure has been improved following comments received.
7-296	7	8	18			Figure 7.2: Caption needs information on the different arrow colours used in the graphic (blue/brown) [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. Caption has been updated.
7-297	7	8	19	8	28	Figure 7.2. Recommend that the text in the caption is included in the main body of the text as there is a lot of information here that is too important to be relegated to a caption. The terminology for describing aerosols radiative effects is quite a departure from the old terminology used in AR4 and older research papers so there needs to be clearer explanation. [Government of United Kingdom of Great Britain & Northern Ireland]	taken into account. Most of the caption is now in Section 7.1.
7-298	7	8	20	8	21	The sentence: "Each of the individual longwave and shortwave CREs is large compared to the ~4 W m–2 radiative forcing of doubling CO2." repeats similar statements in the literature, at times cited to by those who would downplay CO2 forcing significance. The comparison is misleading. Although the CREs are indeed large compared to the radiative forcing by doubling CO2 whose 15 µm band is nearly saturated, they correspond by definition to complete removal of cloud LW or SW opacity. In fact, they are of the same order as the CO2 radiative effect defined as the change in TOA LWRE corresponding to complete removal of CO2 opacity without other changes in atmospheric structure. [Robert Kandel, France]	Accepted. This presumably refers to page 10. The comparison to the CO2 RF has been removed.
7-299	7	8	46	8	46	Section 7.2 : feedbacks...and adjustments ? [Sandrine BONY, France]	Accepted.

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7-300	7	8	50	10	10	Part of this section may contain too much well known background information [Gunnar Myhre, Norway]	Considered. However other reviewers including the TSU have requested this inforrmation.
7-301	7	8	54			replace "nucleate" by "activate" (see comment7) [Andrea Flossmann, France]	Accepted
7-302	7	8				Aerosol-radiation interactions (ARI) and aerosol-cloud interactions (ACI) are so important. Therefore, the authors should consider bringing them forward in the executive summary. Later on there are more detailed results from ARI and ACI. It would be nice to know what they are before digesting those numbers. [Government of United States of America]	Taken into account. ARI and ACI are introduced in Section 7.1 and Figure 7.3. There is a paragraph in the ES to introduce the concept.
7-303	7	9	1	9	4	Brenguier paper focuses on small-scale fluctuations only. There are original experimental (in-situ) papers about wide range of scales, e.g. Malinowski SP, Zawadzki I. 1993. On the surface of clouds. J. Atmos.Sci. 50: 5–13. doi: 10.1175/1520-0469(1993)050<0005:OTSOC>2.0.CO;2 Davis, A., A. Marshak, H. Gerber, and W. Wiscombe, 1999: Horizontal structure of marine boundary-layer clouds from cm- to km-scales. J. Geophys. Res., 104, 6123–6144. doi:10.1029/1998JD200078 Reviews and discussion on about effect of turbulence and turbulent mixing on cloud properties in a wide range of scales and on precipitation mechanism from nucleation to precipitation formation can be found in Bodenschatz E., Malinowski S.P., Shaw R.A., Stratmann F. (2010). Can we understand clouds without turbulence? Science 327: 970–971. Devenish, B. J., Bartello, P., Brenguier, J.-L., Collins, L. R., Grabowski, W. W., IJzermans, R. H. A., Malinowski, S. P., Reeks, M. W., Vassilicos, J. C., Wang, L.-P. and Warhaft, Z. (2012), Droplet growth in warm turbulent clouds. Q.J.R. Meteorol. Soc., 138: 1401–1429. doi: 10.1002/qj.1897 [Government of Poland]	Accepted - thank you, excellent suggestions.
7-304	7	9	1			This is a minor point, but "snow formation for ice clouds" is not a specific microphysical process unlike the other processes mentioned in this sentence; hence it is a bit ambiguous. I believe the authors mean aggregation of ice to form snow, and therefore suggest replacing "snow formation for ice clouds" with "collision and aggregation of ice crystals". [Hugh Morrison, United States]	Accepted
7-305	7	9	7			"Atmospheric dynamics" might be better than "Air circulations" [Adrian Simmons, United Kingdom]	Accepted.
7-306	7	9	10	9	10	"too large" does not make any sense if you don't know that cloud-resolving model is a model of limited domain. [Paul Ginoux, United States of America]	The wording has been changed.
7-307	7	9	10			define "explicit" in the context of cloud models [Andrea Flossmann, France]	The wording has been changed.
7-308	7	9	14			Figure 7.3 is a nice image. [Andrew Gettelman, United States of America]	Thanks.
7-309	7	9	15			Have the cloud systems in Fig. 7.3 been defined previously? e.g. 'mixed phase stratus'? [Government of United States of America]	Considered - we considered this, but to define all these cloud types would be far too much pedagogical material. We think it is informative to give these examples here, but for more basic information other texts must be consulted.
7-310	7	9	18	9	18	"cloud regimes". Aren't these really cloud "types" rather than "regimes"? Many investigators now reserve the word "regimes" for mixtures of cloud types (e.g., Gordon, N. D., J. R. Norris, C. P. Weaver, and S. A. Klein (2005), Cluster analysis of cloud regimes and characteristic dynamics of midlatitude synoptic systems in observations and a model, J. Geophys. Res., 110, D15S17, doi:10.1029/2004JD005027) [Lazaros Oreopoulos, United States of America]	Rejected - for the most part this figure is referring to mixtures of cloud types. We made one small edit to help clarify this.
7-311	7	9	30	10	21	The discussion of annual means is fine. However, there should be some discussion of the annual cycle of zonal means, with SWCRE going to zero in polar winter (cf. e.g. Kandel & Viollier 2010, Fig. 6.) Although it should be obvious, it may be useful to remind readers that the SWCRE is necessarily zero when there is no sun. Observational estimates of SWCRE are also subject to diurnal sampling bias for some cloud types and regions. [Robert Kandel, France]	Rejected - others are complaining already that there is too much pedagogical material and we have no room for additional discussion. The current discussion is so general it is not specific to annual mean, though that is what the figures show.
7-312	7	9	31	9	31	I think the 2/3 estimate is low and a bit sloppy. ISCCP comes in to closer to 70%, while CloudSat/CALIPSO is	Partially accepted -due to contradictory comments we

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						higher. Should be more specific here. [Brian Kahn, United States of America]	have changed simply to "most" of the globa.
7-313	7	9	31	9	31	Line 31: “Clouds cover about two thirds of the globe (Figure 7.4a,c).” However, Fig. 7.4a shows fractional occurrence, but not cloud cover as such. Moreover, CloudSat and CALIPSO are not the best instruments for the determination of global cloud cover. I would think that AIRS and MODIS are more suited for this purpose and they have more than 10 years of datasets available. May I suggest the following: “Clouds cover more than 60% of the globe.” [Kuo-Nan Liou, U.S.A.]	Partially accepted -due to contradictory comments we have changed simply to "most" of the globa.
7-314	7	9	31	9	38	This paragraph provides a number of very specific values but no corresponding references that should be provided. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted
7-315	7	9	31			"On average clouds..." Perhaps be more specific...what fraction/occurence frequency threshold is being used to say that 'clouds cover 2/3rds of the globe'? [Government of United States of America]	Partially accepted -due to contradictory comments we have changed simply to "most" of the globa.
7-316	7	9	33	9	33	Line 33: The value of “-38oC” appears to be not entirely correct. (-40oC has been widely used in text books and cloud physics papers.) Please add a reference. [Kuo-Nan Liou, U.S.A.]	Accepted
7-317	7	9	33			While it is commonly known - at least in approximate terms - it would be good to provide a reference for the homogeneous freezing temperature of water. [Government of United States of America]	Accepted
7-318	7	9	34	9	34	Clouds may be of either *or both* phases at temperatures between 0 and -38C. [Robert Pincus, United States of America]	Accepted.
7-319	7	9	34	9	34	“while temperatures” – as written this sentence is wrong. The tropical upper troposphere is significantly colder than the mid and (in the summer hemisphere) high latitude lower stratosphere, which are at the same altitude, and indeed if this wasn’t the case, the sub-tropical jets would not close off. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted - wording changed
7-320	7	9	34	9	35	"temperatures at any given altitude are warmer in the tropics" is a generalisation, so some qualifying wording is needed, if only involving insertion of a "largely" or a "generally". Temperatures in the tropical uppermost troposphere are colder than at non-tropical latitudes at this altitude, and there regions such as that of the African easterly jet where at low levels the air is warmer further from the equator. See comment 131 as regards near-surface temperature. [Adrian Simmons, United Kingdom]	Accepted. The wording has been changed.
7-321	7	9	36	9	37	At first instance, that seems inconsistent with ice crystals often initiating precipitation, so this statement could benefit from a short explanation (I imagine it has to do with pure ice clouds often being present in very dry air, inhibiting the growth of the ice crystals to large enough sizes. The impression needs to be avoided that the presence of ice crystals inhibits precipitation, since the opposite is true). [Bart Verheggen, Netherlands]	Accepted. The wording has been changed.
7-322	7	9	36	9	38	One should not put a number on the % of precipitating clouds, then say that the number depends on the criterion used, and then not tell the reader what the criterion used to get the 40% number for liquid or the much smaller % for ice is. Many readers will assume you are referring here to a threshold SURFACE precipitation rate below which precip is not detected by current instruments and above which precip is no longer considered to be "light." But Fig. 7.4(d) suggests that you are instead just referring to precipitation-size particles anywhere in the column, whether or not they reach the surface. Then again, your statement in the text that a much smaller % of ice clouds are at least lightly precipitating seems at odds with the consistent finding that most cirrus clouds have a large fraction of their mass in large particles whose fall speeds are significant and would thus be classified as precipitating ice (snow) rather than non-precipitating ice (cloud ice). Fig. 7.4(d) may bias against ice clouds by using the same reflectivity threshold for liquid and ice to define precipitation. These lines need to be rewritten to clarify just what the statistics you are quoting actually refer to. [Anthony Del Genio, United States of America]	Accepted - this statement is now brief and does not specify percentages
7-323	7	9	36	9	38	"Precipitating" doesn't imply reaching the ground, correct? CloudSat is good at detecting precipitation, but it can't tell you if it is reaching the surface because the lowest 3-4 range bins are unusable. [Brian Kahn, United States of America]	Accepted - statement revised
7-324	7	9	36	9	38	The definition of cloud cover is detection limit dependent, which could be mentioned. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted - done

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7-325	7	9	36			Substantiate the 40% by a citation [Jost Heintzenberg, Germany]	Accepted - this statement is now brief and does not specify percentages
7-326	7	9	38			A sentence to elaborate on this thought would be helpful. Also, in Fig 7.4, is precipitation defined as only that reaching the surface? [Government of United States of America]	Accepted - statement revised
7-327	7	9	40	9	40	“clouds above the 440 hPa pressure level are considered “high,”. With tops or bases above 440 hPa? [Lazaros Oreopoulos, United States of America]	Accepted - this is reworded
7-328	7	9	40	9	40	It should be specified that the pressures refer to cloud top pressures [Johannes Quaas, Germany]	Accepted - this is reworded
7-329	7	9	40	9	40	I think “above” and “below” are ambiguous when referring to pressure. Is 450 hPa above or below 440 hPa? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Partially accepted - The 440 hPa note already says "above the 440 hPa pressure level" which should be sufficient, as it should be obvious from the terms "high" and "low" anyway, but we now do this also for the 680 hPa note.
7-330	7	9	40			Since Rossow and Schiffer (1991) were classifying clouds using ISCCP data, it seems that this line needs to specify that clouds *with tops* above 440 hPa are considered high, etc. [Government of United States of America]	Consdered - this has been reworded.
7-331	7	9	40			above and below used in a confusing way since reference is to pressure [W. Paul Menzel, United States of America]	Partially accepted - The 440 hPa note already says "above the 440 hPa pressure level" which should be sufficient, as it should be obvious from the terms "high" and "low" anyway, but we now do this also for the 680 hPa note.
7-332	7	9	45			Do the authors mean "free tropospheric air motion"? [Government of United States of America]	Statement now removed
7-333	7	9	46	9	46	The statement that midlevel clouds are seen mainly in the storm tracks is at odds with Fig. 7.5(b,f), which shows that the greatest concentration of mid-level clouds is actually in the continental ITCZ (with a non-negligible amount, though less than in the storm tracks, in the oceanic ITCZ as well). Given the extensive criticism of GCMs' failure to simulate tropical midlevel clouds and the increasing emphasis on stronger entrainment to produce congestus clouds as an important ingredient for simulating the MJO, these tropical occurrences of midlevel clouds should not be ignored. [Anthony Del Genio, United States of America]	Accepted - reworded; the reviewer is not correct that the greatest concentration is in the ITCZ (see Fig. 7.4), but on the other hand our SOD text ignored the ITCZ which was wrong
7-334	7	9	47			inverse order of words; better "over essentially all" [Andrea Flossmann, France]	Unsure what to change here? It already says this.
7-335	7	9	48	9	48	After "downward" please add: Such low clouds also frequently appear in the shallow and very moist bounady layer over the Arctoc Ocean. [Caroline Leck, Sweden]	Partially accepted - polar clouds now noted
7-336	7	9	48			Should “mean” be placed just before “tropospheric air motion”? [Hugh Morrison, United States]	Accepted - statement removed
7-337	7	9	51			Figure 7.4: I am surprised that there is more clouds on the south pole. Maybe check that the poles were not inverted [Andrea Flossmann, France]	Noted - but it is clear from the tropics, and from the maps, that the figure is not inverted, there really is more cloud detected over the southern ocean. This should not be surprising.
7-338	7	9	52	9	52	I feel there is an ambiguity in frame (b) of Figure 7.4. Is the top solid line the total (liquid plus ice) water path, or is this solid line the ice water path? I also feel the figure needs a peer-reviewed citation or two to back it up. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted
7-339	7	10	6	10	6	The reanalysis used as the source for 500 mb omega in this figure should be named. [Anthony Del Genio, United States of America]	Accepted
7-340	7	10	6	10	6	The source of the 500 hPa vertical velocity, presumably some reanalysis, is not provided. [Lazaros Oreopoulos, United States of America]	Accepted
7-341	7	10	6	10	6	I was missing a reference for the quoted vertical velocities. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted

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7-342	7	10	12	10	24	Recommend adding: "Tropical clouds show a change in albedo from 0.30 to 0.34 or 60 W/m2 between 10 AM and noon, with little variation before and after (Eschenbach 2010). This strong local negative feedback may provide a thermostat type macro negative tropical cloud feedback." Reference: "Willis Eschenbach (2010) The ThermostatThe thunderstorm thermostat hypothesis: How clouds and thunderstorms control the Earth's temperature, Energy and Environment Vol. 21, No. 4 pp 201-216." [David L. Hagen, United States of America]	Rejected - the diurnal cycle of cloudiness does not translate into a feedback on climate (and the 60 W figure is clearly wrong).
7-343	7	10	12	10	24	Recommend addressing negative cloud feedbacks that act contrary to current models. E.g. recommend adding: "Drier soils more likely form afternoon precipitative clouds, giving a negative cloud feedback, contrary to existing models." Christopher M. Taylor et al. (2012) Afternoon rain more likely over drier soils, Nature 489,423–426(20 September 2012)doi:10.1038/nature11377 [David L. Hagen, United States of America]	Rejected - the noted paper does not show or say anthing about clouds or cloud feedbacks.
7-344	7	10	12	10	40	7.2.1.2 Effect of Clouds on Earth's Radiation Budget. This section is missing the major evidence of declining cloud cover of Eastman & Warren 2012. Recommend adding: “The IPCC noted that a 1 percentage point decrease in albedo from 30% to 29% would increase the black-body radiative equilibrium temperature about 1°C, about equal to a doubling of atmospheric CO2 (IPCC report AR4 1.5.2 p.114). This could be due to a 1.5% decrease in clouds since they form up to 2/3rds of planetary albedo. The global average cloud cover now appears to have declined about 1.56% over the 39 years from 1979 to 2009 (~0.4%/decade), primarily in middle latitudes at middle and high levels. (Eastman & Waren, 2012). Thus, most of the observed global warming could be due to declining clouds.”Source: “Ryan Eastman, Stephen G. Warren, Journal of Climate 2012 ; e-View doi: http://dx.doi.org/10.1175/JCLI-D-12-00280.1” [David L. Hagen, United States of America]	Rejected - trends in cloud cover are discussed in Chapter 2, and the overall evidence for trends in cloudiness is not robust.
7-345	7	10	14	10	24	In this paragraph the paper by Stephens et al., Nature Geoscience (2012) should be cited and LWCRF considered to be modified. [Gunnar Myhre, Norway]	Accepted - yes this citation was in a later section but has been moved here.
7-346	7	10	17	10	17	It is preferable to use the terms “solar” and “terrestrial” rather than “short wave” and “long wave” [Johannes Quaas, Germany]	Rejected - we considered this but have chosen to keep shortwave and longwave.
7-347	7	10	17	10	17	As noted in the Exec summary, the Loeb et al values are almost identical to Ramanathan et al’s 1989 values from ERBE. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted, both papers now cited
7-348	7	10	18			I think that long wave number is now considered at least 10% high, but I am not sure if the newest CERES EBAF 2.5 products have been published in the peer reviewed literature yet. [Andrew Gettelman, United States of America]	Accepted - the water vapour influence on this is now noted
7-349	7	10	19	10	20	Although this sentence is technically correct as written, the long-term confusion outside the cloud community about cloud forcing and feedback argues that a clariification of this statement be given. I am not talking here about the scientific issue of short-term cloud adjustments that now are placed into the adjusted forcing, but rather about the simplistic incorrect notions that have found their way into the popular press over the years that the fact that clouds cool the current climate is synonymous with the idea that clouds will mitigate future climate change. At the very least, change the sentence to read "…implies a net cooling effect of clouds on the current climate." Better yet would be to add a sentence, after the one saying that clouds have the potential to cause significant feedback, that clarifies that the sign of the feedback cannot be deduced from the sign of the cloud forcing. I admit that the necessary words can be found earlier and later in this section if one looks for them, but it is in everyone's best interest that the misconception be dealt with clearly and bluntly. [Anthony Del Genio, United States of America]	Accepted - revised as suggested
7-350	7	10	20	10	20	It is very unclear why the CRE is compared to the 4 W m-2 due to doubling CO2, even though this is often done. It feels like comparing an apple with an orange. The point that should be made clear is that if there are cloud feedbacks (which lead to an alteration of CRE) then these would (in percentage terms) only need to be quite small to be competitive with known anthropogenic and natural RF mechanisms which are of order 1 W m-2 [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted - this is now reworded and the direct comparison to CO2 forcing removed.
7-351	7	10	23	10	23	Section 7.2.1.2 : [...] also exert a CRE at the surface and within the troposphere, thus affecting the hydrological cycle and the large-scale atmospheric circulation. [Sandrine BONY, France]	Accepted -
7-352	7	10	23			Although clouds do of course exert a CRE at the surface, numerous studies argue that the global precipitation is constrained primarily by the energy budget of the atmosphere (see comment above). By this logic, a	Accepted - change made

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						change in the CRE at the surface will only affect the global precipitation if it also changes the net radiative heating of the atmosphere. For this to happen it must be different to the change in the CRE at the top of the atmosphere. This will generally be the case in the longwave, but clouds do not absorb strongly in the shortwave, and so changes in shortwave CRE are generally more similar at the surface and the top of the atmosphere (Lambert and Webb, 2008). A change in surface radiation which does not affect the atmospheric energy budget on the timescales of atmospheric equilibriation may then result on a long term warming which will itself change global precipitation, but this would be a consequence of the ultimate warming of the coupled system, not the immediate energetic imbalance at the surface. For this reason, I think it would be preferable to write something like "Clouds also affect the radiative heating of the atmosphere, thus affecting the hydrological cycle." Ref: Lambert, F. H. and M. J. Webb (2008), Dependency of global mean precipitation on surface temperature, Geophys. Res. Lett., 35, L16706, doi:10.1029/2008GL034838. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	
7-353	7	10	27	10	27	Here and other figure captions: define acronyms so that people who only look at the figures can understand them. [Daniel Murphy, United States of America]	Accepted
7-354	7	10	31	10	31	As I pointed out in a comment on an earlier draft, comparing high cloud temperature to that of the surface is the wrong reference to use for the question of what the LWCRE is. Unlike SWCRE, where the contrast between cloud albedo and surface albedo is by far the largest contributor to the cloud effect, for LWCRE, it is the contrast between the cloud radiating temperature and the clear sky radiating temperature that matters. Because of the opacity of water vapor, especially in the tropics which dominates the contribution to LWCRE, radiation to space in clear skies is mostly from a broad range of altitudes centered on the mid-troposphere clear sky emission to space level. Radiation to space from the surface, which occurs only in the narrow IR window near 11.5 microns, is a small fraction of the total clear sky radiation to space. If the surface were the appropriate reference point for calculating clear sky radiation to space, the LWCRE due to low clouds would be much bigger than it actually is (a couple of W/m2 according to Chen and Rossow 2000, J. Clim.), because even low cloud tops in the tropics are ~10 K colder than the surface. [Anthony Del Genio, United States of America]	Accepted - reworded
7-355	7	10	33	10	33	Since you say "and large negative SWCRE" you should also say "with large POSITIVE LWCRE" before that. [Anthony Del Genio, United States of America]	Accepted
7-356	7	10	39	10	39	Section 7.2.1.2 : This statement is right..but it is best placed here ? Would 7.2.4.3.7 be more appropriate? [Sandrine BONY, France]	Accepted - this statement is moved
7-357	7	10	39	10	40	This is a very valid point and suggests there is much to be known about cloud processes -- never mind the aerosols. [Brian Kahn, United States of America]	Thanks
7-358	7	10	39	10	40	I may have misunderstood what was intended here, but the implication seems to be that Wyant et al 2006 assessed the ability of models to reproduce the observed correlation between CRE and measures of stability mentioned in the previous sentence. Although Wyant et al 2006 do note that the three models examined show diverse responses to identical climate perturbations in spite of relatively good agreement with ERBE observations, I can find no mention of stability or inversion strength in that paper. Perhaps this could be rephrased avoid giving the impression that they do? It may also be worth noting that Webb, Lambert and Gregory (2012) examined a larger number of models and found that most of them failed to correctly reproduce the observed relationship between lower tropospheric stability cloud radiative effect in the subtropics. Ref: Webb, M. J., Lambert, F. H., & Gregory, J. M. (2012). Origins of differences in climate sensitivity, forcing and feedback in climate models. Climate Dynamics, 1-31 (in press). [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - wording changed, and statement moved to a later section
7-359	7	10	44	12	28	This section reads more like a review than an assessment and doesnt always make it clear as to where progress has occurred and what are the current limitations. Consider the LES discussion as an example (36-47p11). There is no a prior reason to expect that current LES models represent precipitation any better than coarse resolution models (and in fact they dont). Perhaps more helpful would be the recognition that while the LES resolves cloudscale motions that you could argue are a necessary step towards the realistic represenation of cloud and precipitation processes and aerosol effects on these processes, this still is far from	We have added a discussion to clarify the utility of high-resolution models..

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						sufficient. The sensitvity to microphysicsal formulations and the current crude representation of warm rain across all models is a key limiting factor (e.g. Suzuki et al., 2011). I believe more overarching comments that note microphysical paramterization are still mostly crude in models would be usefu and appropriate. [Graeme Stephens, United States of America]	
7-360	7	10	44			Section 7.2.2.1: The authors do a very good job highlighting uncertainty of GCM microphysics schemes. However, overall I feel that large sensitivities to uncertain microphysics parameters and formulations in high-resolution models (CRM, LES) could be emphasized more. Otherwise, readers may get the impression that microphysics is primarily a challenge for parameterization in large-scale models, but not necessarily in high-resolution models. For example, on p. 7-13 lines 29-33 it is stated that parameterization of microphysics in GCMs is challenging partly because of fundamental details of microphysical processes remain poorly understood, especially for ice and mixed-phase clouds, but this holds true for models of all types and resolutions from LES to GCMs. One could even argue that microphysical uncertainties present a greater challenge in high-resolution models because microphysics is directly coupled to the convective dynamics in these models. I realize that uncertainties related to microphysics in CRM/LES are mentioned on p. 7-11, lines 24-25, but overall this point seems understated. As an example of particular relevance here, recent studies have shown large sensitivity of aerosol effects on deep convection using different types of microphysics schemes in CRMs, even resulting in a change in sign of the effect on precipitation and storm invigoration (e.g., Lebo and Seinfeld 2011; Fan et al. 2012). [Hugh Morrison, United States]	We have added a discussion to clarify the utility of high-resolution models..
7-361	7	10	46	10	46	After cloud condenstaion nuclei please add (CCN). [Caroline Leck, Sweden]	Editorial, accepted.
7-362	7	10	46	10	46	“sub-micrometer” is preferable to “submicron” [Johannes Quaas, Germany]	Editorial, accepted.
7-363	7	10	48	10	48	“progress”: Since when? The following sections related to clouds don't give a sense of what progresses or not have been done since AR4. Some references are dated before AR4 report. [Paul Ginoux, United States of America]	Rejected. The text discusses progress since AR4.
7-364	7	10	51	11	47	The entire chapter 7.2.2.1.1 is rather narrowminded. It gives the impression that explicit (again, what is this?) cloud models is essentially LES/stratocumuls and CRM/cumulus. This is excluding a lot of work done on other type of clouds (arctic?), also regarding aerosol/cloud interaction and precipitation(!!). Should be adressed much wider. PS: I saw that it is indeed correctly adressed in the later chapters, but still you should add something here is well. [Andrea Flossmann, France]	A sentence has been added to clarify the applicability of the LES/CRM strategy to any type of cloud system, anywhere on Earth.
7-365	7	10	51	11	48	For the sake of completeness at the end of this section an information on DNS modeling of cloud properties should be included. Beginning from DNS aimed at understanding of CCN and droplet growth like Lanotte, Alessandra S., Agnese Seminara, Federico Toschi, 2009: Cloud Droplet Growth by Condensation in Homogeneous Isotropic Turbulence. J. Atmos. Sci., 66, 1685–1697. doi: 10.1175/2008JAS2864.1 through DNS aimed at investigation of droplet spatial distribution and droplet collisions like: Wang L-P, Ayala O, Rosa B, Grabowski WW. 2008. Turbulent collision efficiency of heavy particles relevant to cloud droplets. New J. Phys. 10: 075013, DOI: 10.1088/1367-2630/10/7/075013. to modeling of fine-scale turbulent mixing in clouds like and effects on cloud microphysics: Andrejczuk M, Grabowski WW, Malinowski SP, Smolarkiewicz PK. 2004. Numerical simulation of cloud–clear air interfacial mixing. J. Atmos. Sci. 61: 1726–1739. doi: 10.1175/1520-0469(2004)061<1726:NSOCAI>2.0.CO;2 Andrejczuk M, Grabowski WW, Malinowski SP, Smolarkiewicz PK.2006. Numerical simulation of cloud–clear air interfacial mixing: effects of cloud microphysics. J. Atmos. Sci. 63: 3204–3225. doi: 10.1175/JAS3813.1 [Government of Poland]	Accepted. A sentence and one reference have been added.
7-366	7	10	53	10	53	various types of clouds [Ottmar Möhler, Germany]	Editorial, accepted.
7-367	7	10				Section 7.2.1.2: Subsection 7.2.1.2 refers only to top of atmosphere (TOA) effects. Should in-atmosphere effects be dealt with here? For example: L’Ecuyer, T. S., N. B. Wood, T. Haladay, G. L. Stephens, and P. W. Stackhouse (2008), Impact of clouds on atmospheric heating based on the R04 CloudSat fluxes and heating rates data set, J. Geophys. Res.,	We limit the scope of the discussion to stay within length restrictions.

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						113(D8), D00A15, doi:10.1029/2008JD009951. Henderson, D., T. L'Ecuyer, G. Stephens, P. Partain, and M. Sekiguchi (2012), A multi-sensor perspective on the radiative impacts of clouds and aerosols, J. Appl. Meteorol. Clim., in review. Haynes, J. M., T. H. Vonder Haar, T. L'Ecuyer, and D. Henderson (20XX), Radiative heating characteristics of earth's cloudy atmosphere from vertically resolved active sensors, J. Geophys. Res., in review. [Government of United States of America]	
7-368	7	11	5	11	16	LES have been used to test PDFs shapes for statistical cloud schemes. For example: Larson et al. 2002 (doi:10.1175/1520-0469(2002)059<3519:SSAMVI>2.0.CO;2), Bogenschutz et al. 2010 (doi: 10.3894/JAMES.2010.2.10), Perraud et al. 2011 (doi:10.1007/s10546-011-9607-3) [Jean-Christophe Golaz, United States of America]	Testing of parameterizations is mentioned a few lines further down.
7-369	7	11	5	11	16	LES have been critical in the devlopment of modern high-order turbulence closure schemes. For example: Golaz et al 2002 (doi:10.1175/1520-0469(2002)059<3540:APBMFB>2.0.CO;2), Cheng et al. 2004 (doi: 10.1175/1520-0469(2004)061<1621:TLWOIM>2.0.CO;2), Guo et al. 2010 (doi:10.5194/gmd-3-475-2010) [Jean-Christophe Golaz, United States of America]	Testing of parameterizations is mentioned a few lines further down.
7-370	7	11	10	11	16	While I do not disagree with the general usefulness of high resolution models, it feels like their uncertainties are not addressed in comparable detail as the uncertainties of GCMs. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	We have added a discussion to clarify the utility of high-resolution models..
7-371	7	11	11			I think it might be useful to cite some of the CGILS paper at this point as well as later in the chapter – e.g. Bretherton, Blossey and Jones (2012) (already cited elsewhere). [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted
7-372	7	11	12	11	15	I would suggest that our paper using the WRF model to evaluate convective entrainment parameterizations (Del Genio, A.D., and J. Wu, 2010: The role of entrainment in the diurnal cycle of continental convection, J. Clim. 23, 2722-2738) is a good example to cite here as well. [Anthony Del Genio, United States of America]	Accepted. Reference added.
7-373	7	11	13	11	13	Small-scale cloud variability in a very wide range of physical properties, too. For instance, the variability of cloud horizontal scale may have a different behavior with different cloud types, or the optical depth may behave differentl;y from cloud fraction, etc. Also, this is another spot where the fundamental connection between temperature, water vapor, and cloud variability could (and should) be made. [Brian Kahn, United States of America]	The comment is very vague. No change made.
7-374	7	11	18	11	23	I'm just wondering whether the *significant* differences between CRMs and finer-scale models represent random deviations or systematic bias. I'd assume random, as it is stated that the statistical properties of the ensemble are well-represented by CRMs, but if so, it might be worth making this point explicitly, as it speaks to the state of understanding of the underlying processes. [Ralph Kahn, United States of America]	No response needed.
7-375	7	11	19	11	20	Section 7.2.2.1.1 : As far as the cloud fraction is concerned (not the vertical thermodynamic structure), this sentence might be an overstatement. I wouldn't conclude from Xu et al. (2002) that CRMs of deep convective systems reasonably characterize fractional cloud fractions. [Sandrine BONY, France]	The sentence has been reworded.
7-376	7	11	19			Would the phrase "cloud system" be better than "cloud ensemble"? "Ensemble" might be a confusing in this context, where modeling is the focus. Or perhaps ensemble is used to describe several model realizations here? [Government of United States of America]	Accepted.
7-377	7	11	22	11	23	What do you mean by "siginifcantly better or different"? [Jost Heintzenberg, Germany]	The sentence has been reworded.
7-378	7	11	42	11	42	It would be good to inform the reader that unlike GCMs, LES usually have a very superficial representation of aerosol cycles. [Johannes Quaas, Germany]	Taken into account. The section now says "Cloud microphysics, precipitation and aerosol interactions are treated with varying levels of sophistication, and remain a weak point in all models regardless of resolution".

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7-379	7	11	46			Does the phrase "statistics of boundary-layer cloud distributions"mean the statistics of areal cloud fractions? the distribution of varying cloud element extents for an area? The authors should consider clarifying. [Government of United States of America]	The sentence has been clarified.
7-380	7	12	8	12	8	The language is not quite clear: The success of GCRMs arises because they avoid uncertain and/or errooneous cumulus parameterizations altogether. The models themselves may still be biased. [Robert Pincus, United States of America]	Accepted. The text has been changed.
7-381	7	12	8	12	11	Section 7.2.2.1.2 : This statement may be understood as « it is not possible to simulate a realistic diurnal cycle or Asian summer monsoon with a conventional GCM », which I think is not what you mean (but that I have heard sometimes). To avoid this possible misunderstanding, I would suggest to write « [...] challenging for most conventional GCMs ». [Sandrine BONY, France]	Accepted. The text has been changed.
7-382	7	12	8	12	13	The simulation improvements noted here notwithstanding, Noda et al. (2012, J. Climate) report that climatolgical precipitation in non-hydrostatic global models with horizontal resolutions of 7 and 14 km has biases and root mean square errors twice, and correlation coefficients one-half, those in state-of-the-science coupled climate models with horizontal resolutions as coarse as 200 km, clearly indicating that horizontal resolution alone, at least at what is here called GCRM resolution, is not adequate for high-quality climate simulation. The characterization "global cloud resolving model," even for grid spacings as small as 3.5 km, is not accurate. Very few cumulus clouds have horizontal scales that large. [Leo Donner, United States of America]	The text says that the results are sensitive to the parameterizations of turbulence and microphysics.
7-383	7	12	11	12	13	Remove. First we don't know what is NICAM model. Second, we don't care at all that NICAM is in “good agreement with observations”. Third “results are sensitive to parm. of turbulence and cloud microphysics”, if it was the only parameters which were infleuncing models there would be no need to have a 900 pages AR5 report. [Paul Ginoux, United States of America]	The reference to NICAM by name has been removed.
7-384	7	12	11	12	13	My paper is referred in the sentence in Chapter 7; 7.2.2.1.2 Cloud-resolving global models of WG1AR5_SOD_Ch07_Text_Final.pdf (page 7-12, line 11). This sentence should be modified as below, because sensitivity was discussed in Satoh et al., 2010; Kodama et al. 2012, not in Inoue et al. 2010. Inoue et al. (2010) showed that the cloudiness simulated by NICAM is in good agreement with observations from CloudSat and CALIPSO, but the results are sensitive to the parameterizations of turbulence and cloud microphysics (Iga et al., 2011; Satoh et al., 2010; Kodama et al. 2012). New references: Satoh, M., Inoue, T., and Miura, H. (2010) Evaluations of cloud properties of global and local cloud system resolving models using CALIPSO/CloudSat simulators. J. Geophys. Res., 115, D00H14, doi:10.1029/2009JD012247. Kodama, C., Noda, A.T., Satoh, M. (2012) An Assessment of the Cloud Signals Simulated by NICAM using ISCCP, CALIPSO, and CloudSat Satellite Simulators. J. Geophys. Res., 117,D12210, doi:10.1029/2011JD017317. [Toshiro Inoue, Japan]	Accepted.
7-385	7	12	12			Acrpnyms should be defined throughout the text - not just in one large 'compilation of acronyms' for the entire WG report. In this case, NICAM and MJO could stand to be defined. [Government of United States of America]	NICAM is no longer referred to by name, and MJO has been defined.
7-386	7	12	15	12	21	Multiscale Modelling Framework should be mentioned as another name often used for super-parameterization, and that is also how it is called in Figure 7.7. [Government of Australia]	Accepted.
7-387	7	12	18	12	21	This approach is referred to a 'super-parametrization' in the text but as the multiscale modelling framework in Figure 7. It would be good to make this consistent, or at least to make it clearer that they are referring to the same thing. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted.

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7-388	7	12	18			Regarding how superparameterization is described: doesn't superparameterization mean that 2-D CRMs are embedded in each grid cell? If not, what would be the difference between superparameterization and a GCRM? Differences in CRM model domain (smaller in the case of superparameterization)? The differences are not clear. [Government of United States of America]	On lines 15 and 16, the text defines super-parameterization in the way that the reviewer suggests.
7-389	7	12	19	12	12	The wording "process" and "climate" models should not be used, as it might be interepreted that climate models are not based on processes. We suggest to use "fince scale process" and "large scale process" models (This applies also to figure 7.7). [Andrew Ferrone, Germany]	"High-resolution" has been added.
7-390	7	12	23	12	28	It feels like the discussion of superparameterisations is not presented as balanced as the discussion of GCMs for which there is a strong focus on limitations. For example, it is not clear that partially resolved updrafts are a suitable framework for aerosol-cloud interactions, unless subgrid effects are parameterised from e.g. LES models. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	The text cites papers that present results from super-parameterized models, including one that deals with aerosol processes.
7-391	7	12	28	12	28	Add reference to Wang et al. (2012): Wang, M., Ghan, S., Liu, X., L'Ecuyer, T., Zhang, K., Morrison, H., Ovchinnikov, M., Easter, R., Marchand, R., Chand, D., Qian, Y., and Penner, J.E., 2012. Strong constraints on cloud lifetime effects of aerosol using satellite observations. Geophys. Res. Lett., 39, 15, doi:10.1029/2012GL052204. [Steven Ghan, United States of America]	Rejected. The reference is not sufficiently appropriate in the context of this discussion.
7-392	7	12	30	12	37	Figure 7.7 should be updated including information on DNS modeling of cloud processes [Government of Poland]	Rejected. The figure is intended to show the relationships among conventional GCMs, global cloud-resolvling models, and super-parameterized GCMs, which are called MMFs.
7-393	7	12	30			Figure 7.7 is incomprehensible. What are the red brackets, what is MMF. This figure is useless and could be deleted. [Andrea Flossmann, France]	The figure has been modified. MMF is now defined in the text.
7-394	7	12	31	12	37	It will not be clear to the reader that the actual climate change projections discussed in the remainder of the IPCC WG1 document come almost exclusively from the "cloud" in the upper right hand corner of the figure. Simply labeling this part of the figure as "climate system" does not convey the necessary information. Either the figure should be changed so that the upper right portion of the diagram explicitly says "climate change", or a sentence should be added to the captions that makes it clear that the WG1 document is mostly about the models in the upper right portion of the figure. [Anthony Del Genio, United States of America]	The figure has been modified.
7-395	7	12	31			Figure 7.7: Figure excludes superparameterized models which have been explained in a detailed way in the section above. Please consider to include information on those models and there across-scale character [Thomas Stocker/ WGI TSU, Switzerland]	The figure refers to super-paramterized models using the abbreviation "MMF," which is now defined in the text.
7-396	7	12	39	13	23	This section is very poorly written. Some info are even not related to observations but to GCMs!! The section is poorly structured and goes in samll anectodical details instead of summarizing major scientific progress derived from observations. [Paul Ginoux, United States of America]	Rejected. The section refers to models only to show that the observations reveal issues with the models.
7-397	7	12	39	13	23	This section should be organized by first delineating key progress in constrained poorly know but primary parameters, then present succinctly new instruments and field campaigns and how they reduced uncertainties. [Paul Ginoux, United States of America]	The section has been removed, and portions of the old text are now used in several places elsewhere in the Chapter.
7-398	7	12	45	12	49	It should be mentioned explicitly that CloudSat-CALIPSO give access for the first time and at global scale to the vertical distribution of clouds and aerosol properties, which is a major advance that occurred between AR4 and AR5. [Government of Australia]	Accepted. The revised text has been moved to Section 7.2.1.1. .
7-399	7	12	45	12	54	Quid of MISR, MODIS, PARASOL retrieved cloud properties [Paul Ginoux, United States of America]	We no longer attempt to review specific satellite platforms other than those providing data presented in the chapter. We do however refer to the review by Stubenrauch et al. (2013) which discusses the observational capabilities of passive sensors.
7-400	7	12	45			there have been more than just two new satellites launched. [Andrea Flossmann, France]	We no longer attempt to review specific satellite

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							platforms other than those providing data presented in the chapter. We do however refer to the review by Stubenrauch et al. (2013) which discusses the observational capabilities of passive sensors.
7-401	7	12	45			Before the launch of CloudSat and CALIPSO, another A-Train satellite instrument, the microwave limb sounder (MLS), provided the global measurements of ice cloud, water vapor, and gas pollutants that has been actively helping climate models in simulating ice clouds (e.g. Li et al. 2005a; 2007; Jiang et al. 2012), moisture (e.g. Su et al. 2006b; Jiang et al. 2010) and aerosols (e.g. Li et al. 2005b; Jiang et al. 2008) since AR4. Thus the MLS measurements should be at least mentioned in addition to CloudSat and CALIPSO. References: Li, J-L., D.E. Waliser, J.H. Jiang, D.L. Wu, W.G. Read, J.W. Waters, A.M. Tompkins, L.J. Donner, J-D. Chern, W-K. Tao, R. Atlas, Y. Gu, K.N. Liou, A. Del Genio, M. Khairoutdinov, and A. Gettleman, "Comparisons of EOS MLS Cloud Ice Measurements with ECMWF analyses and GCM Simulations: Initial Results," Geophys. Res. Lett. 32, L18710, doi:10.1029/2005GL023788, 28 September 2005a. Li, J-L., J.H. Jiang, D.E. Waliser, and A.M. Tompkins, "Assessing Consistency between EOS MLS and ECMWF Analyzed and Forecast Estimates of Cloud Ice," Geophys. Res. Lett. 34, L08701, doi:10.1029/2006GL029022, 2007. Li, Q.B., J.H. Jiang, D.L. Wu, W.G. Read, N.J. Livesey, J.W. Waters, Y. Zhang, B. Wang, M.J. Filipiak, C.P. Davis, S. Turquety, S. Wu, R.J. Park, R.M. Yantosca, and D.J. Jacob, "Convective outflow of South Asian pollution: A global CTM simulation compared with EOS MLS observations," Geophys. Res. Lett. 32, L14826, doi:10.1029/2005GL022762, 28 July 2005b. Su, H., D.E. Waliser, J.H. Jiang, J-L. Li, W.G. Read, J.W. Waters, and A.M. Tompkins, "Relationships of upper tropospheric water vapor, clouds and SST: MLS observations, ECMWF analyses and GCM simulations," Geophys. Res. Lett. 33, L22802, doi:10.1029/2006GL027582, 2006. Jiang, J.H., H. Su, M. Schoeberl, S.T. Massie, P. Colarco, S. Platnick, and N. Livesey, "Clean and polluted clouds: relationships among pollution, ice cloud and precipitation in South America," Geophys. Res. Lett. 35, L14804, doi:10.1029/2008GL034631, 2008. Jiang, J.H., H. Su, S. Pawson, H.C. Liu, W. Read, J.W. Waters, M. Santee, D.L. Wu, M. Schwartz, N. Livesey, A. Lambert, R. Fuller, and J.N. Lee, "Five-year (2004-2009) Observations of Upper Tropospheric Water Vapor and Cloud Ice from MLS and Comparisons with GEOS-5 analyses," J. Geophys. Res. 115, D15103, doi:10.1029/2009JD013256, 2010 Jiang, J.H., H. Su, C. Zhai, V.S. Perun, A. Del Genio, L.S. Nazarenko, L.J. Donner, L. Horowitz, C. Seman, J. Cole, A. Gettelman, M. Ringer, L. Rotstayn, S. Jeffrey, T. Wu, F. Brient, J-L. Dufresne, H. Kawai, T. Koshiro, M. Watanabe, M. , E.M. Volodin, T. Iversen, H. Drange, M.S. Mesquita, W.G. Read, J.W. Waters, B. Tian, J. Teixeira, and G.L. Stephens, "Evaluation of Cloud and Water Vapor Simulations in CMIP5 Climate Models Using NASA A-Train Satellite Observations," J. Geophys. Res. 117, D1410, 24 PP, 10.1029/2011JD017237, July 2012. [Government of United States of America]	We no longer attempt to review specific satellite platforms other than those providing data presented in the chapter. We do however refer to the review by Stubenrauch et al. (2013) which discusses the observational capabilities of passive sensors.
7-402	7	12	46			A reference for the discussion of CloudSat, CALIOP and CALIPSO is warranted. [Government of United States of America]	Accepted. References now provided.
7-403	7	12	51	12	51	In addition to Illingworth et al. (2007), please also quote Bouniol et al. (2010) who produced an in-depth analysis of the capability of large-scale models to simulate clouds : Bouniol, D. A. Protat, J. Delanoë, J. Pelon, D. Donovan, J.-M. Piriou, A. Tompkins, D. Wilson, Y. Morille, M. Haeffelin, E. O'Connor, and R. Hogan, 2010: Evaluation of four operational model ice cloud representation by use of continuous ground-based radar-lidar observations. J. Appl. Meteor. Clim., 49 (9), 1971-1991. [Government of Australia]	Rejected. The additional reference is not needed.

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7-404	7	12	52	12	54	again the authors imply that only work has been done concerning tropical convection and marine stratocumumuls. There have been big campaigns in the artic e.g. and also on mid-latitude clouds (compare point 16). [Andrea Flossmann, France]	The text has been deleted.
7-405	7	12	52	12	54	What field campaigns? You did not provide an acroynym of the campaigns. Where are they? Whar are their primary objectives? Where were they deployed? You may want to read the aerosol section to have a good idea on how this can be concisely presented. [Paul Ginoux, United States of America]	The text has been deleted.
7-406	7	12	56	12	57	This sentence should make it clear that horizontal distribution of clouds is important too, of course. [Government of United States of America]	The text has been modified.
7-407	7	12	56	12	58	Based on their improved estimates of surface radiation fluxes, Kato et al. (2011) suggest that global precipitation estimates from the Global Precipitation Climatology Project (GPCP) are biased low by about 20%. Climate models evaluated using GPCP have generally been regarded as biased high regarding their global-mean precipitation simulations. The discrepancy between Kato et al (2011) and GPCP is important and requires resolution. [Leo Donner, United States of America]	No response needed.
7-408	7	12	56			As I have commented above, many studies argue that it is the radiative cooling of the atmosphere, rather than the flux at the surface which primarily constrains the global hydrological cycle. Although it is true that the surface radiation is an important part of this, changes in surface radiation alone will not predict the global precipitation change in many situations. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	The net surface radiation is largely balanced by evaporation.
7-409	7	12	58	13	2	Technically this sentence is correct. But the blame should not be placed solely on the models. Since atmospheric net radiative cooling approximately balances latent heating, especially in the tropics, models have been either explicitly or unwittingly forced to simulate inadequate low cloud or moisture in an attempt to get close to the supposedly "observed" global precipitation rate of ~2.7 mm/day claimed by datasets such as GPCP. In fact, it is now known (from the same CloudSat data that Stephens et al. use for LW fluxes) that rain climatologies miss most of the light rain (Berg et al. 2010, J. Appl. Met. Climat.), and they may also underestimate the heaviest rain rates, though the latter is not to my knowledge documented yet. But the bottom line for models has been that one cannot easily match both the GPCP rain rate and the CloudSat surface LW flux at the same time, because the "observed" rain rates and LW fluxes are not consistent with each other - a 10 W/m2 greater downward LW flux demands a global precip rate more like ~3.0 mm/day. So you can say that models have insuffiicient low clouds, but also say that biases in global datasets are partly responsible. [Anthony Del Genio, United States of America]	A sentence has been added to point out the possibility that current precipitation estimates are too low.
7-410	7	13	1	13	1	“larger than the average in climate models”: What is their mean value and provide reference. [Paul Ginoux, United States of America]	The reference is Stephens et al. (2012).
7-411	7	13	1	13	2	Too much emphasis on a single publication that is still controversial. [Daniel Murphy, United States of America]	Rejected. The emphasis is not excessive.
7-412	7	13	2			Does Stephens et al. (2012) implicate only clouds in the longwave flux bias, not lower tropospheric moisture? [Government of United States of America]	Lower troposheric moisture is mentioned in the text.
7-413	7	13	4	13	23	We would like to bring attention to the recent work of Haynes et al. (in review) that may be a useful reference in the background material of Chapter 7. The manuscript is available at http://www.engr.colostate.edu/~jhaynes/papers/haynes_in_review_oct12.pdf Chapter 7.2.2.2 discusses recent observational advances in understanding the role of clouds in the climate system. Haynes et al. (in review) uses active radar/lidar observations from CloudSat and CALIPSO to investigate the role of clouds in the heating of the atmospheric column. It provides a four-year, near-global climatology at a vertical resolution of 240 m, which has not been observed in the past due to passive remote sensing limitations. Information from Haynes et. al. (in review) would complement the Chapter 7 discussion of the problem of low cloud production in models, near the existing reference to (Chepfer et. al, 2008; Haynes et al., 2011; Naud et al., 2010). Specifically, Haynes et al. (in review) finds that low clouds in the mid-to-high latitude winter hemisphere produce a large cloud top cooling signature near 775 hPa. This has implications for the general circulation; if this cooling by vast areas of low cloud is missed in models, circulation errors are possible.	Reference noted, but text not modified.

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						Reference: Haynes, J. M., T. H. Vonder Haar, T. L'Ecuyer, and D. Henderson (20XX), Radiative heating characteristics of earth's cloudy atmosphere from vertically resolved active sensors, J. Geophys. Res., in review. [Government of United States of America]	
7-414	7	13	4	13	23	In the review of recent advances in cloud observations, I suggest mentioning recent observations of exceptionally large (~1000s km) and persistent cirrus clouds (>2 days) [Taylor, et al., Cirrus cloud-temperature interactions in the tropical tropopause layer: a case study, Atmos. Chem. Phys.,2011] [Jeffrey Taylor, United States of America]	Reference noted, but text not modified.
7-415	7	13	5	13	5	convective systems [Brian Kahn, United States of America]	Accepted.
7-416	7	13	5	13	5	all types of convective systems [Ottmar Möhler, Germany]	Accepted.
7-417	7	13	5	13	6	"not always captured by models" What is this information? Are you telling us that we spend billions in satellite instruments to know something obvious? And is that all these 3 cited publications found out? [Paul Ginoux, United States of America]	The sentence has been changed.
7-418	7	13	5			systems (not system) [W. Paul Menzel, United States of America]	Accepted.
7-419	7	13	6	13	8	Section 7.2.2.2 : [...] especially over high-latitude continents [...] and over subtropical oceans (Nam et al., GRL, 2012). [Sandrine BONY, France]	Accepted.
7-420	7	13	7	13	13	"Cloud layers at different levels overlap less often than often used in GCMs" First, you better know what it means "cloud layers at different levels overlap". Second, the eventual problem of GCM has noting to do with the title of the section "Recent Observational Advances". Third, why we should care? [Paul Ginoux, United States of America]	The text has been moved to 7.2.1.1.
7-421	7	13	7			change "less often than often" [Andrea Flossmann, France]	Accepted.
7-422	7	13	12	13	13	Is Kay a friend of yours that we should know? Otherwise, let us know why we should care about his work as this was not explained in previous sections. [Paul Ginoux, United States of America]	Improvements in our ability to test climate model simulations are valuable.
7-423	7	13	15	13	15	"unexpectedly" Why is that? [Paul Ginoux, United States of America]	The text has been deleted.
7-424	7	13	15	13	16	This statement I would recommend to formulate more carefully: "…can be an artefact of …". First of all Korolev et al. certainly did not analyse all cases of large ice crystal numbers, and, as far as I know, other authors still claim to measure large ice crystal numbers without shattering artefacts. [Ottmar Möhler, Germany]	This text has been deleted.
7-425	7	13	15	13	17	The statement "that aircraft observations of … numerous small ice crystals are largely an artefact of crystal shattering" is too generalized. Yes, shattering has been observed, but cases with high concentrations of small cirrus particles exist Cooper and Garrett (AMT, 2011, and J. Appl.Met, 2010). Hence, I suggest deleting this statement. It is a too special issue and cannot suitably assessed in such a short paragraph. [Ulrich Schumann, Germany]	This text has been deleted.
7-426	7	13	16	13	16	"crystal shattering" What is this? What is this "helping to reconcile in-situ and satellite"? We have no idea what you are talking about. [Paul Ginoux, United States of America]	This text has been deleted.
7-427	7	13	18	13	18	After "tested globally" you can add the reference Delanoe et al (2010) - Delanoë J., Hogan R. J., Forbes R. M., Bodas-Salcedo A., Stein T. H. M., 2010: Evaluation of ice cloud representation in the ECMWF and UK Met Office models using CloudSat and CALIPSO data, Quarterly Journal of the Royal Meteorological Society 137, 661 (2011) 2064-2078. [Government of Australia]	Rejected. This paragraph has been deleted.
7-428	7	13	20	13	20	"CRM" And we are again speaking about modeling in a section focusing on recent advances in satellite data. [Paul Ginoux, United States of America]	The observations reveal errors in the models.
7-429	7	13	20	13	23	This statement is unjustified. The data sets of number concentration referred are algebraic combinations of the more fundamental retrievals of optical thickness and particle size. There are uncertainties in the retrievals of optical thickness and significant uncertainties in the retrieval of particle size, especially in inhomogeneous or broken clouds. Retrieval errors are of opposite sign in broken clouds, which tends to reduce the error in	Accepted. Sentence deleted.

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						number estimates (and in liquid water path estimates) but these estimates have been subjected to relatively little scrutiny. The comparisions cited are more bleeding-edge than state-of-the-art. [Robert Pincus, United States of America]	
7-430	7	13	20			What is meant by "too early"? Too early in the day? Too early after saturation occurs? [Government of United States of America]	The text has been clarified.
7-431	7	13	21	13	22	"Global datasets of low-lying liquid cloud droplet number concentration based on passive observations at multiple wavelengths are being improved". Many assumptions go into these retrievals which burdens them with substantial uncertainty (Bennartz 2007 quotes uncertainties up to 80%) and they are usually applied to only near-overcast marine clouds. Quaas et al. (2006) global application of their simple eq. (1) formula to derive CDNC is, in my opinion, unjustified and unwarranted. Their "popularity as a metric for climate model simulation of aerosol-cloud interactions" is therefore in my view ill-advised. I suggest that this sentence be deleted. [Lazaros Oreopoulos, United States of America]	Accepted. Sentence deleted.
7-432	7	13	25			Section 7.2.3: The challenges of cloud parameterization in climate models are well stated in this section. I realize there are strict length limits on the text, but an additional approach for improving cloud parameterization in GCMs is the use of unified convection/PBL/turbulence parameterizations that are based on assumed joint probability density functions (PDFs) and prediction of various PDF moments, such as the Cloud Layers Unified by Binormals scheme (CLUBB; Golaz et al. 2002). These PDF-based schemes are also built on the idea of unifying parameterization of shallow convection and moist turbulence, similar to the approaches cited on p. 7-15 line 9. However, an added benefit of a PDF-based approach is that microphysics can be driven in an explicit, consistent, physically-based way from joint sub-grid PDFs of the thermodynamic and dynamical quantities (e.g., temperature, water vapor, vertical velocity) (Larson and Griffin 2012). Thus, these schemes further build upon the theme of unification by using a single cloud/sub-grid dynamical scheme coupled to a single microphysics scheme to represent all clouds (except deep convective clouds in the current implementations). I note that these schemes have been recently implemented in single-column versions of the GFDL AM3 and NCAR CAM5 models with good results (Guo et al. 2010; Bogenschutz et al. 2012). [Hugh Morrison, United States]	This approach is mentioned in 7.2.3.1.
7-433	7	13	29	13	29	Strictly speaking, the first statement in this sentence is not true, because ice crystals not only form from vapour, but also from freezing of supercooled liquid water or solution droplets. [Ottmar Möhler, Germany]	True, but that level of detail is not warranted in the introductory sentence.
7-434	7	13	29	13	33	Accurate quantification of partitioning between ice and water in mixed-phase clouds, internal versus external mixing of organic and inorganic aerosols, and boundary layer vis-a-vis free-tropospheric aerosols is essential for better representation of aerosol-cloud interactions in the climate models. [Panuganti, C.S. Devara, India]	True, but here we are talking about cloud microphysics, not aerosols (which are discussed in 7.3 and 7.4). No change made
7-435	7	13	29	13	33	Another place that sorely needs some more context regarding temperature and water vapor variability. The turbulent properties should extend to T, q, winds, vertical shear of the environment in which the clouds are embedded, not just the turbulent properties of the clouds themselves [Brian Kahn, United States of America]	The wording has been changed.
7-436	7	13	37	13	37	There are no grids in the real world, so there can be no "subgrid-scale" variability. Just omitting the phrase subgrid-scale would be sufficient. [Robert Pincus, United States of America]	Changed "subgrid" to "small."
7-437	7	13	38			why quote Fig. 7.16 out of order? [Andrea Flossmann, France]	Detailed discussion of the figure is not appropriate until later the chapter.
7-438	7	13	41			Barker et al., 2003, Journal of Climate is a better reference than Cahalan et al 1994 for subgrid variability in cloud and its impact on cloud albedo, IMHO. [Government of United States of America]	Accepted.
7-439	7	13	44	23	45	"cloud macrophysics" would be more precise than "cloud fraction". [Robert Pincus, United States of America]	Rejected. "Macrophysics" is too broad.
7-440	7	13	45	13	45	Line 45: I would suggest that you add a sentence or two to state the status of cloud cover parameterization. Has anyone developed a prognostic equation based on the first principle? [Kuo-Nan Liou, U.S.A.]	Cloud cover parameterizations are included in the various parameterixations mentioned throughout this section.
7-441	7	13	45			The authors should consider adding radiative transfer parameterisation to the list. [Government of United States of America]	Accepted.

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7-442	7	13	46			What is "chemical transport"? [Jost Heintzenberg, Germany]	The sentence has been reworded.
7-443	7	13	51	13	51	Is this supposed to read "evaporation of precipitation" or "evaporation or precipitation"? [Peter Irvine, Germany]	The former. The sentence has been reworded.
7-444	7	13	54	13	55	The following paper also provides a review for different ncleation paramterization useful in this context: Ghan, S. J., H. Abdul-Razzak, A. Nenes, Y. Ming, X. Liu, M. Ovchinnikov, B. Shipway, N. Meskhidze, J. Xu, and X. Shi (2011), Droplet nucleation: Physically-based parameterizations and comparative evaluation, J. Adv. Model. Earth Syst., 3, M10001, doi:10.1029/2011MS000074. [Andrew Ferrone, Germany]	The reference has been added.
7-445	7	13	54	13	55	It could be added that cloud process parametrization is also important in the models used to produce reanalyses that provide information on variations in climate. [Adrian Simmons, United Kingdom]	True but not relevant here.
7-446	7	13	57			but usually at the expense of other aspects of the simulation.' Perhaps the authors could present one example of the other aspects? Otherwise one could assume that the 'other aspects' are not important. [Government of United States of America]	The information requested is available in the references cited.
7-447	7	14	6	14	15	The distinction between one- and two-moment schemes is correct but confusing to the non-expert. Would it not be enough to say that simpler schemes predict only cloud mass, while more elaborate schemes predict both mass and number and allow for a wider range of interactions? [Robert Pincus, United States of America]	The wording has been changed.
7-448	7	14	15	14	17	The Salzmann et al. (2010) version of GFDL AM3 did not participate in CMIP5. [Leo Donner, United States of America]	The reference has been deleted.
7-449	7	14	16			Is "diagnose" the correct word to use? Aren't these schemes predicting the number concentrations? [Government of United States of America]	The wording has been changed.
7-450	7	14	19	14	19	Can "subadiabticity" be replaced with something more informative? [Robert Pincus, United States of America]	The sentence has been reworded.
7-451	7	14	19	14	20	Section 7.2.3.2.1 It would be important to add soluble gases like HNO3 in the list [European Union]	Not clear how this comment is relevant to cloud droplet activation. No change made.
7-452	7	14	20	14	20	For completeness, it would be right to cite one of the widely used Nenes schemes as well, e.g. Fountoukis & Nenes, 2004. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Citation added - But note this needs to be done!
7-453	7	14	21	14	23	Section 7.2.3.2.1 : the problem of GCMs is not so much in their inability to (fully) resolve cloud-scale vertical motions (which is not expected), but in their inability to represent the effect of these motions through parameterizations. [Sandrine BONY, France]	The sentence has been reworded.
7-454	7	14	22	14	22	Aren't the current versions of superparameterized models have a 2-d model as the parameterization? How faithful are 2-d models in reproducing 3-d phenomena? This is probably not the place for this discussion, but it is worth bringing up. [Brian Kahn, United States of America]	Conventional parameterizations rely on one-dimensional entraining plumes.
7-455	7	14	23			Observations from Cloud Sat and the A Train show that cloud liquid water content is systematically oversestimated in climate models, a caveat is needed for this. [Government of France]	Section 7.2.3.4 now cites Nam et al. (2012).
7-456	7	14	43	14	44	Section 7.2.3.2.2 : I would suggest to change « well resolved » in « accounted for », as there might be ways to represent these effects through parameterizations. [Sandrine BONY, France]	Added "or parameterized."
7-457	7	14	43	14	44	"If" the dynamics is not well resolved is not the right word. We know that in GCMs (even including GCRMs) the dynamics of convective updrafts are either not resolved at all or are not resolved well enough to produce the appropriate interactions with the microphysics. Better to say that SINCE the dynamics is not resolved, the issue is whether the interaction with the microphysics can be parameterized in a way that gives useful predictions. [Anthony Del Genio, United States of America]	Added "or parameterized."
7-458	7	14	57	14	57	Saying that improvements in e.g. the MJO tend to come at the cost "of other aspects" of the simulation is more vague than need be. Normally improvments in variability comes at the cost of degradations in the mean state, which is quite interesting. [Robert Pincus, United States of America]	Change made as suggested.
7-459	7	14	57			The text states that improved simulations of the MJO are "usually at the expense of other aspects of the	The additional detail requested is available in the

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						simulations". It might be helpful to provide a few examples of these degredations. [Government of United States of America]	references cited.
7-460	7	15	4	15	4	The citation of my 2007 paper (Del Genio et al. 2007) that was included on p. 14, line 56 would make more sense to include on p. 15, line 4 instead. [Anthony Del Genio, United States of America]	Reference added.
7-461	7	15	8	15	13	Rio et al. (2012, Clim. Dyn.) also couple deep cumulus convection to boundary-layer turbulence and cold pools. Improved diurnal cycles and intra-seasonal variability result (Hourdin et al., 2012, Clim. Dyn.). [Leo Donner, United States of America]	Rejected. The existing text makes this point adequately.
7-462	7	15	9			The last three references do not refer to global models as indicated. [Jean-Christophe Golaz, United States of America]	The sentence has been reworded.
7-463	7	15	12			replace "ameliorated" by "improved" [Andrea Flossmann, France]	Editorial, rejected.
7-464	7	15	15	15	21	Section 7.2.3.3 + 7.2.3.4 : Recent advances in statistical cloud schemes should also be discussed (they are not discussed in Chapter 9 neither). For instance, more models now consider multiple PDFs (e.g. double gaussian, to be consistent with LES results, e.g. Perraud et al., BLM, 2011) to represent the subgrid-scale variability of water (e.g. Jam et al. BLM, 2012), and predict the statistical moments of the PDF(s) based on different subgrid-scale processes (convection, turbulence..). [Sandrine BONY, France]	This approach is mentioned in 7.2.3.1.
7-465	7	15	15	15	21	Section 7.2.3.4. no mention of progress (is there any?) in better specification of systematic diurnal variations in parametrizing cloud radiative effects. [Robert Kandel, France]	This is mentioned in 7.2.3.3.
7-466	7	15	15			I appreciate the addition of this section since the First Order Draft. The relevance arises because a relatively large number of models have adopted these methods since CMIP3. It would be useful to note that overlap is itself a kind of variability, since otherwise the connection may not be clear. In fact, it would be more fair to describe variability arising from overlap first, since all models treat overlap and the methods for doing so in CMIP3 were known to be biased (doi:10.1175/1520-0442(2003)016<2676:ADASRT>2.0.CO;2). [Robert Pincus, United States of America]	Additional discussion of cloud overlap is now given in Section 7.2.1.1.
7-467	7	15	15			The Barker (2008) reference is grey literature; the journal article is doi:10.1029/2002JD003322. [Robert Pincus, United States of America]	Barker et al. is a QJ article.
7-468	7	15	15			The idea of predicting full PDFs of thermodynamic variables is decades old (doi:10.1175/1520-0469(1977)034<0356:TGCMR>2.0.CO;2) and was first implemented in doi:10.1175/1520-0469(2002)059<1917:APPFTS>2.0.CO;2.If the goal is to cite models participating in CMIP which use PDFs then GFDL AM3 should be included (doi:10.1175/2011JCLI3955.1). [Robert Pincus, United States of America]	A reference to Sommeria and Deardorff (1977) has been added.
7-469	7	15	15			Though the new approaches to radiative transfer are stochastic this section is unnecessarily vague. Here's one suggestion: "Since AR3 many models have adopted new methods for treating subgrid-scale variability in cloud properties when calculating the exchange of radiation. These methods are based on drawing discrete samples and are unbiased, which previous treatments of overlap were not." One could optionally add "The discrete samples introduce random but uncorrelated noise in radiant fluxes but this does not affect the simulations as a whole (doi:10.1002/qj.303)." [Robert Pincus, United States of America]	Rejected. The added details mentioned here can be found in the literature cited.
7-470	7	15	17	15	21	Section 7.2.3.4 : Despite these advances, CMIP5 models continue to exhibit the 'too few, too bright' low-cloud problem (Nam et al, GRL, 2012), with a systematic overestimate of the SW radiative effects of low-level clouds for a given cloud fraction ; this bias may arise from different deficiencies (discussed in Nam et al. 2012) , the poor representation of the subgrid-scale horizontal inhomogeneity of cloud optical thickness being one of them. [Sandrine BONY, France]	Change made as suggested.
7-471	7	15	21	15	21	I feel this sentence needs a reference [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	The text refers to Section 7.2.2.2.
7-472	7	15	23	15	23	Lapse rate' only has a short Glossary entry. Thus, the lapse rate feedback may need some more explanation in the chapter as this concept might not be well-known to some readers [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - definition added to lapse-rate section

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7-473	7	15	23	23	10	Section 7.2.4.1 : This whole section is very nice. However, a gap is the absence of discussion of the potential role of changes in convective aggregation or organization on climate and climate sensitivity : CRM studies (e.g. Held et al, 1993 ; Bretherton et al., 2005 ; Muller and Held, 2012) suggest that different forms of convective organization are not equivalent from an energetic point of view, and a recent observational study (Tobin et al., 2012) has confirmed this finding. Khairoutdinov and Emanuel (2010) have suggested (unfortunately, only in a proceeding to my knowledge) that if changes in surface temperature were to generate spontaneous changes in convective organization, it could affect water vapor and cloud feedbacks and hence climate sensitivity. Whether this mechanism actually plays a role in climate change constitutes a key open issue, and I suggest to mention it. [Sandrine BONY, France]	Accepted - Muller and Held and Tobin et al. cited, sentence added
7-474	7	15	25			replace "projecting" by "assessing" or "estimating" [Andrea Flossmann, France]	Unnecesary - clause deleted to save space
7-475	7	15	29	15	31	It is unclear why "(or feedback parameter)" is stated. Is this stating that in some cases the feedback strength is expressed per unit feedback parameter? Or is this where "feedback parameter" is defined? This phrasing is confusing. Also, it seems that there should be a citation for the -3.4 value of the Planck feedback, possibly: Hansen, J., A. Lacis, D. Rind, G. Russell, P. Stone, I. Fung, R. Ruedy, and J. Lerner, Eds., 1984: Climate Processes and Climate Sensitivity: Analysis of Feedback Mechanisms. Geophys. Monogr., Vol. 29, Amer. Geophys. Union, 130–163. [Government of United States of America]	Accepted
7-476	7	15	29			The text states water vapor etc produce most of the simulated feedback and most of the intermodel spread. Is there a section where this analysis is carried out? Or a reference? The magnitude and uncertainty of the feedbacks are discussed in the subsequent subsections - but this reference to the relative importance of these effects seems to be unsupported here. [Government of United States of America]	Accepted - we now refer to Chapter 9 which compares all the feedbacks.
7-477	7	15	30	15	31	Please explicitly state that feedbacks are defined holding all other variables constant, i.e. the computed cloud feedback assumes no change in lapse rate or water vapour (or surface temperature). [Government of United States of America]	Accepted
7-478	7	15	30			The global average surface _air_ temperature (Tas in the CMIP data sets) is often used (for example, in Soden et al., 2008). People do sometimes use the surface temperature (Ts in the CMIP data sets), but I think there is a lot of confusion regarding this definition, since sometimes "surface temperature" is used informally to mean "surface air temperature". It does make a slight difference in the feedback estimates, but I haven't found any cases where it actually changes the conclusions, and the differences between the feedbacks calculated using Tas versus Ts are smaller than the inter-model spread. However, I do prefer to be explicit wherever possible and say "surface air temperature", unless, of course, Ts is being used, in which case the feedbacks may be slightly different from those in some earlier work. [Karen Shell, United States of America]	Partially accepted - since this issue does not significantly affect results, and since it is not always clear which variable is being used in different studies, we do not attempt to distinguish in this report but have added an explicit indication that either model quantity may be used.
7-479	7	15	30			Also, the location of the phrase "(or feedback parameter)" directly after "temperature increase" makes it seem like the former is the definition of the later. Reword sentence: "…expressed as the feedback parameter, its impact on the top …" [Karen Shell, United States of America]	Accepted
7-480	7	15	31	15	31	Line 31: Please add a reference after -3.4 W m-2 K-1. [Kuo-Nan Liou, U.S.A.]	Accepted - Hansen 1984 now cited
7-481	7	15	31	15	31	Maybe Chapter 1 provides the framework, but I feel (maybe via a footnote) that the less expert reader could be helped here, especially with signs. I think the "black-body" (or perhaps better "no feedback") response is the negative of the term here, so that dT=dF/response. It could then be explained that positive feedbacks have negative value, and hence increase dT and negative feedbacks are positive ... [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Partially accepted - standard practice in the literature on individual feedbacks is to use positive values for positive feedbacks, and we prefer this too. The inconsistency of this with some analyses of total sensitivity (and with the glossary definition of "feedback parameter") is now noted.
7-482	7	15	33	16	29	The chapter 7.2.4.1 on the water vapour response basically refers to an exponential increase of WVMR as saturated WVMR is increaseing in such a way, due to temperature increase. This discussion is in contradiction to the argumentation in 7.2.4.3.3 that expects a decrease in low level cloudiness due to surface evaporation reduction following energy constraints. This refers probabaly due to the reduced solar energy due to	Rejected - the statements noted are not inconsistent because they all all refer to different quantities, and these do not have the simple relationships to one another that the reviewer seems to be implicitly

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						particulate pollution. At least the chapters should be consistent as to the response in water vapor. Is it only dependant on temperature, or determined by energy, or both? how? And this part should be consistent with the TS that says that rel hum stays the same. [Andrea Flossmann, France]	assuming.
7-483	7	15	33			Section 7.2.4.1. As I commented on page 3 line 17-24, I feel it would be more logical to move this material to 9.7, where there is already some material on this subject (and no reference to ch7). [Jonathan Gregory, United Kingdom]	Rejected - the chapter scoping has always included this material since the report scoping meeting. A reason for discussing water-vapour feedback here is that questions relating to this feedback revolve around cloud physics
7-484	7	15	35	15	42	The discussion on water-vapour response and feedback (7.2.4.1) is confusing, especially the first paragraph. Is the main point that as temperature rises, WVMR will also rise at a rate similar to the saturated WVMR- so the RH remains nearly constant? Observations seem to strongly support this (Chapter 2). Why would one have expected relative humidily to increase at the same rate as the saturated WVMR? [Government of United States of America]	Accepted - first paragraph reworded
7-485	7	15	37	15	38	"nearly expoentially and very rapidly" is redundant. [Robert Pincus, United States of America]	Rejected - these are not redundant. 0.001%/C would be exponential but not rapid, 1 g/kg/K would be rapid but not exponential
7-486	7	15	40	15	40	“nowhere near this rapid” add “as absolute humidity” to avoid confusion with rapid temporal change [Daniel Murphy, United States of America]	Accepted - first paragraph reworded
7-487	7	15	40			The text states that "changes in relative humidity in warmer climates will be nowhere near this rapid …". Would anyone have expected the RH to increase significantly? The statement seems odd. If there is evidence that the RH will not significantly change - that is worth pointing out if there is evidence for that. And if there is, a reference should be provided. [Government of United States of America]	Accepted - first paragraph reworded
7-488	7	15	40			Is "rapid" the best word? The authors might consider "dramatic" instead. [Government of United States of America]	Unnecessary - phrase deleted/reworded
7-489	7	15	40			"changes in RH in warmer climates will be nowhere near this rapid" confused me at first, since you're comparing changes in saturation specific humidity to changes in relative humidity, which have different units, and it still allows for the possibility of somewhat rapic changes. Perhaps, "changes in relative humidity in warmer climates will be small, contributing little to changes in the specific humidity compared to the influence of the saturation specific humidity." which I think it closer to what you intend to say. [Karen Shell, United States of America]	Accepted - first paragraph reworded
7-490	7	15	40			The phrasing here could be taken to mean that relative humidity is expected to increase in the warmer climate, but not so rapidly as specific humidity. I don't think that can be what is intended, since Sherwood et al (2010) shows that models exhibit increases in relatiive humidity around the tropopause, but decreases in the tropical upper troposphere and in midlatitudes. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - first paragraph reworded
7-491	7	15	41	15	41	"new work highlights some robust changes.." This needs to be tied to a refereed paper. [Anthony Del Genio, United States of America]	Accepted - this point was elaborated and supported furhter down with a citation, but to solve the problem we've simply deleted it here.
7-492	7	15	41	15	41	The variations by region claim should have a reference. [Brian Kahn, United States of America]	Accepted - this point was elaborated and supported furhter down with a citation, but to solve the problem we've simply deleted it here.
7-493	7	15	41	15	41	"new work" begs for a citation or two. [Robert Pincus, United States of America]	Accepted - this point was elaborated and supported furhter down with a citation, but to solve the problem we've simply deleted it here.
7-494	7	15	41	15	41	Can you give references for the 'new work'? [Kate Willett, United Kingdom]	Accepted - this point was elaborated and supported furhter down with a citation, but to solve the problem we've simply deleted it here.

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7-495	7	15	41			The authors should cite this new work that highlights robust changes that vary by region. [Government of United States of America]	Accepted - this point was elaborated and supported furhter down with a citation, but to solve the problem we've simply deleted it here.
7-496	7	15	44	15	53	This paragraph is much better than the corresponding paragraph in Chapter 2, which inexplicably fails to reference Dessler and Davis(2010), as pointed out in comment 118. I do have a couple of comments on the present paragraph, however. [Adrian Simmons, United Kingdom]	No change requested.
7-497	7	15	45	15	47	It might be worth pointing out that plateauing is not so apparent in the observations from ships over the oceans as shown in Willett et al. (2012) - Willett, K. M., D. I. Berry and A. Simmons, 2012: [Global Climate] Surface Humidity [in "State of the Climate in 2011"], Bulletin of the American Meteorological Society, 93, (7), S43-S44. [Kate Willett, United Kingdom]	Unnecessary - we have deleted this reference due to comments of several reviewers, since the near-surface humidity is not important to the feedback and is discussed in Chapter 2.
7-498	7	15	46	15	48	"One exception is that meteorological station data suggest a plateauing of WVMR near the land surface over the last decade or so, but humidity at this level exerts little greenhouse effect..." The "plateauing of WVMR near the land surface over the last decade or so" extends through the troposphere and is global. This plateauing, which can be seen in my column water vapor measurements from Texas (1990 to present), is shown to be global in nature in NVAP-M, a significant improvement and expansion of the first NASA Water Vapor Project (NVAP). The NVAP-M finding should be added to this sentence together with the reference in the following row: [Forrest Mims, United States of America]	Rejected - on the grounds that (1) multiple observing systems such as radiosondes and microwave sounders dispute the claims of the reviewer, (2) trends are treated properly in Chapter 2. The reference to near-surface plateauing has been deleted due to requests from other reviewers.
7-499	7	15	46	15	48	Citation for proposed addition described in previous row: Thomas H. Vonder Haar, Janice L. Bytheway and John M. Forsythe. Weather and climate analyses using improved global water vapor observations. GEOPHYSICAL RESEARCH LETTERS, VOL. 39, L15802, 6 PP., 2012. doi:10.1029/2012GL052094. [Forrest Mims, United States of America]	Rejected - see previous response
7-500	7	15	46		47	Misleading in that this result follows from our physical understanding, given the plateauing of temperature as made plain by 10-25, lines 29-33 – text like that referencing Simmons & al could usefully replace this [William Ingram, United Kingdom]	Accepted - the offending text has been removed, since this issue is discussed in Chapter 2.
7-501	7	15	46			To the best of my knowledge the plateauing of WVMR (and corresponding fall in relative humidity, as temperatures have risen) was identified first in reanalyses, and confirmed by direct analysis of meteorological station data. At least that's how we published it in Simmons et al. (2010). So I would suggest adding "and reanalyses" after "meteorological station data" and adding, at the end of the line after "decade or so" a cross-reference "(Section 2.5.5)". I think it's better for the reader to cross-reference section 2.5.5 rather than reference Simmons et al.(2010) and possible other papers in the peer-reviewed literature. [Adrian Simmons, United Kingdom]	Unnecessary - we have deleted this reference due to comments of several reviewers, since the near-surface humidity is not important to the feedback and is discussed in Chapter 2.
7-502	7	15	47	15	47	“but humidity at this level exerts little greenhouse effect”. This is only true if the greenhouse effect is measured as the difference between upwelling LW radiation at the surface and the outgoing LW radiation at TOA. However, near-surface humidity has a HUGE affect on the downwelling LW radiation to the surface which can also be considered a measure of the greenhouse effect, see for example, Stephens, Graeme L., Martin Wild, Paul W. Stackhouse, Tristan L'Ecuyer, Seiji Kato, David S. Henderson, 2012: The Global Character of the Flux of Downward Longwave Radiation. J. Climate, 25, 2329–2340. [Lazaros Oreopoulos, United States of America]	Unnecessary - we have deleted this reference due to comments of several reviewers, since the near-surface humidity is not important to the feedback and is discussed in Chapter 2.
7-503	7	15	48	15	53	Two sentences refuting Paltridge (2009) seems perhaps overkill. [Robert Pincus, United States of America]	Rejected - other reviewers have praised this, and we feel it is important.
7-504	7	15	50	15	50	Can you go further here and state that the decreasing trends shown in the NCEP-NCAR reanalyses are not reproduced in any of the other reanalyses including both the newer reanalyses and the older ERA-40 reanalysis and that unlike NCEP-NCAR these newer products do not solely assimilate tropospheric humidity from radiosondes which is known to have a dry bias? I'm not convinced that NCEP-NCAR's decreasing tropospheric humidity is as equally plausible as the increasing tropospheric humidity shown in other data-products and feel that the text should reflect this a little more strongly. [Kate Willett, United Kingdom]	Rejected - we believe the existing text already implies clearly enough that the NCEP reanalysis is less believable, and do not have the space for more discussion of this.
7-505	7	15	50	15	51	I would delete "which are considered more reliable for trends". This is a questionable statement. The problem in determining trends is primarily one of instrumental data that have biases that vary over time (as occurs with	Accepted

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						both radiosonde and satellite-measured radiance data) and that vary in coverage over time. These problems have to be taken care of whether one is using reanalysis or carrying out a direct study of instrumental data. Combining different types of data in reanalysis is in fact a good way of identifying and adjusting for spurious trends in some of the types of data. Reanalysis offers the most promising approach to long-term trends over a period when there is a major change in instrumentation, such as middle-stratospheric temperature for the period spanning the change from SSU to AMSU-A data, though there are still issues to be addressed here. I suggest simple deleting words here, as this discussion is best placed in Chapter 2 (even if I have had to make quite a few comments on what is there at present in this draft version). [Adrian Simmons, United Kingdom]	
7-506	7	15	50		51	Seems to wordy to me - “and by actual observations”? [William Ingram, United Kingdom]	Accepted
7-507	7	15	51	15	52	A decrease over Australia is entirely consistent wit hthe drying of the subtropics. This makes complete sense for this region. Other regions should be moistening. [Brian Kahn, United States of America]	No change requested
7-508	7	15	52			Not quite plain what “anomalous” means here [William Ingram, United Kingdom]	Accepted - changed to "exceptional"
7-509	7	15	56			The discussion of using natural fluctuations as analogues for long-term feedbacks is spread out a bit (there's some on page 7-21, iine 41, and some on page 9-77, for example). Some places suggest there may be a difficulty in using seasonal or interannual fluctuations, but other sections don't mention a difficulty. Three recent papers (Colman, R. A., & Hanson, L. I. (2012). On atmospheric radiative feedbacks associated with climate variability and change. Climate Dynamics. doi:10.1007/s00382-012-1391-3; Dalton, Meghan M., and Shell, Karen M., 2012: Comparison of short-term and long-term radiative feedbacks and variability in 20th century global climate model simulations, major revisions for J. Clim; and Dessler, A. E. (2012). Observations of climate feedbacks over 2000-2010 and comparisons to climate models. Journal of Climate. doi:10.1175/JCLI-D-11-00640.1) all three find that short-term feedbacks in GCMs determined using a few decades corresponding to the well-observed recent past are not well-correlated with century-scale feedbacks in GCMs. Note that this doesn't contradict correlations of long-term feedbacks with some quantities (e.g., Hall and Qu), but it does mean the assumption that short-term observations can be used to determine long-term feedbacks is not always valid. [Dalton's paper is available at http://people.oregonstate.edu/~shellk/JCLI-S-12-00752-1.pdf and is due mid-January.] [Karen Shell, United States of America]	Partially accepted - text revised to make clearer the limitations of this.
7-510	7	16	7			Reads as if substituted by humans (as artificial tracers) [William Ingram, United Kingdom]	Accepted - wording modified
7-511	7	16	14	16	14	Regionally there is evidence that anthropogenic-aerosols may be moderating the impacts of long-lived greenhouse-gases, the later which are believed to increase the strength of the Southern Hemisphere jet streams (Kushnir et al., 2001). A study by Rotstayn et al. (2012) explains the weakening of the jet by a realistic treatment of aerosols in their mode, which would explain the multi-decadal observed rainfall decline in south-western Western Australia (Frederiksen et al, 2011). Kushner, P. J., I. M. Held, and T. L. Delworth, 2001: Southern Hemisphere atmospheric circulation response to global warming. J. Climate, 14, 2238-2249. Rotstayn, L.D., Collier, M.A, Jeffrey, S.J., Kidston, J., Syktus, J.I. and Wong, K.K.: Anthropogenic effects on the subtropical jet in the Southern Hemisphere: aerosols versus long-lived greenhouse gases, Environ. Res. Lett., submitted. Frederiksen, C. S., J. S. Frederiksen, J. M. Sisson, and S. L. Osbrough, 2011: Australian winter circulation and rainfall changes and projections. International Journal of Climate Change Strategies and Management, 3, 170{188, doi:10.1108/17568691111129002. [Mark Collier, Australia]	Rejected - these are all valid points but the section is on feedbacks. Aerosol forcing of the circulation is covered elsewhere.
7-512	7	16	20	16	21	"Idealised CRM simulations of warming climates also show upward shifts..." upward shifts in cloud-top height? saturation water vapor mixing ratio? [Government of United States of America]	Accepted - reworded
7-513	7	16	22			Might want to indicate that GCMs suggest that these relative humidity changes, at least on the global average, do not contribute to the spread of climate sensitivites (Held and Shell, 2012) [Karen Shell, United States of America]	Accepted - however this point is now added in the "feedback synthesis" section rather than here.
7-514	7	16	26			Drop “somewhat” or replace with something more specific [William Ingram, United Kingdom]	Rejected - the estimates given in Solomon et al. are highly uncertain; the main point is that it is a small contribution which is conveyed by the current wording.
7-515	7	16	26			“do not have a” → “have no”? [William Ingram, United Kingdom]	Accepted - wording changed

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7-516	7	16	27			Too general: Joshi & al showed only that this was true in 1 case. (It may well be true in all cases published, but we don't know, & I'm sure I could concoct a model for which it was untrue if I tried long enough,) "but where examined this was found to be due to"? [William Ingram, United Kingdom]	Accepted -reworded
7-517	7	16	28		29	The reference should anyway be at the end. [William Ingram, United Kingdom]	Accepted
7-518	7	16	31	17	11	Excellent clarification of an important point. [Robert Kandel, France]	Thanks
7-519	7	16	31			the chapter about lapse rate and WV only treats the tropics; no relationship elsewhere? Should be generalized with respect to the following chapter. [Andrea Flossmann, France]	Accepted - last sentence reworded to clarify
7-520	7	16	37	16	41	There is what to me is a conceptually simpler alternative explanation of the relationship of the lapse and water vapor feedbacks. It involves how greenhouse gases raise mean altitude where the atmosphere radiates to outer space, then that lift is transmitted down to the surface via a lapse rate. Stronger or weaker lapse rates are closer to dry or wet adiabats; hence a relationship between the lapse rate and humidity feedbacks. [Daniel Murphy, United States of America]	Rejected - yes this is an alternate way of expressing the same thing, but does not seem any clearer, does not reveal the key role of relative humidity, and other reviewers liked the way it was currently explained
7-521	7	16	38			"negative feedback on global temperature..." is this global temperature, as in earth-atmosphere system temperature? Clarity in this terminology would be helpful to the reader. [Government of United States of America]	Partially accepted - this is not ambiguous, as the whole feedback concept rests on describing the system by a single scalar temperature change. There is now a new Box 7.1 which helps explain this, however.
7-522	7	16	39	16	41	Section 7.2.4.2 : Discrepancies between lapse rate changes predicted by models and inferred from observations are discussed here for middle and high latitudes, and later on for the tropics (page 17, lines 6-7). It might be good to discuss both together, and then conclude about the expected impact of these discrepancies on climate sensitivity estimates (as done page 17, lines 8-11). [Sandrine BONY, France]	Rejected - this text does not mention any such discrepancy but only notes spread (now reworded). Discrepancies are only discussed later, where the expected impact (nil) is already explained.
7-523	7	16	44			Figure 7.8 is gibberish. What are these lamda values? This figure is of no use, if not explained properly. [Andrea Flossmann, France]	Accepted - figure is now revised.
7-524	7	16	45	16	46	The caption needs to more explicitly guide the reader to what is in the figure. Specifically, after "the Planck response" you should add (lambda-T), and then similarly for lambda-L and lambda-Q. And "individual feedbacks from water vapour and lapse rate" should be changed to "individual feedbacks from lapse rate and water vapour" to match the left-to-right sequence in the figure itself. [Anthony Del Genio, United States of America]	Accepted - figure is now revised.
7-525	7	16	53		54	Tidy line break [William Ingram, United Kingdom]	Editorial
7-526	7	16	53			See earlier comment re:"clear-sky" [Karen Shell, United States of America]	Accepted - this term is dropped
7-527	7	16	57	16	58	Again, this sentence is technically correct but dangerous as written. After all this time I still encounter people who say (even at meetings) that GCMs assume fixed relative humidity. You need to make clearer in this sentence that BECAUSE simulated CLIMATE changes in relative humidity in GCMs are not large, it can be illuminating FOR ANALYSIS PURPOSES ONLY to look at feedbacks in an idealized fixed relative humidity framework. I realize that you have discussed what GCMs actually do on p. 7-15, but one should make every effort to avoid even the chance of a mistaken interpretation by readers who are not knowledgeable about GCMs. [Anthony Del Genio, United States of America]	Accepted - this text is more clearly worded.
7-528	7	16	57	17	4	The argumentation of a reduced impact of specific humidty seems in contradiction to the chapter above. And it seems to support the comment I raised under point 24. [Andrea Flossmann, France]	Rejected - the reviewer has evidently not understood the text. We hope the revised wording corrects this problem.
7-529	7	16	57			Section 7.2.4.1 indicates some regional changes in RH. Maybe "global" rather than "systemic", since these RH changes are related to changes in various atmospheric systems. [Karen Shell, United States of America]	Accepted - added 'global'
7-530	7	16	58			Suggest rephrasing to "...in which relative rather than specific humidity is held fixed..." [Government of United States of America]	Accepted

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7-531	7	16				Figure 7.8 This figure isn't so easy to follow given that the suffixes T, L, H and Q aren't explicitly stated, nor is Planck response discussed in the text. [Kate Willett, United Kingdom]	Accepted - figure revised
7-532	7	17	1	17	2	Saying that "the climate is fundamentally less stable" is confusing. Presumably what is meant is that feedback factors are larger, but this doesn't change the physical system, which the phrase implies. [Robert Pincus, United States of America]	Accepted - reworded
7-533	7	17	1	17	4	Use of term "unphysical" is not advised for one of the choices for defining feedback parameters. Cases can be made for both choices, which have differing merits. The definitions ultimately are arbitrary to some extent. Particularly given the need for IPCC to communicate clearly, the choice of terminology is important. Also along these lines, describing the climate as "fundamentally less stable" with an alternate set of definitions for feedback parameters is likely to confuse all but readers highly cognizant with the feedback literature. [Leo Donner, United States of America]	Accepted - word 'unphysical' deleted
7-534	7	17	1			"In that framework," Which one are we talking about? [Government of United States of America]	Accepted - wording clarified
7-535	7	17	1			“as per” barely English – replace with “as is” - or just a comma? [William Ingram, United Kingdom]	Accepted - phrase deleted
7-536	7	17	3	17	4	The wording (“… in first setting out the traditional framework.”) is extremely confusing. [Yi Ming, United States of America]	Accepted - new wording
7-537	7	17	6			This needs clarification. 1.What is the nature of the departures? Steeper lapse rate or less-steep lapse rate? 2. Specify *moist* adiabatic lapse rate 3. Are the departures present in the mean state, or do they occur as the tropics warms? 4. It is not clear from what is written that a problem exists: Departures from the moist adiabatic lapse rate are expected in the upper troposphere, where the temperature profile is more stable (at pressures less than about 300 hPa) due to the influence of ozone heating. I would not expect the mean state temperature profile nor its change as the tropics warms to perfectly track that of the moist adiabat. 5. "would be larger:" should this be "are"? [Government of United States of America]	Accepted - this section now reworded.
7-538	7	17	6			NO! “moist adiabatic” [William Ingram, United Kingdom]	Accepted
7-539	7	17	8			See earlier comment re:"clear-sky" [Karen Shell, United States of America]	Accepted
7-540	7	17	9			"have little influence on and might even slightl increase it" is somewhat confusing, because I assume you're implicity contrasting with some proposed assumption that the RH would decrease it, but the sign doesn't really matter if there is little influence anyway. It's also not clear to me whether the "increase" is in the (positive) water vapor + lapse rate feedback, or you mean that the increase is in the (negative) total feedback parameter, which would correspond to a decrease in climate sensitivity. [Karen Shell, United States of America]	Accepted - reworded
7-541	7	17	10	17	11	The section covers the evidence well, but the conclusion seems insufficient in one respect. It states that the evidence 'continues to support', but makes no comment on whether confidence is therefore higher or not. [Government of Australia]	Accepted - reworded
7-542	7	17	13	22	3	Section 7.2.4.3 : This section provides a nice assessment of the progress made since the AR4 in our understanding of the sign of cloud feedbacks and the underlying physical processes. However, the section hardly discusses the interpreation of the spread of cloud feedbacks among climate models (it is discussed only line 16, page 17). I didn't find this discussion in Chapter 9 neither (at least for the multi-model spread ; chapter 9 discusses a bit more the spread of cloud feedbacks in perturbed physics experiments performed by single models). It would be worthwhile to assess the current interpretation of the spread of cloud responses to climate change (feedbacks vs adjustments, aerosol vs non-aerosol effects, cloud types or regimes, cloud	Partially accepted - this is an important topic, but is covered throughout the discussions of the individual contributions to cloud feedback. For example there is a figure showing the spread for different cloud types. So we do not feel that this topic has been underrepresented

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						properties, etc). [Sandrine BONY, France]	
7-543	7	17	13			This section is called "cloud feedbacks" but includes sections on rapid adjustments and on observations. The section should be renamed or reorganized, especially as neither the adjustments nor the observations are hinted at in the introductory text. [Robert Pincus, United States of America]	Accepted - title changed and some content added
7-544	7	17	15	17	21	Section 7.2.4.3 : The organization of this paragraph, and its articulation with the following one, could be improved : Progress in the quantification of cloud feedbacks does not come only from the separation of different cloud types but also (and mostly?) from a better distinction between feedbacks and forcings/adjustments (discussed in the second paragraph). I would suggest to mention it right from the beginning. [Sandrine BONY, France]	Accepted - this is now done
7-545	7	17	16			Consider rephrasing to something like: "All global models also continue to produce a near-zero to moderately strong positive cloud feedback." [Government of United States of America]	Accepted - suggested wording adopted
7-546	7	17	21	17	21	The reference to "satellite simulators" is not necessary. The term hasn't been used to this point so is unclear. More importantly, the cloud radiative kernals of Zelinka et al. use ISCCP simulator output as a matter of convenience. It would be more fair to say that the progress has been made by looking at residuals, as in Soden et al. 2008, and in computing the cloud effects directly, as in Zelinka et al. [Robert Pincus, United States of America]	Accepted - suggested wording adopted
7-547	7	17	21			Sanderson and Shell [Sanderson, Benjamin M. and Karen M. Shell, 2012: Model-specific radiative kernels for calculating cloud and non-cloud climate feedbacks, J. Clim., doi: http://dx.doi.org/10.1175/JCLI-D-11-00726.1, in press.] also provides a combined radiative kernel/ISCCP simulator technique for calculating cloud feedbacks that gets around some of the issues with using the same kernels for all the models. Could add this after the Zelinka ref if you want. [Karen Shell, United States of America]	Rejected - thanks for the suggestion but in the interests of brevity we have not added another citation here, especially as we do not have the space to mention the minor issue of mode-dependence of kernels.
7-548	7	17	23	17	31	Section 7.2.4.3 : Some recent assessments of cloud feedbacks (submitted before July 31st!) did separate the effects of feedbacks and adjustments (e.g. Vial et al., Clim. Dyn., 2012). [Sandrine BONY, France]	Accepted - more attention is now paid to the few studies that have reported this quantity.
7-549	7	17	24			See earlier comment re:"surface air temperature" [Karen Shell, United States of America]	Rejected - for simplicity we are not belaboring the distinction other than as noted in the response to the previous comment on this.
7-550	7	17	25	17	26	It would be useful to add a phrase explaining why "some simple methods [for inferring feedbacks] can make positive feedbacks look negative" so that the importance and correctness of the diagnostic advances can be understood. [Robert Pincus, United States of America]	Accepted - short explanation added
7-551	7	17	26	17	28	"Moreover, it is now recognised…rapid adjustments rather than feedbacks", please provide references. [Chien Wang, United States of America]	Accepted - the citation to section 7.2.5.6 has been moved to here.
7-552	7	17	26	17	28	It might aid the reader if some references to the relevant papers are made here (e.g. Gregory and Webb 2008, Andrews and Forster 2008). [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - the citation to section 7.2.5.6, where the papers are cited, has been moved to here.
7-553	7	17	26		28	I think this sentence needs a reference – Gregory & Webb (2008), maybe? [William Ingram, United Kingdom]	Accepted - the citation to section 7.2.5.6 has been moved to here.
7-554	7	17	34	17	35	Lines 34 and 35: What about high-level cloud cover? [Kuo-Nan Liou, U.S.A.]	Accepted - headings and text revised to clarify that the first section includes this too.
7-555	7	17	41	17	41	This is only true for optically thick high clouds such as convective anvils. Thin cirrus clouds have a definite positive net TOA cloud radiative effect (e.g., Chen and Rossow 2000, J. Clim.) [Anthony Del Genio, United States of America]	Accepted - wording changed, and the requested paper is now cited
7-556	7	17	41	17	43	"High clouds at low and middle latitudes exert little net top-of-atmosphere radiative effect in the current climate due to near-compensation between their longwave and shortwave cloud radiative effects (Kiehl, 1994)." There are a few problems with this statement. First, the Kiehl paper is only about the tropics ("low latitudes"), not midlle latitudes, and about deep convection. Second, what is meant by "high clouds"? Cirrus does not have a near-zero net, for example. Third, why is no reference made to Fig. 7.6 which shows the net radiative effect	Accepted - all requested changes made including new paragraph on thin cirrus

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						(zero values are hard to distinguish in this particular color scale)? In the end, I don't think this statement is being substantiated. [Lazaros Oreopoulos, United States of America]	
7-557	7	17	41		43	Kiehl said nothing about mid latitudes, surely? [William Ingram, United Kingdom]	Accepted - text changed
7-558	7	17	42	17	42	The Kiehl reference really isnt the most appropriate reference on this cancellation topic (and in fact that reference fails to properly decribe how the cancellation comes about). The cancellaltion was noted in studies before erbe, by Twomey in the 80s, note in one I published in 1980, and others but perhaps the most appropriate reference on this topic I believe is the paper of Harrison et al (1987) that provied the first early results of ERBE and most quntitiative estimate of the net CRE . Estimating these cloud radiative effects was one of the main achievements of ERBE. [Graeme Stephens, United States of America]	Accepted - reference added to Harrison et al. (1990)
7-559	7	17	46	18	23	I remain unconvinced that the FAT is real and I know I am not alone on this so this entire discussion will raise some degree of skepticism amonst experts snd fodder for criticism (which I am not against). As in my FOD comments I emphasize again that cloud feedback has to take into consideration the combined LW and SW effects that the FATfails to do so. You can see how these effects largely offset in Fig 7.9 for high clouds so you can see FAT is hardly the whole story. I assume these results in this Fig are global but this is not stated (and should be). I had always considered FAT hypothesized for tropical anvils and when focused on the tropics the SW and LW effects cancel even more than they do globally. So yes while the raising of convective cloud tops is expected and obvious and supported the net effect on radiation balance is less certain and not at all obvious. While I dont quibble over the most likely postive ferdback verbage (p3 line 26) I consider this high cloud feedback is much more uncertain than is the opinion in this chapter. [Graeme Stephens, United States of America]	Partially accepted - the text has been substantially revised to discuss the offsetting between LW and SW noted by the reviewer, but also to note that it is successfully reproduced by the same GCMS that are showing FAT, thus it cannot be evidence that something is missing from GCMs as the reviewer alleges. Also the role of CRMs in supporting FAT, and in arguing against compensating changes in cloud albedo, is now more clearly articulated. Uncertainties associated with thin cirrus are much more strongly emphasised in the following section
7-560	7	17	54	17	54	See earlier comment - it is the difference between the cloud and the clear sky emission level temperatures that matters, not the difference between the cloud and surface temperatures. [Anthony Del Genio, United States of America]	Accepted - the reviewer's suggested alternative also has problems however because it implicitly counts water-vapour changes as part of the cloud feedback. We have found a new wording that we hope will be satisfactory.
7-561	7	17	54	17	55	without necessarily affecting albedo'? According to whom? and what is the basis of this rather weak statement - clearly the resultsof Fig 9 don't support this statement and in fact the comment on p18 line 32 contradicts this [Graeme Stephens, United States of America]	Accepted - this statement has been replaced by a more careful discussion of the evidence.
7-562	7	17	54			"increasing the cloud's greenhouse effect without necessarily affecting its albedo"; The Zelinka and Hartmann 2010 paper has a clearer description of this feedback: "Because the clouds are not warming in step with the surface temperature, [they] become less efficient at radiating away [energy]...thus the clouds are acting as a positive feedback on climate." Also, should greenhouse effect be clearly defined in this chapter, as it is in chapter 1? [Government of United States of America]	Partially accepted - this statement has been revised, but the suggested statement also has problems so has not been adopted. Greenhouse effect is defined in the glossary and in previous chapters
7-563	7	18	1	18	4	The wording suggests that the "high cloud" and "cloud height" contributions are independent of each other, when really they are individual terms from two different ways of subdividing the entire cloud feedback. [Karen Shell, United States of America]	Accepted - this section has been reworded.
7-564	7	18	2			reads to me too much as if "high cloud" & "cloud height changes" are alternatives [William Ingram, United Kingdom]	Accepted - this section has been reworded.
7-565	7	18	4	18	5	and elsewhere. There are good comparisons of GCM to cloud-resolving models. An example of what an assessment should do. [Daniel Murphy, United States of America]	Comment unclear. In any case this has been rewritten to clarify the role of the CRMs .
7-566	7	18	5			Reference to Kubar et al. 2007 is not correct, as it did not include a cloud-resolving model. Instead, reference the following in addition to Kuang and Hartmann 2007: Tompkins, A. M., and G. C. Craig, 1999: Sensitivity of tropical convection to sea surface temperature in the absence of large scale flow. J. Climate, 12, 462–476. Harrop, Bryce E., Dennis L. Hartmann, 2012: Testing the Role of Radiation in Determining Tropical Cloud-Top Temperature. J. Climate, 25, 5731–5747. [Government of United States of America]	Accepted - suggested references added.

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7-567	7	18	8			“predicted” → “simulated” [William Ingram, United Kingdom]	Accepted - wording changed
7-568	7	18	15	18	18	The altitude structure and dynamics of global tropopause layer, particularly over tropics, need detailed study for better understading of strtosphere-troposphere coupled/exchange processes that play a pivotal role in the aerosol-cloud chemistry. [Panuganti, C.S. Devara, India]	Rejected - there is no space in this chapter to add any more discussion of this topic, especially in this section which does not concern aerosol-cloud chemistry.
7-569	7	18	18			The reference to Zelinka and Hartmann (2010) is incorrect and should be replaced with the following: 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. [Government of United States of America]	Accepted
7-570	7	18	19	18	19	Norris et al. (2012) was rejected. [Robert Pincus, United States of America]	Accepted
7-571	7	18	19	18	26	Clarity issue: ll. 19 and 20 report a rising trend in high clouds as confirmation of GCMs. Ll. 26 and 27: "Nearly all GCMs simulate a reduced…high cloud amount in warmer climates in low- and mid-latitudes. The former probably is meant as evidence for the altitude feedback, but the text is confusing to read as is. [Leo Donner, United States of America]	Partially accepted - there has been some revision of this text
7-572	7	18	19		20	This statement is not supported in Stubenrauch et al. (2012) nor in the publications from scientists working with data from remote sensing of clouds (GEWEX, HIRS, AVHRR, MODIS Science Team, …). For the satellite community, the ability to infer the presence of optically thin ice clouds depends critically on careful calibration of each sensor that comprises the record. This is due to the cloud signal being close to the signal-to-noise ratio for the sensor channels. The satellite community is spending considerable time improving sensor calibration, which will lead to a better description of cirrus properties over the past 30 years. Given that, there is some evidence of tropical opaque high clouds increasing as well as decreasing in the past 20 years (HIRS reprocessing after recalibration is revealing modest decreases in the Western Pacific from 1987 to 1996, followed by increases from 1996 to 2001, and thereafter decreases – averaging out to little overall change). Extending data sets further back to 1979 requires careful radiometric recalibration that is yet to be accomplished for AVHRR or HIRS. [W. Paul Menzel, United States of America]	Accepted - Norris et al was rejected and is no longer cited, nor is any claim now made of observed change in cloud height.
7-573	7	18	20			Does this mean the Norris et al report found a trend in high clouds increasing in altitude? [Government of United States of America]	Accepted - this reference has been removed.
7-574	7	18	22	18	22	replace “satellite” with “instrument” (Terra is the satellite and MISR the relevant instrument) and “artefact of instrument problems” with “an artefact of sampling biases in the satellite data” (see Evan and Norris 2012). [Lazaros Oreopoulos, United States of America]	Accepted - text has been shortened making this complaint irrelevant
7-575	7	18	25	19	2	It seems to me that "cloud amount" is not adequate term here. Changes in vertical extent, horizontal coverag, or both reflect different forcing/feedback/response mechanisms. [Chien Wang, United States of America]	We have defined the term 'cloud amount' for purposes of this chapter to be an inexact term encompassing cloud quantities both in the horizontal and vertical directions.
7-576	7	18	30			This statement does not appear to be entirely true. In Figure 7-9, it is clear that the net high and mid-level cloud feedback is positive and there does not appear to be support for "a near-cancellation of LW and SW effects for the mid- and high-level cloud amount changes". In fact in many models, high- or mid-level cloud feedbacks are larger than those from low clouds, so the statement that the net effect "comes mainly from the changes in low clouds" is a little misleading. [Government of United States of America]	Accepted - these statements have been reworded for better clarity.
7-577	7	18	31	18	31	Section 7.2.4.3.1 : [...] this comes mainly from changes IN low clouds [...] [Sandrine BONY, France]	Editorial
7-578	7	18	31	18	31	add "of" between "changes" and "low clouds". [Chien Wang, United States of America]	Editorial
7-579	7	18	31			changes in low clouds [W. Paul Menzel, United States of America]	Editorial
7-580	7	18	34	18	44	Section 7.2.4.3.1 : it could be mentionned that the impact of circulation shifts on cloud phase changes will be discussed in section 7.2.4.3.4. [Sandrine BONY, France]	Accepted

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7-581	7	18	43	18	43	"in global average", should this read "on global average" or "on the global average"? [Peter Irvine, Germany]	Accepted
7-582	7	18	48			Which analysis is referred to when "above analysis" is stated? [Government of United States of America]	Accepted - wording removed
7-583	7	18	51	18	54	This section highlights the fact that because models do not produce enough cloud in the mean state, the negative feedback arising from increases in cloud amount may be unrealistic. This logic makes sense, however a second opposing effect is not discussed here: The negative cloud feedback in these regions is dominated by the cloud optical depth feedback, not the cloud amount feedback (Soden and Vecchi 2011; Zelinka et al 2012b, Zelinka, et al. 2012c). Because models tend to simulate clouds that are too optically thick in the mean state compared with ISCCP (Zhang et al. 2005, Klein et al. 2012), this implies that the negative optical depth feedback magnitude is underestimated, per the argument of Stephens (2010). Thus there are reasons to believe that a large negative cloud feedback at high latitudes is plausible, and perhaps underestimated by models. The reader is left with a different impression from reading this section. Klein, S.A., Y. Zhang, M.D. Zelinka, R.N. Pincus, J.Boyle, and P.J. Gleckler, 2012: Are climate model simulations of clouds improving? An evaluation using the ISCCP simulator. Accepted pending minor revisions to J. Geophys. Res. Soden, B. J. and G. A. Vecchi, 2011: The vertical distribution of cloud feedback in coupled ocean-atmosphere models. Geophys. Res. Lett., 38, L12704, doi:10.1029/2011GL047632. Stephens, G., 2010: Is there a missing low cloud feedback in current climate models? GEWEX News, 20 (1), p.5-7. Zelinka, M.D., S.A. Klein, and D.L. Hartmann, 2012b: Computing and Partitioning Cloud Feedbacks Using Cloud Property Histograms. Part II: Attribution to Changes in Cloud Amount, Altitude, and Optical Depth. J. Climate, 25, 3736–3754. doi:10.1175/JCLI-D-11-00249.1. Zelinka, M.D., S.A. Klein, K.E. Taylor, T. Andrews, M.J. Webb, J.M. Gregory, and P.M. Forster, 2012c: Contributions of Different Cloud Types to Feedbacks and Rapid Adjustments in CMIP5. Accepted pending minor revisions to J. Climate. Zhang, M. H., and Coauthors, 2005: Comparing clouds and their seasonal variations in 10 atmospheric general circulation models with satellite measurements. J. Geophys. Res., 110, D15S02, doi:10.1029/2004JD005021. [Government of United States of America]	Accepted - this has been shortened and reflects multiple interpretations rather than just the one given in the SOD.
7-584	7	18	56	19	2	An important point about the cloud radiative feedbacks has been missed here although I had mentioned it in my comments on the FOD as I recall. My paper (Stephens and Ellis, 2008) noted how the radiative effects of high clouds are enhanced by the reduction of low and mid level clouds. So even when no change to high clouds occur, the radiative effects of these changes to clouds below induce feedbacks. This was discussed wrt AR4 in that paper and I think it is the forst to do so. This in turn had an important feedback on precipitation and exemplified a cloud-radiation feedbacks can influence preciptation. This feedback was discussed in my paper Stephens et al., 2005 based on analysis of observations, it is also supported in teh Lebsock et al observational study and is also demonstrated in CRM RCE experiements I reported on (Stephens et al., 2009). See also comment 11 below. [Graeme Stephens, United States of America]	Rejected - the impact of low cloud changes on the hydrological cycle via their radiative effects, which is not a radiative "feedback" per se at least as the word is being used in this chapter, is mentioned in the section on the hydrological cycle. The papers noted by the reviewer are interesting but do not quantify mechanisms of feedback on the global radiation budget relevant to section 7.2. We have added a note to the text explaining that 7.2.5 focuses only on feedbacks on the global energy budget.
7-585	7	19	1	19	2	This is one of those dangerous things the GCM world tends to do - because a large impact of thin clouds on climate change has not yet been simulated by any model, we tend to take the consensus as unofficial guidance that the feedback is not important. But what we need to do in each case is to ask ourselves whether we have a physical basis for anticipating that the simulated feedback (or lack thereof) is correct. So e.g. you rightfully claim some confidence in the cloud height feedback because the physics of it is thought to be well understood (although even there one might wonder whether a future generation of GCMs that incorporates stronger convective entrainment might get a different result). But for thin cirrus do we have any confidence that we are simulating them well at all? We clearly are not resolving them vertically, we are determining how	Accepted - agree completely and this paragraph has been strengthened.

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						much ice is detrained from convection using a precip efficiency tuning knob in many GCMs, we do not have sophisticated parameterizations of homogeneous nucleation in most models, and in probably no model can we make a confident statement about the role of heterogeneous vs. homogeneous nucleation at very cold temperatures. So should the responses of GCMs so far being small be presented as support for the unimportance of thin cirrus, or should our limitations in simulating thin cirrus be the basis for stating that we are not yet in a position to tell whether thin cirrus are or are not important to climate change? I'd argue for the latter, and the sentence as written does not give me that impression. [Anthony Del Genio, United States of America]	
7-586	7	19	1			Not true – the effect was large in HadCM2, as documented by Williams, Senior & Mitchell (2001) [William Ingram, United Kingdom]	Accepted - ref added and statement modified to read "recent GCMs"
7-587	7	19	3	19	3	The following section (7.2.4.3.3) finishes with a useful paragraph in which "confidence" is expressed. I feel it would be useful to do the same for 7.2.4.3.2 [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted - done
7-588	7	19	4	19	51	Section 7.2.4.3.3. cites and discusses a considerable amount of recent work on this important and difficult question. However, there is no discussion or even mention of progress (is there any?) in better modelling of systematic diurnal variations observed for many low cloud systems, especially the "most potent". The diurnal variation must strongly affect the SWCRE for such clouds. Reliable modelling of these diurnal variations, and how they can change with warming, is essential if estimates of low cloud radiative feedback are to be considered reliable. [Robert Kandel, France]	Accepted - a brief mention of the diurnal cycle issue is now made earlier in the chapter.
7-589	7	19	4	19	51	it's better to indicate that all the works discussed here did not include aerosol related effects. [Chien Wang, United States of America]	Accepted - this is now noted in the beginning of the feedback section since it applies to the entire section.
7-590	7	19	4	20	18	Sections 7.2.4.3.3 + 7.2.4.3.4 : Brient and Bony (GRL, 2012 : How may low-cloud radiative properties simulated in the current climate influence low-cloud feedbacks under global warming ?) suggest that the strength of cloud-radiative effects in the current climate might affect the amplitude of the low-cloud response in climate change (but not its sign), owing to a robust and positive feedback between PBL cloud-radiative effects, PBL relative humidity and cloud fraction (which is likely to be at work in every model). Given the over-estimate of PBL cloud-radiative effects in most CMIP5 models (Nam et al, GRL, 2012 : The too few, too bright low-cloud problem in CMIP5 models), this so-called 'beta' feedback is likely to enhance the spread of the model cloud feedbacks. [Sandrine BONY, France]	Rejected - since we are very short of space, even the sign of the low-cloud feedback is uncertain, and it is not clear what the implications are for feedback strength of the "too few too bright" problem, the reviewer's point is judged too far down in the details for us to add more text.
7-591	7	19	5	19	6	Might it not be preferable to cite more of the low cloud feedback studies that contributed to this finding in AR4? This conclusion was based on several papers, and strictly speaking what is written is inaccurate, as Bony and Dufresne (2005) examined feedbacks in transient coupled model experiments, rather than the slab model experiments used at that time to relate feedback spread to equilibrium climate sensitivity. If there is not room to cite other papers, perhaps a more general citation (Bony et al 2006, or Randall et al 2007) would be more appropriate. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - citation changed to Randall et al. 2007.
7-592	7	19	6	19	6	Section 7.2.4.3.3 : In addition to Bony and Dufresne (2005), you could mention Webb et al. (2006). [Sandrine BONY, France]	Accepted - citation changed to Randall et al. 2007.
7-593	7	19	9	19	10	I don't think that Williams and Webb 2009 do actually show that the inter-model spread in low cloud feedback is 'most potent' over tropical and subtropical regions. In fact, the bottom panel of their Figure 5 suggests that the inter-model spread of the extra-tropical feedback is larger than that in the tropics in the 10 models versions they examined. It is true that the results from Williams and Tselioudis (2007) suggest this, although that study examined just six models. Perhaps it would be appropriate to move the reference to Williams and Webb (2009) to the 'although not exclusively so' part of the sentence, in the parentheses with Trenberth and Fasullo, 2010. It may also be appropriate to add a reference to Webb, Lambert and Gregory (2012), who show that inter-model differences in cloud feedbacks and adjustments over the tropical oceans contribute more to the range in climate sensitivity than other regions, but that those other regions are required to account fully for the highest model sensitivities. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - citations moved, and statement simplified
7-594	7	19	9			Problem is perhaps not a good term. It does not seem to be a problem, but a spread or feedback uncertainty	Accepted

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						[Andrew Gettelman, United States of America]	
7-595	7	19	9			"potent"'s not the right word, surely! "greatest"? [William Ingram, United Kingdom]	Accepted - word deleted
7-596	7	19	12	19	15	Section 7.2.4.3.3 : In addition to Medeiros et al. (2008) and Zhang and Bretherton (2008), you may also cite Brient and Bony (Clim. Dyn, in press) who found the same low-cloud response to climate warming in multiple model configurations (OAGCM, AGCM, aquaplanet AGCM and single-column model). [Sandrine BONY, France]	Accepted - citation added
7-597	7	19	25		27	This sentence is badly obscure. [William Ingram, United Kingdom]	Accepted - sentence revised
7-598	7	19	27			I am not sure that Webb and Lock, 2012 is relevant to this point. Perhaps the intention was to refer to Webb, Lambert and Gregory (2012)? Even so, I'm not convinced that this statement is compatible with the results from WLG12 (Please see my comment above referring to Page 7-10, Lines 39-40.) [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - reference unnecessary and deleted.
7-599	7	19	28	19	28	Should this read "appears to have a small effect" rather than "appears to be a small effect"? [Peter Irvine, Germany]	Accepted
7-600	7	19	28		30	It should be acknowledged that such a mechanism would operate on scales not properly resolved by GCMs, so their collectively not doing it much could well be a systematic error [William Ingram, United Kingdom]	Rejected - the evidence that the effect is small does not come primarily from GCMs (see following section).
7-601	7	19	32	7	45	Somewhere in this paragraph the main conclusion of the Zhang et al. (2012) CGILS paper should be discussed: That some SCMs seem to be dominated by the increase in surface evaporation and boundary layer deepening in a warmer climate, while others seem to be dominated by the drying of the boundary layer by cloud-top entrainment or shallow cumulus subsidence in a warmer climate, and this may explain much of the spread in the parent GCM simulations of low cloud feedback. [Anthony Del Genio, United States of America]	Rejected - this paper is already cited and these competing mechanisms are already discussed, albeit with different language than suggested by the reviewer.
7-602	7	19	32	19	45	contradiction to chapter 7.2.4.1 as mentioned under point 24. All discussion on humidity should be harmonized. Also, there should be some discussion on cloudiness. [Andrea Flossmann, France]	Rejected - there is no contradiction between anything in this paragraph, which discusses boundary-layer clouds, and the earlier sections which discussed free-tropospheric humidity.
7-603	7	19	35	19	36	I think that a reference to Webb and Lock, 2012 is more appropriate than a reference to Lock, 2009 at this point, as Lock 2009 did not consider the warming climate or cloud feedback. (Note Lock 2009 is cited appropriately earlier in the chapter). [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted
7-604	7	19	36	19	38	I agree that Reick et al 2012 and Webb and Lock 2012 have in common the idea that energetic constraints prevent surface evaporation increasing at a rate sufficient to balance other changes in the moisture budget of the subtropical cloud layer. However, Webb and Lock 2012 also found in their model that boundary layer mixing transports more water into the subcloud layer and less into the cloud layer, possibly because of reduction in local winds. This might be relevant to mention. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Rejected - too much detail for the space available
7-605	7	19	40	19	41	"These mechanisms could explain why models consistently produce positive low-cloud feedbacks." Are most GCMs really capable of representing the three mechanisms described? [Lazaros Oreopoulos, United States of America]	Reworded to 'These mechanisms, crudely operating through parameterized representations of cloud processes, could explain why climate models consistently produce positive low-cloud feedbacks'
7-606	7	19	41			Are these specifically the low cloud feedback parameters that are being cited, or the total cloud feedback? Are the values cited from Webb et al. (2012)? [Government of United States of America]	Accepted - now clarified, with citation to the relevant figure.
7-607	7	19	45			Most conclusions of the above paragraph rely on model simulations, however if the low cloud liquid water content is overestimated, that leads to an underestimate of the sensitivity of the albedo, hence to a possible understimate of the negative component of the feedback mechanism. [Government of France]	Duly noted, but there are many other isssue with model simulations of similar importance. This is handled by increasing the assessed uncertainty range compared to that predicted by model simulations.
7-608	7	19	47	19	51	This is a very nice summary. [Robert Pincus, United States of America]	No change requested

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7-609	7	19	47		51	Again, the plausible inadequacy of GCM vertical resolution should be acknowledged [William Ingram, United Kingdom]	Rejected - the existing text already notes the poor ability of GCMs. Vertical resolution is moreover not the essential point since in principle this could be overcome with good parameterisations.
7-610	7	19	48	19	48	Should it be "low cloud amount" here? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted - title of section changed to add "amount"
7-611	7	19				Although some authors suggest that overestimations of cloud brightness will cause an increased magnitude of feedback (e.g. Karlsson et al. 2008 Clim Dyn) the non-linear increase in albedo with LWC means that models with positive LWC biases will simulate an unrealistically low change in albedo sensitivity to changes in LWC and therefore underestimate the strength of this feedback (e.g. Graeme Stephens, GEWEX News) [Richard Allan, United Kingdom]	Duly noted, but there are many other isssue with model simulations of similar importance. This is handled by increasing the assessed uncertainty range compared to that predicted by model simulations.
7-612	7	20	1			add energy constraints (modification in evaporation) in the discussion [Andrea Flossmann, France]	Rejected - not obvious to us that any energy constraint enters in here, and it is not necessary to list more factors and we need to save space
7-613	7	20	4			“optical depth” here is “opacity” on the figure: change one for consistency [William Ingram, United Kingdom]	Accepted
7-614	7	20	5			The authors might consider reivisng the text to read: "...yielding a positive global mean longwave optical depth feedback....outweighs the negative global mean shortwave optical depth feedback in most models." Note that this statement may need to be changed if the authors decide to use a slightly modified version of the Zelinka et al. (2012b) results, which have been provided to Lead Author Steve Sherwood. In these revised CFMIP1 results, the ensemble mean SW optical depth feedback outweighs the LW optical depth feedback. Also, the dominance of SW over LW becomes even more dramatic in the CFMIP2 models analyzed by Zelinka et al. (2012c). Zelinka, M.D., S.A. Klein, K.E. Taylor, T. Andrews, M.J. Webb, J.M. Gregory, and P.M. Forster, 2012c: Contributions of Different Cloud Types to Feedbacks and Rapid Adjustments in CMIP5. Accepted pending minor revisions to J. Climate. [Government of United States of America]	Accepted - the statement has been eliminated now that Zelinka's revised numbers no longer show any tendency toward positive feedback from opacity.
7-615	7	20	6			Omit “at latitudes” as it adds nothing [William Ingram, United Kingdom]	Accepted
7-616	7	20	9	20	9	Typo: "… and possibly to poleward…" [Ralph Kahn, United States of America]	Editorial
7-617	7	20	11	20	18	I was a bit surprised by this paragraph, but maybe this reflects my ignorance with the recent literature in the area. The negative cloud feedback identified by Mitchell and colleagues in their model was a strong one and at least conceptually a robust one. What has changed in the intervening years? Have there been papers that have demonstrated that Mitchell et al's methodology led to a significant overestimate? Is it that other models do not include the mechanism leading to this feedback (how do they partition between liquid and ice phase if not on tempertaure?) and if, as is said on line 18, it is particularly uncertain, could you not at least indicate the likely sign and and potential size? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted - this section has been reworded to clarify these issues. While under simplistic assumptions this mechanism can be important, we now identify the likely main reason it does not appear in GCMs which is that the environment of clouds does not actually warm up on average.
7-618	7	20	12	20	15	These statements depend on assumptions that should be made clear [Jost Heintzenberg, Germany]	Accepted - reworded
7-619	7	20	12			I don't where Mitchell et al. (2010) substantiate this statement [Jost Heintzenberg, Germany]	The statement was based on comparing ice-particle size distributions in the cited work from satellite-based remote sensing with typical effective radii of 10 microns in liquid clouds. This has been reworded to clarify, and the ref has been changed to a specific figure in Donovan 2003 which clearly shows reff as a fn of temperature in ice clouds.
7-620	7	20	12			“so” makes immediate sense if the reader knows/remembers that scattering occurs at the surfaces of the particles, but would it help some readers to tell/remind them? [William Ingram, United Kingdom]	Accepted - done
7-621	7	20	18			It might also be appropriate to cite here the two papers by Ogura et al on the mechanisms of mixed-phase	Rejected - these papers are a bit hard to interpret and

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						feedbacks, cited in Chapter 9, page 74, lines 27-30. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	go into more detail than we can afford here.
7-622	7	20	20	20	47	Please consider the possibility ICE-MICROORGANSM-AEROSOL-CLOUD-FEEDBACK in a future climate: The Arctic low-level clouds have a pronounced influence on the surface energy budget. In summer, a scarcity of CCN concentrations leads to optically thin clouds compared to similar clouds at lower latitudes. These are conditions that maximize effects of changes in CCN and cloud droplets on short-wave radiation. The sources of these CCN are mostly located along the marginal ice zone and north thereof towards the pole. Marine micro-organisms or specificall marine gels (described below, see comment #20) have been demonstrated to be the primary contributors to CCN, and hence an important control on the optical properties. Recent findings Arctic (Bigg et al., 2004; Leck and Bigg, 2005a; Orellana et al., 2011). This implies that the number of CCN and thus number of cloud droplets will be dictated by the number of airborne particles originating in the surface microlayer of the open leads (see comments #14,#20). As bacteria and viruses are probably major players in this marine gel production a corollary is that climate feedback due to temperature-dependent DMS production by phytoplankton is irrelevant (Leck and Bigg, 2007). Marine gels response to the changing Arctic climate is thus key to a possible negative feedback that would slow down the melting of summer sea ice. We know that the immobile ice algae as well as the floating phytoplankton are likely to be strongly affected by changing sea-ice conditions (Wassman and Reigstad 2011). But whereas both generate dissolved organic matter and are hence a potential source of airborne marine gels, their relative importance is not fully understood. The recent report of a sub-ice phytoplankton bloom by Arrigo and colleagues (2012), in conjunction with previous and current observations (see comment #20), strongly suggest increased activity as the Arctic warms if they are found to be a strong contributor of marine gels, phytoplankton might facilitate an enhanced reflectivity of the low-level clouds that help counteract increased ice melt. The melting of sea ice might reduce or even eliminate the habitat of ice algae and it might have indirect effects. The presence of sea ice has prevented, or significantly controlled, wind- driven mixing of the surface layer of the Arctic Ocean. This has kept the floating phytoplankton mostly at the surface. Thinner ice or more open ocean areas would allow the wind to stir the surface ocean, deepen or break the mixed layer, thereby reducing algal growth. If organic matter derived from ice algae was confirmed to be a major source of the microgels throughout the Arctic, future warming might imply reduced supply of CCN and thus very optically thin clouds with enhanced surface warming. On the other hand, ice formation during freeze-up excludes salt brine and other substances, including dissolved organic matter likely assembled as gels. These gels can end up in both the surrounding water and the atmosphere during this crucial period. Clearly, there are too many unknowns at this stage to fully assess the likelihood of a negative feedback involving ice melt, micro-organisms and clouds. Work (comment #20 for references) at Mace Head and Lizard Island in Australia an several other locations outside the Arctic have given indications that similar microcolloidal (marine gels) particles to those observed in the Arctic occur in all the world's oceans and are likely to supply or modify a significant part of the general both primary organic aerosol (POA) and primary biological aerosol particles (PBAP)l in response to changes in ocean surface temperature. REF: Arrigo K R et al. (2012) Science 336 (6087): 1408, doi: 10.1126/ science.1215065. Bigg, E.K., C. Leck and L. Tranvik, 2004, Particulates of the surface microlayer of open water in the central Arctic Ocean in summer, Marin Chemistry, 91, 131-141. Leck, C., and E.K. Bigg, 2005a, Biogenic particles in the surface microlayer and overlaying atmosphere in the central Arctic Ocean during summer, Tellus 57B, 305-316. Leck, C. and K. Bigg, 2007, A modified aerosol–cloud–climate feedback hypothesis. Environmental Chemistry, 4, 400–403, doi:10.1071/EN07061. Orellana M.V., P.A. Matrai, C. Leck, C. D. Rauschenberg, A. M. Lee, and E. Coz 2011, Marine microgels as a source of cloud condensation nuclei in the high Arctic, PNAS, 108 (33): 13612-13617. Wassmann P, Reigstad M (2011) Future Arctic Ocean seasonal ice zones and implications for pelagic-benthic	Partially accepted - the reviewer correctly notes that we currently ignore climate-aerosol-cloud feedbacks, and we now include a short comment on them. However, we do not find any compelling and quantitative evidence that such feedbacks are significant. This is especailly the case in the Arctic, where clouds have a small net radiative effect and are expected to be vastly more sensitive to meteorological than to aerosol changes.

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						coupling. Oceanog 24:220-231. [Caroline Leck, Sweden]	
7-623	7	20	20	25	37	The first sentence gives the impression that the Arctic low-level clouds always give a warming. That is not true, observations show a short period during the summer when they are cooling, see for example Karlsson and Svensson (2010, referrence in Chapter 9) [Gunilla Svensson, Sweden]	Rejected - the sentence already clearly states that there is cooling in the summer but that this is outweighed in the annual mean.
7-624	7	20	20	25	37	The role of Arctic clouds for the surface energy balance, lower stability and radiation in the present climate is discussed for the present climate in CMIP3 models in Svensson and Karlsson (2011 J. Climate) and could be a usefull reference to strengthen this paragraph. [Gunilla Svensson, Sweden]	Rejected - paper is informative but not about the issues discussed in the assessment text
7-625	7	20	21		23	Clarify the immediate radiative effect is meant, not the eventual temperature change [William Ingram, United Kingdom]	Accepted
7-626	7	20	23			"They also cool the atmosphere," As a vertical average over the troposphere? In the free troposphere? [Government of United States of America]	Rejected - this level of detail is unnecessary and not justified given the uncertainties
7-627	7	20	27	20	37	This sections needes to be clarified concerning the use of "Low cloud". Please repalce "low" with "low-level" whenever used. [Caroline Leck, Sweden]	Partially accepted - "low level" does not seem any clearer than "low", but we have hyphenated low-cloud when used as a modifier, for a bit more clarity
7-628	7	20	35	20	37	Regarding the Vavrus et al study: Is this seasonal effect expected to increase in amplitude as Arctic sea ice extent decreases over the next few decades? [Government of United States of America]	Rejected - there is insufficient space to address the possible seasonal cycle changes.
7-629	7	20	36	20	36	“especially during autumn and winter when open water and very thin sea ice increase considerably”. Open water increases during the winter? [Lazaros Oreopoulos, United States of America]	Yes. I added 'the onset of' before 'winter' here to clarify
7-630	7	20	39	20	47	This paragraph seems included to refute a single study, which seems overkill. [Robert Pincus, United States of America]	Accepted - this material has been compressed (it was actually discussing two studies, not one) and now finishes with a section summary.
7-631	7	20	42		44	No it is not consistent, it's the opposite. (I haven't read Gagen & al, but anyone who thinks they can tell what cloud cover was millennia ago from tree rings would have a much harder time convincing me than they seem to have had with you!) [William Ingram, United Kingdom]	Rejected - it is not opposite. GCM studies show more cloud cover in warmer climates, which gives a negative feedback during the summer (but not averaged over the year).
7-632	7	20	43			Does the term 'above studies' refers to the Liu et al. (2008) study mentioned in the previous sentence? It would seem contradictory to the results of the studies mentioned in the previous paragraph- please clarify this. [Government of United States of America]	Accepted - this text has been reworded for clarity.
7-633	7	20	53	21	1	I found this sentence very hard going [William Ingram, United Kingdom]	Accepted - this has been reworded
7-634	7	20	53	21	1	Some word(s) must be missing in this sentence. Should it be "but also the presence of rapid adjustments" ?? [Robert Kandel, France]	Accepted - this has been reworded
7-635	7	20	54	20	54	add "of" between "because the presence" and "rapid adjustments" [Peter Irvine, Germany]	Accepted - this has been reworded
7-636	7	20	54	20	54	the presence of? [Brian Kahn, United States of America]	Accepted - this has been reworded
7-637	7	20	54			"presence of" [Richard Allan, United Kingdom]	Accepted - this has been reworded
7-638	7	20	54			“... presence rapid ...” should be “... presence of rapid ...” [James Coakley, United States of America]	Accepted - this has been reworded
7-639	7	20	54			"presence rapid adjustments"? [Jost Heintzenberg, Germany]	Accepted - this has been reworded
7-640	7	20	54			Insert “of” before “rapid” [William Ingram, United Kingdom]	Accepted - this has been reworded
7-641	7	21	1			Cleaner to replace “discussed in” with brackets? [William Ingram, United Kingdom]	Accepted - this has been reworded

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7-642	7	21	4	21	11	Section 7.2.4.3.6 : Vial et al. (Clim. Dyn, 2012) also assessed the relative spreads of cloud feedbacks and adjustments in CMIP5 models, and found a smaller contribution of cloud adjustments. [Sandrine BONY, France]	Accepted -now noted
7-643	7	21	4	21	11	interesting point on the time scales to reaction (rapid with respect to SST mediated); however, it should be clarified whether the conclusions reached in the other chapters pertain to the rapid reaction or the SST mediated one. This chapter should have a conclusion that includes this point. [Andrea Flossmann, France]	Accepted - this issue is now addressed by a new box in Chapters 8, and new text in Section 7.1.
7-644	7	21	4	21	11	The statement "rapid cloud adjustments....causes less than 20% of their equilibrium radiative feedback" has two problems. First, the subject-verb agreement is wrong. Second, cloud adjustments do not cause radiative feedbacks. The authors seem to have meant to write "...causes less than 20% of their equilibrium climate sensitivity", or "...have 20% as large an effect on climate sensitivity as do feedbacks." Thus, the statement should be re-written. [Government of United States of America]	Accepted - the section is rewritten
7-645	7	21	5	21	11	Plece specify to which forcing the figures of 2.2 K and 1.08K presented here correcpond. [Andrew Ferrone, Germany]	Accepted
7-646	7	21	7	21	7	A bit confusing about the signs in this paragraph and at this line in particular [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted reworded
7-647	7	21	7	21	8	Colman and McAvaney (2011) did indeed attribute cloud adjustment to low cloud, but only in one model. So did Wyant et al 2012. It may also be relevant to note however that Webb, Lambert and Gregory (2012) showed that inter-model differences in cloud adjustments across 11 models over the subtropical oceans were largest in regions of high static stability, and dominated by the shortwave component, consistent with what would be expected for low clouds. They also showed that these were correlated with rapid adjustments in stability. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - we now cite the new Zelinka et al. study, which confirms this result in an ensemble of models using kernel techniques. Both this study and one by Tomassini et al. show that clouds decrease at all levels, such that the low clouds end up having the greatest effect due to their net radiative importance.
7-648	7	21	7	21	8	I think it would be helpful to say more about the physical mechanism of cloud adjustments described in Colman and McAvaney (2011), and also the mechanism proposed by Wyant et al 2012 to explain low cloud adjustment in the SP-CAM. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - the basic stabilization mechanism is now mentioned
7-649	7	21	7			“most of these come” → “mostly” [William Ingram, United Kingdom]	Accepted
7-650	7	21	7			Perhaps it might be clearer to say 'rapid adjustments in a suite of climate models cause less than 20% of their equilibrium radiative response'. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - reworded
7-651	7	21	8	21	11	Strictly speaking, the numbers quoted here represent the uncertainty from CO2 forcing (including adjustments) and feedbacks. As such they include the uncertainty in instantaneous CO2 forcing as well as rapid adjustments. Perhaps it might be useful to additionally note that Webb, Lambert and Gregory (2012) estimate that cloud feedbacks contribute four times as much to inter-model spread in climate sensitivity as cloud adjustments. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - this is now clarified
7-652	7	21	9			Please specify that this is the uncertainty in equilibrium climate sensitivity. [Government of United States of America]	Accepted- this text has been rewritten
7-653	7	21	10	21	10	These values (in K) are meaningless - I think they refer to the double CO2 case, whereas the rest of the chapter uses "sensible" units of W m-2 K-1 [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted- this text has been rewritten
7-654	7	21	11			A link to the rapid response of the hydrological cycle to radiative forcings may be appropriate here. [Richard Allan, United Kingdom]	Rejected - there was a link at the end of the previous paragraph
7-655	7	21	13	22	3	Section 7.2.4.3.7 : It could also be mentionned (e.g. around lines 1-2 of page 22) that cloud feedbacks at work in response to volcanic eruptions can also differ from those at work in climate change (Yokohata et al., GRL, 2005). [Sandrine BONY, France]	Accepted
7-656	7	21	13			Section 7.2.4.3.7: This discussion of the constraints of the feddback by observations misses a discussion of the importance of anthropgenic aersol emissions, which clearly have a n influcence as showns later in this chapter and this needs to be filtered out of the observations. [Andrew Ferrone, Germany]	Rejected - we find no papers in the peer-reviewed literature suggesting any interference between anthropogenic aerosol sources and the short-term

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							natural temperature fluctuations used to attempt to infer observational constraints on cloud feedback. We suspect that the reviewer is confused and believes that these studies used trends, in which case he would have a valid concern, but in fact none of the studies assessed is based on trends. It is now more clearly stated in the text that observed cloudiness trends are mostly inconclusive.
7-657	7	21	18	21	18	Section 7.2.4.3.7 : One approach is to seek observable aspects of present-day cloud behaviour that reveal cloud feedbacks...or some components of cloud feedbacks. [Sandrine BONY, France]	Accepted
7-658	7	21	18	21	34	Section 7.2.4.3.7 : Brient and Bony (GRL, 2012) found a strong linear relationship between the strength of low-cloud radiative effects simulated by the IPSL model in the current climate and the magnitude of the low-cloud response in climate change. The IPSL model produces a climate change cloud feedback in the upper range of CMIP models. Using observational constraints on present-day low-cloud radiative effects, they suggest that the IPSL low-cloud feebdack might be overestimated by 50 percent. [Sandrine BONY, France]	Rejected - the mechanism found in this study is more relevant to discussions earlier in the chapter where it is already cited. We now clarify that this section concerns studies based on natural global climate variability.
7-659	7	21	18	21	34	1. Line 23: "characteristics" is ambiguous. Please be more specific. 2. Clement et al. (2009) found that only one model looked good, but Broccoli and Klein (2010) found, using more complete model output from one of these models (GFDL CM2.1) "better agreement with observations, suggesting that more detailed analysis of climate model simulations is necessary." Clement et al. used total cloud fraction rather than the more appropriate low cloud fraction, or even more appropriately, a satellite simulator-derived low cloud fraction that is more comparable to observations. 3. Another study that can be cited in this paragraph is Zelinka and Hartmann (2011), which showed that tropical high cloud tops shift upwards as the tropics warms interannually, in a manner consistent with the constraints predicted by the FAT hypothesis, just as they do in GCMs in a warming climate. Broccoli, A. J. and S. A. Klein, 2010: Technical comment on "Observational and model evidence for positive low-level cloud feedback". Science, 329, 277-a, doi: 10.1126/science.1186796. 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. [Government of United States of America]	Partially Accepted - we clarify (1), and briefly note the Broccoli and Klein study, but do not say anything here about FAT because this and other mechanisms were already covered in a previous section.We now clarify that this section concerns studies based on natural global climate variability.
7-660	7	21	18	21	34	The new Fasullo and Trenberth paper (Science, 2012) could fit in here. They found a correlation between boreal summer subtropical RH and ECS in CMIP3 models, with most models overestimating the RH, suggesting an underestimate of the cloud feedback. [Karen Shell, United States of America]	Accepted - yes we were going to put that one in, thanks.
7-661	7	21	20			Put Gettelman et al 2012a reference with Yokohama et al reference: CMIP3 results were removed in review (now accepted). [Andrew Gettelman, United States of America]	Accepted
7-662	7	21	22			Remove Gettelman et al 2012a from here: multi model results are not in the final paper. [Andrew Gettelman, United States of America]	Accepted
7-663	7	21	23	21	24	I don't think that Trenberth and Fasullo 2010 do show a correlation between net cloud feedback and characteristics of the Southern Hemisphere storm track. My reading of that paper is that they show a correlation between the net top-of-atmosphere radiation averaged over the entire southern hemisphere and the climate sensitivity. This might suggest a relationship with the Southern Hemisphere storm track and cloud feedback, but that doesn't necessarily follow. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Accepted - reworded accordingly
7-664	7	21	29	21	29	It is not fair to say that Clement et al. (2009) "provided no evidence for such a link." The arugment in the paper is clear: the observed multi-variate, decadal response of low clouds over the Pacific is reproduced by only one model. Since these clouds explain much of the diversity in climate sensitivity in the CMIP ensemble, the model most like the observations is considered most trustworthy. It would be fair to say that this evidence is not	Accepted - this has been reworded to say there is no mechanism, and a comment by Broccoli and Klein is now cited who argue that other models might reproduce the observations too if they were analyzed

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						conclusive. [Robert Pincus, United States of America]	more carefully.
7-665	7	21	31			Change while to whereas [Jost Heintzenberg, Germany]	Accepted
7-666	7	21	36	21	47	See earlier discussion of natural fluctuations as analogues. Note that Dalton and Shell doesn't look at cloud feedbacks, but this paragraph refers to overall climate sensitivity as well as cloud feedbacks. This might be too much work at this point, but an overall discussion of this issue in Ch. 9 might be useful. [Karen Shell, United States of America]	This comment is unclear and should maybe have been addressed to Chapter 9.
7-667	7	21	40	21	40	Lindzen and Choi (2011) has been discredited by Dessler (2011) and Trenberth et al. (2011). Just because those authors are vocal and IPCC attempts to be balanced is no reason to cite an incorrect result. [Daniel Murphy, United States of America]	Rejected - yes we agree but feel that the current wording is adequate, and that it is better to cite these studies and give the key reasons not to accept them rather than ignore them.
7-668	7	21	43	21	44	Inclusion of land-surface processes into the AOGCMs enhances understanding of the cloud-temperature relationship. [Panuganti, C.S. Devara, India]	Rejected - no action suggested by the reviewer and none clear to us
7-669	7	21	44			"...consistent with observations (Dessler, 2010) from which estimated feedback is highly sensitive to dataset and time-period (Masters, 2011)." [Richard Allan, United Kingdom]	Rejected - was unable to find the cited reference
7-670	7	21	49	21	51	I fully agree with the first part of this sentence, but quesiton whether for a method to be "accepted" it should necessarily be shown to work consistently when applied to different climate models. If some of the models are physically unrealistic in their representation of cloud processes and feedback (likely the case given the spread amongst the models), then why require a method to work consistently across different models? Seems like circular reasoning. I would consider softening the importance of the second test. [Norman Loeb, United States of America]	Accepted - this has been reworded to "its assumptions should be appropriately tested in climate models."
7-671	7	21	49	21	51	Perhaps reword this - I understand what the authors are trying to say, but, taken out of context, I think this could be mid-read and mis-understood- I think there is a difference between a feedback having a sound physical basis and it being "shown to work" in climate models. The climate models may not include the appropriate physical mechanisms. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted - this has been reworded to "its assumptions should be appropriately tested in climate models."
7-672	7	21	51	21	52	Needs some editing [Brian Kahn, United States of America]	Accepted
7-673	7	22	5	23	10	Good discussion in Sect. 7.2.4.4. (feedback synthesis) even if it does not mention specifically the low cloud diurnal variation question that is my hobbyhorse. [Robert Kandel, France]	Thanks
7-674	7	22	7	22	8	May add on what timescale this is refering to. [Gunnar Myhre, Norway]	accepted
7-675	7	22	7	22	14	Overall confidence of water vapour/lapse rate feedback being positive should be stated (as it is in fact stated in the executive summary, but not here). This would appear to be at the level of high confidence, given the strongly reinforcing lines of evidence. [Government of Australia]	Accepted
7-676	7	22	7	22	30	Section 7.2.4.4- here is an example of where the Feedback Synthesis provides a very nice summary. The authors could even highlight the significant findings in bold (i.e. net radiative feedback from all clouds is judged likely to be positive). Consider adding similar summaries at the end of each main subsection. [Government of United States of America]	Partially accepted - thanks for the nice comment. We do not wish to highlight anything in bold, but have indeed added synthesis statements to the other subsections .
7-677	7	22	8	22	9	"The water-vapour and lapse-rate feedbacks should be thought…", this needs to be elaborated somewhat. [Chien Wang, United States of America]	Partially accepted - we now point to the section where this is elaborated, but there is no point repeating this in the summary
7-678	7	22	8		9	This sentence would be fine if the conventional forms of these feedbacks were all that is available, but ignores 7-16,57 to 7-17,4. [William Ingram, United Kingdom]	Accepted - new text has been added about this
7-679	7	22	9	22	10	"To estimate a 90% probability range for that feedback, we double the variance of GCM 10 results about the mean to account for possible common errors among models,..." This sounds rather like hand-waving...perhaps the authors could better explain the reasoning here. Was this choice done simply to get better agreement with AR4? [Government of United States of America]	Accepted - no, AR4 did not make an explicit assessment of cloud feedbacks (we removed the reference to this which was misleading). We did this to account for "deep uncertainty". It may look like

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							hand-waving but there is no way to do this except subjectively, which the text is clear about
7-680	7	22	10		11	Giving a best estimate to 3 significant figures, & then the range in brackets as if unimportant, when the range doesn't match to 1 is absurd! Quote the range first & the best estimate afterwards in brackets. [William Ingram, United Kingdom]	Accepted - we now quote these to two sig figs. This is the minimum needed to quanatify the uncertainty (difference between high and low values).
7-681	7	22	12			here observational evidence is mentioned. Contradiction to preceeding chapter that concluded that there is no observational evidence. Please clarify [Andrea Flossmann, France]	Rejected - observational evidence was presented in Section 7.4.2.1.
7-682	7	22	13	22	13	Shouldn't "clouds" be replaced by "lapse-rate" here? [Steven Ghan, United States of America]	Rejected - no, we meant clouds.
7-683	7	22	16	22	16	consistent or consistently? With each other, or feedbacks consistently appear within models? Not clear. [Brian Kahn, United States of America]	Rejected. "consistently" is what is meant, and the sentence seems clear.
7-684	7	22	16	22	30	Lines 16-30: I would submit that the key to cloud cover feedback appears to be related to cloud cover parameterization, which is linked to H2O. [Kuo-Nan Liou, U.S.A.]	Rejected - not sure what change the reviewer wants. Earlier sections note the link between changes in cloud cover and water vapour.
7-685	7	22	17	22	17	Proper English will use "Firstly" instead of "First". [Caroline Leck, Sweden]	Rejected - the reviewer is incorrect.
7-686	7	22	20	22	20	Proper English will use "Secondly" instead of "Second". [Caroline Leck, Sweden]	Rejected - I suggest the reviewer consult an English dictionary.
7-687	7	22	20	22	22	I dont understand this part of the synthesis - when I read 7:18:27-32 I read that there is a near-cancellation of mid-high clouds and the remaining feedback is due to low clouds. Here you seem to say that the mid-high clouds contribute a positive feedback [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted - the reason you don't understand it is because it didn't make sense. We have changed this statement.
7-688	7	22	21	22	22	"more than it increases the greenhouse effect" should be "decreases". If this is supposed to read "increases" then the sentence does not make sense as then the two effects are in the same direction, i.e. Both are positive feedbacks. [Peter Irvine, Germany]	Accepted - but this phrase is now removed
7-689	7	22	22	22	22	Correct to “DECREASES the greenhouse effect”. Any reduction in cloud cover almost always decreases the greenhouse effect. [Lazaros Oreopoulos, United States of America]	Accepted - but this phrase is now removed
7-690	7	22	22			To clarify the text, the authors should consider revising it to read: "decreases the greenhouse effect" [Government of United States of America]	Accepted - but this phrase is now removed
7-691	7	22	23	22	23	Proper English will use "Thirdly" instead of "Third". [Caroline Leck, Sweden]	Rejected
7-692	7	22	23	22	26	While models suggest an expansion of the Hadley Circulation (HC) in a warmer climate, they also suggest a weakening. For some reason, the latter is not mentioned. In any event, if both an expansion and weakening of the HC occurs with global warming, then wouldn't the latter offset some of the positive feedback associated with the expansion in the subtropical regions (i.e., weaker subsidence in descending branches of HC could offset middle and high cloud amount decrease associated with expansion of HC)? [Norman Loeb, United States of America]	Rejected - first, models do not agree on weakening of the Hadley Cell, although they do show a slowing of overturning generally. Second, even if they did, there is no basis for asserting this would change cloud amounts in any particular way. The positive feedback comes from the displacement of the clouds relative to incoming sunlight. If there were some effect to happen not captured by any GCM, this goes into the box of "deep uncertainty" that we treat by inflating the variance of the GCM estimates
7-693	7	22	27	22	28	"...differs significantly among models, explaining a proportion of the range in simulated cloud feedbacks, and lacks…" [Richard Allan, United Kingdom]	Rejected - we are not trying to explain model spread here but identify robust mechanisms.
7-694	7	22	28	22	30	The "small net radiative effect" statement may not be entirely accurate In the five CFMIP2 models analyzed by Zelinka et al. (2012c), the effect of increasing cloud optical depth at high latitudes as the planet warms is fairly large and -- when rapid adjustments are properly accounted for -- the net cloud optical depth feedback is a respectable -0.15 W/m2/K, which is actually slightly larger in magnitude than the positive net cloud amount feedback. Also, is this statement meant to suggest that the optical depth increases are only caused by phase	Rejected - the reviewer is confusing optical depth change with phase change. The optical depth changes globally are near zero, and regionally result mainly from changes to water transport. We hope the revised discussion in 7.2.5.4 helps.

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						transition? There is also an overall increase in cloud water content that contributes to clouds becoming more opaque. Zelinka, M.D., S.A. Klein, K.E. Taylor, T. Andrews, M.J. Webb, J.M. Gregory, and P.M. Forster, 2012c: Contributions of Different Cloud Types to Feedbacks and Rapid Adjustments in CMIP5. Accepted pending minor revisions to J. Climate. [Government of United States of America]	
7-695	7	22	28	22	30	Perhaps the radiative effect is further modified by changes in the lifetime of the cloud. Not sure if there are good references for this, but I think some old papers by Mitchell et al (1989, Science) touch on this. [Brian Kahn, United States of America]	Partially accepted - the discussion of this has been enhanced in 7.2.5.4.
7-696	7	22	28	22	30	See especially line 30 "small radiative effect" - this follows on from my comment at 7:20:11-18 - in the earlier paragraph we were told that this feedback is "highly uncertain" and now we are told that it is "small" - the reader will need to know how you get from "highly uncertain" to "small", as it certainly was not small when first proposed. Again, perhaps there is something in the literature in the intervening years that convincingly shows that the methodology adopted by Mitchell and colleagues led to a significant overestimate of this feedback that I have missed. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted - we have changed this to "small in models," and have added a sentence to the following paragraph reiterating that mechanisms associated with microphysical change are highly uncertain and could be missing from GCMs.
7-697	7	22	33	22	41	Figure 7.10 mentions a narrowing of the ITCZ, nowhere discussed in the text. Please add a discussion in this respect somewhere. Also, the TS(p. 45 line 7) states that the tropical regions broaden. Seems in contradiction to the narrowing of the ITCZ, right? [Andrea Flossmann, France]	Accepted - the ITCZ is now discussed in 7.3.52.
7-698	7	22	43	22	46	Section 7.2.4.4 : Nice synthesis ! One comment, however, about the statement : « some GCM biases suggest positive feedbacks are underestimated » (lines 45-46) : I couldn't find where this idea was discussed in the previous sections. Actually, the over-estimate of the low-cloud radiative effects in current climate models might tend to over-estimate the positive feedbacks (Brient and Bony, GRL, 2012), despite their under-estimate of the low-cloud fraction. [Sandrine BONY, France]	Accepted - this statement has been changed to say that some observational studies suggest that GCMs with strongest positive feedback are more realistic, and the relevant section is now cited.
7-699	7	22	45	22	46	These statements should have references. [Government of United States of America]	Accepted - All statements in the synthesis refer back to the previous sections. We now refer back to section 7.2.5.7 for one of them; the others should be obvious
7-700	7	22	45		46	Where does this clause about under-estimation come from? [William Ingram, United Kingdom]	Accepted - this statement has been changed to say that some observational studies suggest that GCMs with strongest positive feedback are more realistic, and the relevant section is now cited.
7-701	7	22	55			It is stated that positive feedback found in GCMs comes mostly from mechanisms now supported by other lines of evidence - and therefore a mean from GCMs is the best feedback estimate. Are negative feedbacks also supported by evidence? If not, how is a mean of all GCMs judged to be the best estimate? [Government of United States of America]	Irrelevant - the only robust negative feedback is the Planck response, which is completely uncontroversial and needs no further support. It is not really worth stating this here in our view, so no change made .
7-702	7	22	57	22	57	Line 57: Using this line of argument, one can also stipulate that there is no accepted basis to support individual GCMs (since tuning is the key to success). [Kuo-Nan Liou, U.S.A.]	No response needed. This is an issue taken up by Chapter 9.
7-703	7	22	57			This value of 0.8 seems high from looking at Fig. 7-9. Is this the correct value? [Government of United States of America]	Accepted - no it was not. This has been fixed.
7-704	7	22	58	22	58	"must be" - I dont really understand the "must be" but I certainly agree that, as long as we cannot "discredit" (may be an unfortunate use of words) models, it is the safest assumption. Perhaps reword? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Partially accepted - this has been reworded.
7-705	7	23	1	23	2	Regarding the phrase, "(particularly involving thin high clouds or low clouds)" ...citations for the importance of these would be helpful...Turner et al 2007 BAMS for low-level; Comstock/Ackerman 2002 JGR for high-level? [Government of United States of America]	Partially accepted - these points are already made in the previous sections.
7-706	7	23	4			Surely “spread” means range, not standard deviation? [William Ingram, United Kingdom]	Accepted - we have revised this

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7-707	7	23	6	23	7	Section 7.2.4.4 : This range of cloud feedback parameters could be translated into a range of climate sensitivity (e.g. as done by Dufresne and Bony, 2008). [Sandrine BONY, France]	Rejected - Since we are basically ratifying the GCM spread it will be evident that we are endorsing a larger spread of climate sensitivities; it opens a can of worms if we give a range here, before the report has taken on board other evidence to constrain the climate sensitivity.
7-708	7	23	14	23	17	Can you give a reason why changes to the hydrological cycle due to land use change are not discussed here? Is there insufficient literature, insufficient evidence, consensus that that effects of land use change are negligible or is it that its covered elsewhere - or other? [European Union]	Taken into account. Land use changes are discussed briefly in Chapter 12, and we have added a cross-reference.
7-709	7	23	16	23	17	The statement, "The impact of water vapour sources from combustion at the Earth's surface is thought to be negligible." might deserve a reference. [Government of United States of America]	Noted, but we are not aware of a reference for this statement, although it is easily backed up by a calculation.
7-710	7	23	17	23	17	Not discussed because they are discussed in another chapter (in which case state which chapter)? Because it has been concluded that they are not important (in which case cite a reference)? Or because although they might be important no one has looked at this question yet? Whatever the answer, give a reason for not discussing this topic. [Anthony Del Genio, United States of America]	Taken into account. Land use changes are discussed briefly in Chapter 12, and we have added a cross-reference.
7-711	7	23	19	23	19	I do not understand why the direct emission of water vapour by aircraft is not considered in this chapter - the deposition in to the stratosphere is as much a source of (probably rather small for the present fleet) forcing as irrigation [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account. We do now report a RF due to stratospheric water vapour emitted by aircraft. However this is a very small term (smaller than what's expected from irrigation) and we can't devote much space to it.
7-712	7	23	19	23	51	There have been some new developments in the estimation of contrails and contrail induced cirrus cloud properties. They should be included. The study authors (for search) are: P. Minnis, J. Penner, P. Yang and others who are doing work on Aviation effects on climate (ACCRI Program). [Government of United States of America]	taken into account. We assume the reviewer refers to the study by Yi et al. (Simulation of the global contrail radiative forcing: A sensitivity analysis, GRL, 2012).
7-713	7	23	21	23	22	The -40°C limit is currently often used as a threshold to detect where contrails might occure, and gives good results for the current fleet, however it does not represent a physical thereshold. To the determine the physical threshold temperatures for formation of contrails, the Schmidt-Applemen thermodynamic criterion has to be fulfilled as described in Schumann, U., 1996: On conditions for contrail formation from aircraft exhausts. Meteorol. Z., N.F. 5, 4−23. [Andrew Ferrone, Germany]	taken into account through a small change in words.
7-714	7	23	28	23	29	I miss a discussion of aviation aerosol effects on clouds and the hydrological cycle. Hence, I suggest to state that aviation-induced aerosol-cloud changes have been identified in model studies but cannot be quantitatively assessed so far. Line 28-29 state correctly that aerosol emitted within the aircraft exhaust may also affect high-level cloudiness, and refer to Section 7.4.4 for further discussion. However section “7.4.4. Adjustments in Cold Clouds” does not seem to discuss aerosol-cloud interaction with aerosols from aircraft. Section 7.5.2, page 7-48, includes a few references on aerosol impact on cirrus, but there is no link to aviation impact in this section. In fact the reference to Hendricks et al. (2005) might be better replaced by a reference to Hendricks et al. (JGR, 2011) [Ulrich Schumann, Germany]	partly taken into account. The reviewer is correct that section 7.4.4 does not provide further discussion. However there is very little material available in the literature to be assessed. Aerosol effects on clouds from aviation are therefore not discussed in details.
7-715	7	23	29	23	31	What was AR4's conclusion on contrails and their impact on surface temperature/diurnal temperature range? Previous studies linked contrails to changes in diurnal temperature range but more recent modelling studies have shown that this is unlikely to be as great an effect as previously thought. Given the prevalence of aviation and its likely increase, and its importance to society can this issue be given a little more detail. [European Union]	partly taken into account. The conclusion was rephrased using a confidence level statement. However we cannot give much more detail given length limitation and the relatively small RF by contrails and contrail-cirrus. AR4 already stated that studies linking contrails to change in DTR were weak and not substantiated quantitatively.
7-716	7	23	29			Clearer to add “so” after “and”? [William Ingram, United Kingdom]	partly taken into account. Sentence was rephrased.
7-717	7	23	30			Omit “the assessment”, or make it useful as “XX's assessment” [William Ingram, United Kingdom]	partly taken into account. Sentence was rephrased.

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7-718	7	23	31			Regarding the phrase, "diurnal temperature range" should this read "diurnal surface temperature range"? [Government of United States of America]	accepted. this is correct. Corrected.
7-719	7	23	33	23	33	This paragraph, culminating in the final RF estimate, is hard to follow and seems to rely mostly on an ill-justified scaling (i.e. a reference is needed for the 22% scaling) of earlier reviews, rather than a proper consideration of the recent literature. The more recent Lee et al. (2010, Atmospheric Environment doi:10.1016/j.atmosenv.2009.06.005) paper lists several recent papers, but additionally their central estimate is 12 mW m-2 for 2005. For the 22% scaling one would end up at 15 mW m-2 (which would round down to 10 if that is what is being done here). Some note is needed to say that this simple linear scaling may not be robust, as it assumes an equal likelihood of persistent contrail formation per km flown, and this is a little unlikely given changing geographical distributions of flights. Other recent work (e.g. Fichter et al (2011 http://dx.doi.org/10.1016/j.atmosenv.2010.11.033) seems ignored, while Burkhardt and Karcher (2011 - which is cited at line 46) come up with only 4.3 mW m-2 for line shaped contrails for 2002 emissions, while Voigt et al (2011 doi:10.1029/2011GL047189) come up with a quite different (and less rigorous in my view as the authors scale to young contrails (less than 10 minutes!) whether they are persistent or not) value. Rap et al (JGR 10.1029/2009JD012443) come up with values in the range 4 to 8 mW m-2 for 2002 values and scaling RPK's from http://www.airlines.org/Pages/Annual-Results-World-Airlines.aspx gives you 13 mW m-2. So there is a great diversity of values in the recent literature which has not been assessed here, and I think scant support for the final central value that has been adopted, which I believe should be somewhat lower and nearer 10 Wm-2. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	taken into account. More recent estimates are now integrated in the assessment. The rationale for getting to the final estimate is provided in supplementary material to the chapter because of length limitations.
7-720	7	23	33	23	51	This section mixes RF and AF. Is this intentional or accidental? Please clarify. [Andrea Flossmann, France]	Taken into account but explanation has to be short because of page limit. This is indeed intentional. AF (now ERF) estimates include rapid adjustments on cirrus clouds.
7-721	7	23	33	23	51	These paragraphs are confusing and need to be rewritten to provide a clearer review of estimates of RF/AF for contrails. E.g. lines 33 and 34: "…, and quoted Sausen et al. (2005) to update the 2000 forcing for aviation-induced cirrus (including linear contrails) to +0.03…". Also, both AF and RF are mentioned in these paragraphs without any explanation of why there are differences (aside from including AICs in the AF). [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account but explanation is short because of page limit. The inclusion of contrail-cirrus in the AF estimate and the associated decrease in natural cirrus are the main reasons for the difference between RF and AF.
7-722	7	23	33			,and quoted: add space [Urs Baltensperger, Switzerland]	Editorial, Agreed.
7-723	7	23	35			W m-2 . Delete space [Urs Baltensperger, Switzerland]	Editorial, Agreed.
7-724	7	23	41	23	45	This discussion seems to include the hidden assumption of what is and is not a "cloud." As shown in Long et al. (2009), the decadal increase in clear (i.e. cloudless) sky downwelling SW reaching the surface for the continental US is most likely attributable to ice crystal increases in the upper troposphere associated with increased commercial air traffic, not the documented decreases in aerosol loading. Our historical definition of "clear sky" does include up to an optical depth of 0.15 - 0.2 of primarily sub-visual cirrus (Dupont et al., 2008). But it is well known that satellites have difficulty detecting these above the background, and these and their influences are included in the "clear-sky" part of the calculation of radiative forcing. Thus it seems some discussion of "clear-sky" anthropogenic induced climate change should be included. Dupont J.C., M. Haeffelin, and C.N. Long (2008): Evaluation of cloudless-sky periods detected by shortwave and longwave algorithms using lidar measurements, GRL 35(10), doi:10.1029/2008GL033658. Long, C. N., E. G. Dutton, J. A. Augustine, W. Wiscombe, M. Wild, S. A. McFarlane, and C. J. Flynn (2009): Significant Decadal Brightening of Downwelling Shortwave in the Continental US, JGR, 114, D00D06, doi:10.1029/2008JD011263. [Government of United States of America]	noted but no change in text is made. The attribution to changes in downwelling SW/LW to commercial air traffic in the Long et al study is speculative and presented as such in the paper. More precisely Long et al do not attribute the observed brightening to commercial air traffic, but speculate that cirrus haze may have contributed against the decrease in aerosol loading. This study does neither invalidate nor strengthen the assessment made in section 7.2.5.
7-725	7	23	41	23	51	The impact of aviation contrails and contrail-induced cirrus in the presence of clear- and cloudy-sky conditions, on regional climate needs to be assessed in the radiative forcing estimations. [Panuganti, C.S. Devara, India]	rejected. Due to page limit we cannot discuss the geographical distribution of the contrail RF. Moreover it is not within the scope of this chapter to discuss the regional response to forcing mechanisms.

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7-726	7	23	41	23	51	Section 7.2.5.1 assesses the combined contrail and contrail-induced cirrus AF to be +0.05 (-0.02 to +0.15) W m-2. This result is o.k., though the upper bound has only vague support. An upper bound of 0.1 instead of 0.15 W m-2 seems better supported by the papers cited. [Ulrich Schumann, Germany]	partly taken into account. The rationale for the upper bound is now provided in supplementary material.
7-727	7	23	41			One "estimates" too many in this line. [Adrian Simmons, United Kingdom]	Editorial.
7-728	7	23	44	23	44	Did Schumann and Graf really estimate the adjusted forcing? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	partly taken into account. Schumann and Graf estimate the ERF in that they consider changes in cloudiness, but they do not consider rapid adjustments.
7-729	7	23	47	23	47	On page 55 (column 2) Burkhardt and Karcher give a 2002 value of 37.5 mW m-2 (including, as here, contrails and contrail cirrus), so I am not sure where the +0.03 Wm-2 comes from. And scaling this value to 2010 one gets 0.06 W m-2. So it is again unclear to me where these numbers come from. Note also that the available (although as yet quite scant) literature points to a rather low efficacy of contrails indicating that the AF is probably somewhat smaller than the RF estimate (see e.g. Rap et al JGR doi:10.1029/2010GL045161) [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	partly taken into account. The rationale for the estimates is now provided in supplementary material. We have used the estimate from Burkhardt and Karcher that includes the reduced natural cirrus.
7-730	7	23	53	23	53	Line 53: I would submit that any discussion on “Irrigation-Induced Cloudiness” is not credible based on the fact that the linkage between H2O and cloud formation is local and could not be resolved by a regional model. In the event such a model exists, please explain. [Kuo-Nan Liou, U.S.A.]	Rejected. The reviewer's assertion that regional models do not qualify to investigate regional changes to water vapour and clouds is not convincing. Models parametrize cloud processes as extensively discussed in Section 7.2. We claim only "low confidence" in the result, due in part to concerns about the ability to model the effect.
7-731	7	23	55	23	23	Does "from irrigation" include land use and related evaporation changes? [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	no, and this is stated explicitly in the chapeau of section 7.2.5.
7-732	7	23		23		Subsection 7.2.5.1: A recent highly relevant reference missing in this subsection is "Simulation of the global contrail radiative forcing: A sensitivity analysis" by Bingqi Yi, Ping Yang, Kuo-Nan Liou, Patrick Minnis, and Joyce Penner, submitted to Geophysical Research Letters. [Lazaros Oreopoulos, United States of America]	accepted. The paper is now cited.
7-733	7	24	9	24	9	aerosol particles (aerosols): delete (aerosols), is redundant [Elisabetta Vignati, Italy]	Partly taken into account. A footnote has been added to section 7.1 to explain our use of the term "aerosols". We have deleted (aerosol particles).
7-734	7	24	9			Strike: "(aerosol particles)"; not synonymous. [Stephen E Schwartz, United States of America]	Partly taken into account. A footnote has been added to section 7.1 to explain our use of the term "aerosols". We have deleted (aerosol particles).
7-735	7	24	16	30	18	…and elsewhere... AERONET is clearly the “gold standard” for aerosol properties, particularly when comparing with space-borne remote sensing data. Nonetheless, I suspect AERONET presents a “cloud-free” only view of aerosols. Kaufman et al., IEEE Trans. Geos. Rem. Sens. 43, 2886 (2005) and Kaufman et al., GRL, 33, L07817, doi:10.1029/2005GL025478 (2006) began to explore the sensitivity of AERONET extinction measurements as the stringent cloud screening was relaxed. Some of the signals he found were expected based on the space-borne observations finding changes in the aerosol properties with the proximity to clouds. AERONET provides a biased view particularly of absorbing aerosols, for which the sky has to be severely clear and aerosol optical depths have to be rather large in order to obtain meaningful retrievals of the aerosol properties. Unfortunately, I know of no other credible studies testing the sensitivity of the AERONET data to cloud screening thresholds. Even though AERONET is rightfully considered the “gold standard,” some mention of this “cloud-free” bias should be clearly stated. Comparisons of model results and space-borne observations also need to be interpreted with this bias in mind. While I understand that some of the satellite comparisons are stringently screened when comparisons are made, I’m less sure about the model-AERONET comparisons that have appeared in the literature. [James Coakley, United States of America]	Taken into account. AERONET optical depth products are stringently cloud screened and hence represent cloud-free conditions. We are aware that AERONET-derived aerosol absorption retrievals require absolutely clear-sky conditions and large optical depths (AOD>0.4). Related text has been slightly revised to mention this "cloud-free" bias.
7-736	7	24	20	24	26	PBAP not listed as fraction of the atmospheric aerosol. The term dust is not defined. The term dust is very soft. Should it be mineral aerosol? [Ruprecht Jaenicke, Germany]	Editorial. Accepted. The PBAP is added, and the dust is defined as mineral dust

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7-737	7	24	20	24	31	The use of "sea spray" must be defined: Blanchard and co-worker (Blanchard, 1971; Blanchard and Syzdek, 1988) have long advocated that a significant proportion of a remote oceanic aerosol is derived from bubble bursting (film – and jet drops), resulting most commonly from entrainment of air induced by wind stress at the air-water interface, but also from non-wave mechanisms (see comment #7). A process usually referred to as sea spray, and commonly thought of as being pure sea salt particles. However, in this process, bubbles scavenge not only sea-salt (NaClaq) but also microorganisms and organic compounds. Please avoid using sea spray only refereing to sea-salt particles. Use sea spray to be common to both those made up of sea salt (NaClaq) and organics. Call them primary marine nascent particles. If organic they should be referred to as both primary organic aerosol (POA) and primary biological aerosol particles (PBAP), as both are soursed from the ocean surafce. REF: Blanchard, D. C. 1971. The oceanic production of volatile cloud nuclei. J. Atmos. Sci., 28, 811–812.Blanchard, D. C. and Syzdek, L. D. 1988. Film drop production as a function of bubble size. J. Geophys. Res., 93, 3649−3654. [Caroline Leck, Sweden]	Taken into account. We defer the discussion of sea spray to the next subsection.
7-738	7	24	20	34	39	In all of section 7.3, insufficient distinction is made between aerosol number and aerosol mass. While this appears as "the size distribution" in places, the word "concentration" should never be used without "number" or "mass" (or even "surface area") to modify it. This is especially important because the advances in understanding of the particle number budget and lifecycle have been extensive since the 4th Assessment. One example of how important this is is the effect of BC mitigation on climate, when one consideres both the aerosol-cloud and aerosl-radiation interactions. [Neil Donahue, United States of America]	Accepted. It was already the case for most occurrences of the word "concentration". Other occurrences have been qualified.
7-739	7	24	20		31	Regarding the section where Figure 7.11 is introduced and p7-122 where Figure 7.11 is located, there are some aspects worthy of discussion: 1) Gases with low volatility do not originate necessarily from gases with high volatility, the way it is drawn now makes folks think that there is a chain process going on. But really there is not. The emissions and depositions of gases of low volatility should be parallel to those of gases of high volatility, because they are independent. Then there are interactions between these two as the authors wanted to show. 2) Free radicals like OH, HO2, NO3, are very reactive and initiating many of the reactions of gas and particle transformations, yet they are not included in this chart. 3) In the green box of processed aerosols, in addition to size, composition, mixing state, phase, there is also particle shape and morphology. 4) Also, the processed aerosol green box is confusing, because the properties described in the () exist for the primary particles too. Are the secondary particles not processed particles? Do the authors want to illustrate that the aerosol properties got changed in the “processed aerosol” box? 5) The captions indicate that two-way interactions are increasingly recognized, why not show these? 6) Ice nuclei can be shortened to IN as the abbreviation was introduced in the text. [Government of United States of America]	Partly taken into account. There is a limited amount of information that can be put on such a diagram. We have changed "processed aerosols" to "aged aerosols". An additional arrow was added for direct emissions of low-volatility gases. Shape and morphology have been added.
7-740	7	24	22	24	23	(such as sulphate, nitrate, sea salt) please add ammonium, such as sulfate, nitrate, ammonium, sea salt, same as Line 26 [Junying Sun, China]	Accepted.
7-741	7	24	22	24	25	The words 'sea salt' and 'sea spray' are used. It is better to define the differences between these two terms here itself instead of explainations given in line Nos. 36-37 on page 7-26 [Government of India]	Taken into account. We refer to sea salt here now.
7-742	7	24	23	24	23	(section 7.3.1.1) “…black carbon (BC, a distinct type of carbonaceous material formed from the incomplete combustion of fossil and biomass based fuels), …” Black Carbon (soot) is formed from incomplete combustion at hot, air-starved conditions. Simply incomplete combustion is not adequate, there has to be lack of oxygen and temperatures higher than 1000 °C. Otherwise smoldering – another form of incomplete combustion – would also be included, but smoldering certainly does not produce any soot (BC) at all. [Erik Swietlicki, Sweden]	Added "under certain conditions". A more complete definition is provided in Section 7.3.3.
7-743	7	24	23	24	23	.. (also termed organic carbon or organic aerosol)… they are not the same, and the two terms would generate confusion, delete the phrase in the brackets [Elisabetta Vignati, Italy]	Accepted.
7-744	7	24	23	24	24	We find that this definition of BC is not informative enough: "black carbon (BC, a distinct type of carbonaceous material formed from the incomplete combustion of fossil and biomass based fuels)". Please define more explicit; "a distinct type of carbonaceous material". [Government of NORWAY]	Taken into account. A more complete definition is provided in Section 7.3.3.
7-745	7	24	25	24	30	The sentences should be rephrased like that: Dust, sea spray and BC are introduced into the atmosphere as primary particles. In the present-day atmosphere the majority of BC, sulphate and nitrate come from	Partly taken into account. The paragraphs was modified slightly to make it clearer that we

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						anthropogenic sources, whereas sea spray and most mineral aerosol is of natural origin and it is influenced by both natural and anthropogenic sources of volatile organic compounds. [Elisabetta Vignati, Italy]	discussboth primary and secondary OA here.
7-746	7	24	29	24	29	Replace "is" with "are", [Steven Ghan, United States of America]	Editorial. Accepted.
7-747	7	24	29	24	30	Comment also here on the anthropogenic versus natural sources of primary OA. [Larry Horowitz, United States of America]	Accepted. The sentence was modified.
7-748	7	24	31	24	31	I think the reference here should be to Table 7.3, not Table 7.2. [Anthony Del Genio, United States of America]	Editorial. Accepted.TSU has made a mistake in renumbering tables in the SOD.
7-749	7	24	31	24	31	Second reference, at end of line, should be to Table 7.3, not Table 7.2. There appears to be no Table 1. [Leo Donner, United States of America]	Accepted. TSU has made a mistake in renumbering tables in the SOD. Table 1 was in the exec summary.
7-750	7	24	31	24	31	rephrase as: summarized as well as the characteristics and role of the main aerosol species in table 7.3 [Elisabetta Vignati, Italy]	Rejected. Clear and correct as it is.
7-751	7	24	39	24	44	Table 7.2 shows the result from an array of emission estimates of mass. Recent advances conclude that it is the properties, including morphology and state of mixture, of the individual airborne particles at a given size or within a given size range which are of importance of the model estimates on radiative forcing by aerosols and not just some integral or average property over an undefined size spectra and large number of particles as would be determined by bulk analysis. What have we learnt? How will the bulk mass numbers be converted into an aerosol property unit valid for individual particles? [Caroline Leck, Sweden]	Noted but no change is made. The sentiment of the comment is accepted. However, emissions by mass is the most stratightforward way of conveying infomation. The issues brought up by the reviewer are discussed in the forthcoming sections.
7-752	7	24	42	24	42	Line 42: Has "BVOCs" been defined previously? (See Page 7-27, Line 4.) [Kuo-Nan Liou, U.S.A.]	Editorial. Accepted.
7-753	7	24	42	24	43	At this point in the chapter, the acronyms BVOC and DMS have not yet been defined and thus should be here. In fact, DMS and CCN even appear on p. 7-4 in the Executive Summary without being defined. [Anthony Del Genio, United States of America]	Editorial. Accepted.
7-754	7	24	49	24	50	Table 7.3: Strong objection against the statement that …."PBAP", which are treated by analogy to other coarse mode type" which implies that PBAP consist only/mainly in the coarse fraction. Is only the terrestrial areas considered? However, in the marine environment the surface microlayer (SML) has shown to contain particulate matter in sizes below a micrometer and in large concentrations (109 ml-1 or more) with modal diameter sizes of 10 nm in some samples with 40 nm up to about 500nm dominating. The matter including marine colloidal gels (see comment #20 below) and micro-organisms (viruses and bacteria) all showed to form a substantial part of the submicrometer aerosol in the above air (Bigg and Leck, 2008; Leck and Bigg, 1999;2005a;2005b;2008). REF: 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. Leck, C., and Bigg, E. K. (1999). Aerosol Production Over Remote Marine Areas—A new Route. Geophys. Res. Letters 26:3,577–3,581. Leck, C., and E.K. Bigg, 2005a, Biogenic particles in the surface microlayer and overlaying atmosphere in the central Arctic Ocean during summer, Tellus 57B, 305-316. Leck, C., and E.K. Bigg, 2005b, Source and evolution of the marine aerosol—A new perspective, Geophys. Res. Lett., 32, L19803, doi:10.1029/2005GL023651. Leck, C. and K. Bigg, 2008, Comparison of sources and nature of the tropical aerosol with the summer high Arctic aerosol, Tellus, 60B (1), 118–126,doi:10.1111/j.1600-0889.2007.00315.x. [Caroline Leck, Sweden]	Taken into account. We have revised the PBAB part in section 7.3.2. We dow refer to terrestrial PBAP when appropriate.
7-755	7	24	49			what are AeroCom models? [Andrea Flossmann, France]	Taken into account. A reference was added to the AeroCom models.

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7-756	7	24	52			Please add ASCOS-2008, Arctic Summer Cloud Ocean Study (www.ascos.su.se, which) is an IPY project under the AICIA- IPY umbrella and an endorsed SOLAS project. No other effort ever than the this Swedish icebreaker based program integrating the lower atmosphere/upper ocean processes with linkages to marine microorganisms relevant to the formation of low-level clouds, north of 80° has been performed. [Caroline Leck, Sweden]	Noted but no change is made. The link provided is broken and no reference is provided. This paragraph is not meant to list all past field campaigns.
7-757	7	24	54	25	6	EUCAARI/EUSAAR studies e.g. Asmi A et al., Kulmala et al., ACP, EUCAARI overview should be added [European Union]	Editorial. Accepted.
7-758	7	24	57	24	58	The reference to '(ICARB 2008 and references there in)' is incorrect. It should be corrected as '(ICARB, Moorthy et al., 2008; Lawrence and Lelieveld, 2010 and references there in)'. The relevant references are given a few rows below [K KRISHNA MOORTHY, INDIA]	Taken into account. Moorthy et al. Has been spelled out. We have not added a second reference though.
7-759	7	24		25		The summary of the paragraph describing past field studies is fairly limited. Many campaigns are not mentioned. Also, for the campaigns that are mentioned, not all key references are provided. For example, the MILAGRO study, minimally, the L.T. Molina overview paper should be cited (http://www.atmos-chem-phys.net/10/8697/2010/acp-10-8697-2010.html), Molina,L.T., Madronich,S., Gaffney,J.S., Apel,E., deFoy,B., Fast,J., Ferrare,R., Herndon,S., Jimenez,J.L., Lamb,B., Osornio-Vargas,A.R., Russell,P., Schauer,J. ., Stevens,P.S., Volkamer,R., and Zavala,M.: An overview of the MILAGRO 2006 Campaign: Mexico City emissions and their transport and transformation, Atmos. Chem. Phys., 10, 8697-8760, doi:10.5194/acp-10-8697-2010, 2010. [Government of United States of America]	Rejected. We can not mention all of the field campaigns. The text makes it clear that it is just a selection.
7-760	7	25	1	25	6	The pole-to-pole sampling strategy of the HIPPO campaigns provides excellent constraints for global modelling, worth adding to this list: e.g. Schwarz et al., GRL, (2010). [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Rejected. HIPPO is mentioned later on so we'd rather not cite them all here.
7-761	7	25	3	25	4	(section 7.3.1.2) "…the Amazonian Aerosol Characterization Experiment 2008 (AMAZA-08, Martin et al., 2010a)" The acronym is AMAZE. [Erik Swietlicki, Sweden]	Accepted. Changed.
7-762	7	25	4			Section 7.3: Please remove: and [Government of Poland]	Rejected. Text is correct as it is.
7-763	7	25	5			Section 7.3: Please add: and Maritime Aerosol, Clouds and Radiation Observation in Norway (MACRON, Markowicz et al.., 2012) [Government of Poland]	Rejected. There are too many campaigns and we cannot mention all of them. The text is clear that this is only a selection.
7-764	7	25	6			Please add the EUCAARI campaign to the list of major regional campaigns (Kulmala et al., Atmos. Chem. Phys., 9, 2825-2841, 2009). [European Union]	Editorial. Accepted.
7-765	7	25	8	25	22	Only studies related to aerosol mass have been mentioned, also aerosol number size distribution climatology should be mentioned as well as aerosol hygroscopicity, since number and hygroscopicity are much more important in climate point of view than mass [European Union]	Rejected. This paragraph is specifically about aerosol mass concentration. Systematic measurements of aerosol number size distribution and aerosol hygroscopicity are still limited in many regions of the world but some important results have been assessed in 7.3.3.2 and 7.3.3.3, respectively
7-766	7	25	12	25	22	Since particle optics and effects on clouds are most sensitive for particles between 0.1 and 1 micron, it would better here to discuss concentrations of such particles rather than PM10 microns. At least PM2.5. If that is not feasible, at least acknowledge the fact that PM10 is not the most appropriate measure for climate. [Steven Ghan, United States of America]	Noted, but no change is made here. There is no single aerosol parameter that is the most appropriate for climate. This figure is there to show a climatology of the aerosol in different regions. Aerosol chemical composition data from fine particles, such as PM2.5 or PM1, are very limited so far in various regions of the world. Moreover for sulfate, nitrate, ammonium product, EC and OC, their concentrations are almost the same in both PM2.5 and PM10. Yes, the ACI are most sensitive for fine particles, but ARI relate both to fine and coars particles. Even for the coarse mineral aerosol, CCN activation capacity of mineral aerosol will be enhanced through heterogeneous chemical

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							reaction with polluted gases. This is a very hot topic, especially in Asian polluted areas.
7-767	7	25	14	25	15	In the rural U.S. and South America….in the Figure 7.12 for rural N. America it is SO4 the major contributor not OC; also in Oceania OC is the major contributor but this is not mentioned. [Elisabetta Vignati, Italy]	Accepted. The text has been modified.
7-768	7	25	14			It appears that in Fig.7.12 that OC is a close second in rural U.S., but does contribute the largest fraction in the Urban U.S.- should this be 'urban U.S. and South America' ? [Government of United States of America]	Accepted. The text has been changed.
7-769	7	25	15	25	15	… OC contributes the largest mass(?) fraction … Also elsewhere in this and the following parts of the aerosol section clearly specify if you mean aerosol number, mass or any other aerosol property when talking about concentrations or fractions. [Ottmar Möhler, Germany]	Accepted. Yes, it is mass fraction in these two regions. The "mass" has been added in the text.
7-770	7	25	15	25	16	Here the use of OC is wrong. It should be organic aerosol (OA). [European Union]	Taken into account. Most of data used here are OC, some are OA. We converted all OA data into OC for easy camparisons. This has been added to the figure caption.
7-771	7	25	19	25	19	The term BC is reported here while in the figure the measurements regard EC and not BC. Text and Figure should be consistent. [Elisabetta Vignati, Italy]	Accepted. BC has been changed to EC for consistency, and a small note has been added.
7-772	7	25	19		21	south Asia is missing from the regions listed. [Umesh Kulshrestha, India]	Accepted. South Asia has been added into the new diagram
7-773	7	25	21	25	22	Object to the statement that sea salt aerosol can be dominant at oceanic remote sites (see comment #19 below). Agree with the number 50-70% BUT it applies NOT to the submicromer aerosol, but to supermicron mass YES! [Caroline Leck, Sweden]	No change made. Larger than 50% can be considered as a dominant fraction, and this paragraph is about aerosol smaller than 10 micron, including submicron and supermicron mass.
7-774	7	25	24	25	53	See comment #13 which also apply also units of mean mass surface concentrations. [Caroline Leck, Sweden]	See our reply to comment #13 by the same reviewer.
7-775	7	25	25	25	53	Figure 7.12: increase size of the stamps; put references in main text, as this caption is unreadable [Andrea Flossmann, France]	Partly taken into account. Figure readibility has been improved. However references are left in the caption, which is where they belong.
7-776	7	25	56			Section 7.3: Please add after 2001: Smirnov et al.. 2009; Smirnov et al.., 2011), POLAR-AOD network (Mazzola et al.., 2012), [Government of Poland]	Partly taken into account. There are many AOD networks, and we cannot mention all of them. The text has been made clear that the list is not exhaustive.
7-777	7	25	57	25	57	other ground-based networks, add references such as PHOTON, CARSNET etc. such as Che, H., et al. (2009), Instrument calibration and aerosol optical depth validation of the China Aerosol Remote Sensing Network, J. Geophys. Res., 114, D03206, doi:10.1029/2008JD011030. [Junying Sun, China]	Accepted. The Che et al reference was added. The text has been made clear that the list is not exhaustive.
7-778	7	25	57			(Two reviewers made this comment) Aircraft instruments also provide AOD. Also, MODIS, MISR and POLDER were not dedicated to aerosols as they provide measurements for of many other constituents and various applications though they are more advanced than AVHRR or TOMS. Therefore, the authors might consider changing the sentence to "…other ground-based networks, aircraft instruments (e.g. Russell et al., 2007; Rogers et al., 2009), and a number of satellite-based sensors. Retrievals from satellite instruments such as ..." The references are: Russell, P. B., Livingston, J. M., Redemann, J., Schmid, B., Ramirez, S., Eilers, S. A., Kahn, R., Chu, A., Remer, L., Quinn, P. K., Rood, M. J., and Wang, W.: Multi-grid-cell validation of satellite aerosol property retrievals in INTEX/ITCT/ICARTT 2004, J. Geophys. Res., 112, D12S09, doi:10.1029/2006JD007606, 2007. and Rogers, R. R., Hair, J. W., Hostetler, C. A., Ferrare, R. A., Obland, M. D., Cook, A. L., Harper, D. B., Burton, S. P., Shinozuka, Y., McNaughton, C. S., Clarke, A. D., Redemann, J., Russell, P. B., Livingston, J. M., and Kleinman, L. I.: NASA LaRC airborne high spectral resolution lidar aerosol measurements during MILAGRO: observations and validation, Atmos. Chem. Phys., 9, 4811–4826,	Partly taken into account. The reviewer is correct, however aircraft AOD measurements are unusual. Moreover the INTEX field campaign is already mentioned. The sentence introducing the MODIS/MISR/POLDER has been modified to account for this comment.

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						doi:10.5194/acp-9-4811-2009, 2009. [Government of United States of America]	
7-779	7	25	60	25	60	There are only two carefully validated long-term aerosol products over global ocean from AVHRR now. One is the GEWEX/GACP product (Geogdzayev et al., 2002) and another is NOAA product (Zhao et al., 2002; Zhao et al., 2004; Zhao et al., 2008). It is a negligence without mention NOAA AVHRR aerosol product here. The other AVHRR aerosol products are either just for a short term or for a specific regions and in research mode without sustainable maintenance and user access service. References for NOAA Long-term AVHRR Aerosol Product: 1) Zhao, X., L. L. Stowe, A. B. Smirnov, D. Crosby, J. Sapper, and C. R. McClain, Development of a global validation package for satellite oceanic aerosol optical thickness retrieval based on AERONET observations and its application to NOAA/NESDIS operational aerosol retrievals, J. Atmos. Sci., 59, 294-312, 2002. 2) Zhao, X., O. Dubovik, A. B. Smirnov, B. N. Holben, J. Sapper, C. Pietras, K. J. Voss, and R. Frouin, Regional Evaluation of an AVHRR two-channel aerosol retrieval algorithm, J. Geophys. Res., 109, D02204, doi:10.1029/2003JD003817, 2004. 3) Zhao, T. X.-P., I. Laszlo, W. Guo, A. Heidinger, C. Cao, A. Jelenak, D. Tarpley, J. Sullivan, Study of Long-term Trend in Aerosol Optical Thickness Observed from Operational AVHRR Satellite Instrument, J. Geophys. Res., 113, D07201, doi:10.1029/2007JD009061, 2008. [Xuepeng (Tom) Zhao, United States of America]	Accepted. Zhao et al. (2008) is now cited.
7-780	7	25	60			The authors may want to update the AVHRR references by replacing "(Geogdzhayev et al., 2002; Jeong and Li, 2005)" with "(Jeong and Li, 2005; Mishchenko et al., 2012; Zhao et al., 2011)" [Government of United States of America]	Accepted. Zhao et al. (2008) is now cited. Reference Zhao et al (2011) has not been provided.
7-781	7	26	1	16	1	Missing references for ATSR series. Consider adding e.g. Thomas, G.E., C.A. Poulsen, R. Siddans, A.M. Sayer, E. Carboni, S.H. Marsh, S.M. Dean R.G. Grainger and B.N. Lawrence, Validation of the GRAPE single view aerosol retrieval for ATSR-2 and insights into the long term global AOD trend over the ocean, Atmospheric Chemistry and Physics, 10, 4849—4866, 2010. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reference for ATSR series is cited now.
7-782	7	26	1	26	2	I have an objection to saying that datasets with longer measurements records are (more) useful. While TOMS and AVHRR AOD have much longer data records, the two datasets often fail to agree with each other unless one carefully conducts additional quality control for those data sets as shown by Jeong and Li (2005) and Jeong et al. (2005). I believe one can also say that MODIS or SeaWiFS AOD (Levy et al., 2007; Hsu et al., 2012) are more useful for building global aerosol climatology out of satellite-based measurements. [References: a) Levy, R. C., L. A. Remer, S. Mattoo, E. F. Vermote, and Y. J. Kaufman (2007), Second-generation operational algorithm: Retrieval of aerosol properties over land from inversion of Moderate Resolution Imaging Spectroradiometer spectral reflectance, J. Geophys. Res., 112, D13211, doi:10.1029/2006JD007811., b) Hsu, N.C., R. Gautam, A.M. Sayer, C. Bettenhausen, C. Li, M.-J. Jeong, S.-C. Tsay, and B.N. Holben (2012), Global and regional trends of aerosol optical depth over land and ocean using SeaWiFS measurements from 1997 to 2010, Atmos. Chem. Phys., 12, 8037-8053, doi:10.5194/acp-12-8037-2012.] [Myeong-Jae Jeong, Republic of Korea]	Taken into account. Longer measurement records are useful for deriving climatology but there are indeed uncertainties as discussed in Chapter 2, which is cross-referenced here. The sentence has been revised to cite Li et al (2009) in the last sentence.
7-783	7	26	3	26	3	… AERONET measurements, there are still large differences in regional…. Differences between what and what? [Elisabetta Vignati, Italy]	Accepted. "differences among satellite products".
7-784	7	26	4	26	4	To clarify the text, the authors might consider changing: "...differences in sampling, cloud screening and treatment of the surface reflectivity (Kokhanovsky et al., 2010)." to "...differences in sampling, cloud screening, treatment of the surface reflectivity, and lack of sensitivity to aerosol microphysical properties such as complex refractive index, multimodal size distribution, and morphology (Knobelspiesse et al., 2012; Kokhanovsky et al., 2010; Mishchenko and Travis 1997)." [Government of United States of America]	Partly taken into account. Differences in "aerosol microphysics" is now assumed.
7-785	7	26	4	26	4	after "sampling," add "calibration,". [Zhanqing Li, United States of America]	Accepted.
7-786	7	26	4	26	4	after "reflectivity" add "aerosol size distribution" [Zhanqing Li, United States of America]	Taken in account. "aerosol microphysical properties" has been added.

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7-787	7	26	4	26	4	before "Kokhanovsky" add reference "Li et al. 2009", noting that this paper is a summary of the GEWEX working group on assessment of all major satellite-based AOD products. It reviews extensively and rigorously the strengths and limitations of varous algorithms and products. I'd suggest to use a combination of Fig 1 and 5 in this paper to show the large discrepancies in AOD and ensuing uncertainties in the estimates of aerosol direct radiative forcing. [Zhanqing Li, United States of America]	Accepted. Suggested reference included.
7-788	7	26	4	26	4	Suggest adding a sentence something like "Li et al (2009) provided a nice review on the major uncertainties in the aerosol retrievals from various satellite instruments and their impacts on monitoring aerosol long-term trend" at the end of this line. Reference: Li, Z., T. X.-P. Zhao, R. Kahn, M. Mishchenko, L. Remer, K.-H. Lee, M. Wang, I. Laszlo, T. Nakajima, and H. Maring, Uncertainties in satellite remote sensing of aerosols and impact on monitoring its long-term trend: a review and perspective, Ann. Geophys., 27, 2755-2770, 2009. [Xuepeng (Tom) Zhao, United States of America]	Accepted. Li et al. (2009) is cited now and the exisiting sentence reflects the purpose of the suggested sentence.
7-789	7	26	8	26	8	It might be worth mentioning here that a monthly global AOD climatology was produced from the combination of satellite (mainly MODIS and MISR) AOD measurements with AERONET, that is used to constrain the AeroCom (and other) models (Kinne et al. ACP 2006). [Ralph Kahn, United States of America]	Rejected. Exisiting statement is more generelized and not specific to one study. Therefore, exisiting statement is retained. Moreover Kinne et al (2006) is not post-AR4.
7-790	7	26	11	26	11	MACC is mentioned in Figure 7.13 but not elsewhere in the text. Should this significant research effort not be mentioned as a relatively recent advance in our ability to study aerosols? [European Union]	Rejected. There are other effort to assimilate aerosol in models, therefore we do not wish to highlight further one example. MACC is referred to in the figure caption.
7-791	7	26	11	26	11	the wavelength of the optical depth needs to be stated. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. AOD wavelengths are now cited.
7-792	7	26	12	26	12	Volcanic aerosols are an important source for natural aerosols, able to cause an offset to global temperatures over a number of years. In the attribution of the anthropogenic aerosol effects, the observations of tropospheric and stratospheric aerosol of volcaninc origin is needed, as well as the associated feedbacks on stratospheric ozone and cloud formation. At this point in the text, a reference to related statements in Ch. 2 (e.g. Sec.2.2.2.2, Sec.2.2.2.4) and Ch. 11 (e.g. Faq.11.2) would be appropriate and appreciated. [Arnoud Apituley, The Netherlands]	Rejected. This statement seems out of place and we are not sure which page/line it refers to.
7-793	7	26	14			I don't see "circles", just "dots" [Andrea Flossmann, France]	Taken into account. The circles have been removed from the figure.
7-794	7	26	17	26	18	CALIPSO does not measure extinction. So while it plays an important role for validating models, it has a limited ability to constrain AERONET Single Satter Albedo. The text might be revised to clarify this point. [Government of United States of America]	Rejected. CALIPSO provides "extinction coefficient" as a product as shown on Figure 7.14. It is not clear what the reviewer means about SSA, which is not discussed here.
7-795	7	26	17	26	25	It is incorrectly stated that space borne lidars (plural) provide aerosol climatology, as only one space borne lidar does this: CALIOP. The other lidar in space GLAS (not referenced in the text) does not provied sufficient data foraerosol climatology. [Arnoud Apituley, The Netherlands]	Accepted. Sentence has been revised.
7-796	7	26	17			The word 'climatology' seems to be used loosely here- 4-5 years of CALIPSO data hardly provides a 'climatology'. This is probably common practice throughout the document- but the authors should be careful referring to short data records as 'climatologies' [Government of United States of America]	Rejected. "climatology" here is accurate enough for aerosols. We have mentioned interannual variability above.
7-797	7	26	21	26	21	Aerosol vertical distributions are routinely observed in a number of coordinated networks, in particular EARLINET in Europe (Pappalardo, G., et al. (2010), EARLINET correlative measurements for CALIPSO: First intercomparison results, J. Geophys. Res., 115, D00H19, doi:10.1029/2009JD012147.), ADnet in Japan (N. Sugimoto, A. Shimizu, I. Matsui, X. Dong, J. Zhou, X. Bai, J. Zhou, C.-H. Lee, S.-C. Yoon, H. Okamoto, I. Uno, Network Observations of Asian Dust and Air Pollution Aerosols Using Two-Wavelength Polarization Lidars, Reviewed and Revised Papers Presented at the 23rd International Laser Radar Conference, July 2006 Nara, Japan (23ILRC, ISBN 4-9902916-0-3) pp. 851-854) and several other regional networks that are linked together in the GAW network GALION (Bösenberg et al., GAW report 178, 2007). [Arnoud Apituley, The Netherlands]	Taken into account. We now mention ground-based lidars, but homogeneity of data remain a problem.

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7-798	7	26	21	26	21	Although GALION at this time is not yet capable to provide a global aerosol climatology of homogeneous quality, the ground based lidar data are of crucial importance for the quality assessment and assurance of the spaceborne observations that do provide global coverage. [Arnoud Apituley, The Netherlands]	Taken into account by mentioning ground-based lidars. No references to support mentioning this.
7-799	7	26	23	26	24	The reference to measurements from commercial aircraft is confusing. These are specially instrumented aircraft. Rather than extend the explanation, delete the sentence to shorten the chapter. [Daniel Murphy, United States of America]	Rejected. We mean commercial aircraft here.
7-800	7	26	26			It should be noted that a near global climatology of aerosol properties in the tropopause region is available from commercial aircraft flights (Heintzenberg, J., Hermann, M., Weigelt, A., Clarke, A., Kapustin, V., Anderson, B. and co-authors 2011. Near-Global Aerosol Mapping in the Upper Troposphere and Lowermost Stratosphere with Data from the Caribic Project. Tellus 63B, 875-890). [Jost Heintzenberg, Germany]	Accepted. This replaces the current CARIBIC reference.
7-801	7	26	27	26	27	The model names used in Fig. 7.14 are not consistent with the names for the same models used in the Aerocom II papers; this should be harmonised. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted. The model names are now consistent with the names used in AeroCom II papers, and with Fig. 7.17 in the supplementary material.
7-802	7	26	27	26	27	In the figure 7.14 both measurements and modelled values are reported but the comparison is not commented in the text. Why leaving both then in the figure? [Elisabetta Vignati, Italy]	Accepted. A sentence was added, this statement is important for our assessment of Rfari by BC in section 7.5.1.
7-803	7	26	36	26	39	Please add more information: In the open ocean the dominant source of bubbles is the breaking of wind-generated waves crashing at the surface. Within other areas as in the central Arctic, the low wind and short open-water fetch precludes production of bubbles by wave breaking suggesting that the bubbles are generated by processes below the surface. Norris et al., 2011 showed that when the surface water was open to the atmosphere bubble concentrations increased with the heat loss to the atmosphere. Other possible non-wave related sources of bubbles, common to areas wing low wind regimes, include the release of bubbles trapped in melting sea ice, expelled by freezing water (Wettlaufer, 1998), produced by the respiration of phytoplankton (Medwin 1970; Johnson and Wangersky, 1987), or released from the sea bed (e.g. Leighton and Robb 2008). REF: Johnson, B. D. and Wangersky, P. J.: Microbubbles: Stabilization by monolayers of adsorbed particles, J. Geophys. Res. 92, 14641-14647, 1987. Medwin, H.: In situ acoustic measurements of bubble populations in coastal waters, J. Geophys. Res., 75, 599-611, 1970. Norris, S. J., I. M. Brooks, G. de Leeuw, A. Sirevaag, C. Leck, B. J. Brooks, C. E. Birch, and M. Tjernström, 2011, Measurements of bubble size spectra within leads in the Arctic summer pack ice, Ocean Sci., 7, 129–139. Wettlaufer, G.: Introduction to crystallization phenomena in natural and artificial sea ice, in The Physics of ice covered seas edited by M. Lepparantä, Univ. of Helsinki, Helsinki, pp. 105-195, 1998. Leighton, T. G. and Robb, G. B. N.: Preliminary mapping of void fractions and sound speeds in gassy marine sediments from sub-bottom profiles, Journal of the Acoustical Society of America, 124, EL313-EL320, 2008. [Caroline Leck, Sweden]	Partly taken into account. The sentence of bubble bursting process was slightly modified. The is no space for discussing all potential processes for bubble generation specific to certain regions.
7-804	7	26	36	27	30	This section on aerosol sources might include a bit of the recent satellite contributions. Chemical transport models parameterize aerosol sources using a source strength and an injection height. Inverse modeling (e.g., Dubovik et al., 2008) has been used to constrain source strength from regional-to-global distributions of observed AOD. Source strength has also been constrained based on forward modeling plus satellite AOD snapshots for wildfires (Petrenko et al., JGR 2012). Injection height can be obtained from lidar, as you mention, but also from stereo imaging, which offers much greater coverage, so it captures aeroosl source regions much more frequently (e.g., val Martin et al., JGR 2010; 2012; Scollo et al., JGR 2012). In general, stereo imaging and lidar are complementary, with imaging providing upwind constraints on injection height and	Partly taken into account. Section 7.3.2.1 already cites the Dubovik et al (2008) study (page 27, line 30). Satellites are also mentioned on page 26, line 53. A short sentence has been added regarding therole of satellite in biomass burning inventories. The model aerosl vertical distribution is discussed later in Section 7.3.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						lidar obtaining downwind backscatter or extinction profiles, the *combination* offering especially good constraints on model aerosol vertical distribution (e.g., Kahn et al., GRL 2008). [Ralph Kahn, United States of America]	
7-805	7	26	36	27	37	"Sea spray particles comprise sea salt and primary marine matter, and are produced at the sea surface by bubble bursting induced mostly by breaking waves." For the average wind velocity of the oceans (9 m/s), only 3% (and less down to 0.05%, depending on the method) of the ocean surface is covered by whitecaps (Magdalena D. Anguelova, Michael H. Bettenhausen, Peter W. Gaiser: Passive Remote Sensing of Sea Foam using Physically-Based Models. magda@nrl.navy.mil). Please explain, how such a small fraction is supplying the whole ocean with sea spray aerosol. Other mechanisms must exist (rising air bubbles released in warmer waters without whitecaps). [Ruprecht Jaenicke, Germany]	Partly taken into account. Breaking waves do not have to produce whitecaps. The sentence of bubble bursting process was slightly modified to reflect the fact that all bubbles do not come from breaking waves (in the absence of a reference quantifiying better other sources of bubbles).
7-806	7	26	38	26	38	should be (Kerminen et al, 2010; Sipilä et al., 2010) [Ari Asmi, Finland]	Corrected. Relates to page 27.
7-807	7	26	39	26	46	A controversial issue in the composition of primary organic aerosol, that obviously came from the sea, has been the absence of sea salt on both primary organic aerosol (POA) and primary biological aerosol particles (PBAP) in sizes <200 nm diameter as noted by Gras and Ayers (1983), Leck et al. (2002), Pósfai et al. (2003), in Leck and Bigg, (2005a;b) and in Bigg and Leck (2008). In comparison with the organic aerosol (primary and/or secondary) experimentally produced or sampled in situ, range from 20-98% sea salt by mass (O'Dowd et al. 2004, Facchini et al. 2008, Keene et al. 2007, Russell et al. 2010) to 100% in particles even as small as 10 nm in diameter (Clarke et al., 2003). On the other hand Lewis and Schwartz (2004) gave a comprehensive review of field measurements of sea salt concentration related to size as a function of wind speed in which production of particles <500 nm diameter seems to have been slight or absent at any wind speed in almost all of the data sets, although the majority of size distributions have dealt only with larger sizes. To explain the lack of accumulation mode sea salt particles < 200 nm diameter the Bigg and Leck (2008) proposed a bubble-induced mechanism responsible for transporting polymer microgel-rich organic material from the bulk seawater into the open lead surface microlayer (SML, <1000 µm thick at the air-sea interface). It was suggested that the highly surface active polymer microgels (see comments #14 and #20) readily could attach to the surface of rising bubble and self-collide to form larger aggregates. Consequently, polymer gels and their aggregate production, as well as the embedded solid particles such as bacteria, phytoplankton and its detritus can be carried to the SML by rising bubbles selectively. Before bursting, bubbles rest in the SML and therefore are likely to have walls composed largely of exopolymers that give them strength, with embedded particulate matter that may be points of weakness as the water drains from between the walls. Following the burst, the film drop fragments would not be drops of salt water but of surfactant material with salt-free water and any particles attached to the fragments. Even the ability of jet drop particles that are mainly composed of sea salt to act as CCN is not straightforward. Their observed coating of highly surface-active exopolymers is known to revert to short chain compounds after exposure to UV (Orellana et al., 2011) which might make them less soluble than as if they only consisted of sodium chloride. Thus in terms of mass, sea-salt seems likely to be major constituent of supermicron aerosol in the marine atmosphere but certainly not in terms of number. Both POA and PBAP in sizes <200 nm diameter (accumulation mode) seems to dominate over sea-salt and can thus, over remote oceans, significantly contribute a significant fraction of the marine CCN population. REF: Clarke, A., Kapustin, V., Howell, S., Moore, K., Lienert, B., Masonis, S., Anderson, T., Covert, D.: Sea-Salt Size Distributions from Breaking Waves: Implications for Marine Aerosol Production and Optical Extinction Measurements during SEAS*, J. Atmos. Ocean. Tech., 20, 1362-1374, 2003. Leck, C., M. Norman, E. K. Bigg, and R. Hillamo (2002), Chemical composi- tion and sources of the high Arctic aerosol relevant for fog and cloud formation, J. Geophys. Res., 107(D12), 10, 4135, doi:10.1029/2001JD001463. Leck, C. and K. Bigg, 2008, Comparison of sourcesand nature of the tropical aerosol with the summer high Arctic aerosol, Tellus, 60B (1), 118–126,doi:10.1111/j.1600-0889.2007.00315.x. Facchini M. C., M. Rinaldi, S. Decesari, C. Carbone, E. Finessi, M. Mircea, S. Fuzzi, D. Ceburnis, R.	Partly taken into account but there is no space to go into details. Marine POM is furthery discussed in this section with a couple of new references.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Flanagan, E. D. Nilsson (2008), Primary submicron marine aerosol dominated by insoluble organic colloids and aggregates, Geophys. Res. Lett., 35(17), L17814, doi:10.1029/2008GL034210. Gras, J. L., and G. P. Ayers (1983), Marine aerosol at southern mid-latitudes, J. Geophys. Res., 88(C15), 10,661–10,666. Lewis, E. R., and S. E. Schwartz (2004), Sea salt aerosol production: Mechanisms, methods, measurements and models – A critical review, Geophys. Monogr. Ser., vol. 152, p. 413, AGU, Washington, D. C. Keene, W. C., Maring, H., Maben, J. R., Kieber, D. J., Pszenny, A. A. P., Dahl, E. E., Izaguirre, M. A., Davis, A. J., Long, M. S., Zhou, X., Smoydzin, L. And Sander, R.: Chemical and physical characteristics of nascent aerosols produced by bursting bubbles at a model air-sea interface, J. Geophys. Res., 112, D21202, doi:10.1029/2007JD008464, 2007. Orellana M.V., P.A. Matrai, C. Leck, C. D. Rauschenberg, A. M. Lee, and E. Coz 2011, Marine microgels as a source of cloud condensation nuclei in the high Arctic, PNAS, 108 (33): 13612-13617. Po´sfai, M., J. Li, J. R. Anderson, and P. R. Buseck (2003), Aerosol bacteria over the Southern Ocean during ACE-1, Atmos. Res., 66, 231–240. Russell, L. M., Hawkins, L. N., Frossard, A. A., Quinn, P. K., and Bates, T. S.: Carbohydrate like composition of submicron atmospheric particles and their production from ocean bubble bursting, P. Natl. Acad. Sci. USA, 107, 6652-6657, doi:10.1073/pnas.0908905107, 2010. [Caroline Leck, Sweden]	
7-808	7	26	42	26	44	Th FACCHINI et al. study WAS NOT the first to acknowledge the primary source of biogenic organic matter in the submicrometer diameter size range. This is the present status of knowledge: In 2011, researchers confirmed for the first time that the particles found in the marine atmosphere over the central Arctic Ocean behave as polymer nano-and microgels (marine gels) and originate in the upper most surface waters (Orellana et al. 2011) from biological secretions as of the activity of sea-ice algae, phytoplankton and bacteria. Marine gels are highly hydrated (99% water) polysaccharide molecules partly hydrophilic/hydrophobic in nature with highly surface-active properties. The polysaccharide molecules spontaneously form 3-dimensional networks inter bridged with divalent ions (Ca 2+/Mg2+), to which other organic compounds, such as proteins and lipids, are readily bound (Verdugo, 2012 gives an review). Their importance as a significant source for CCN and thus for cloud formation over the inner part of the central Arctic Ocean was also inferred from the Orellana et al. study, a behavior also supported by the theoretical study by Ovadnevait et al. (2011). These results verify past studies of the aerosol-cloud relationship over the Arctic pack ice area during the last decade (Bigg et al., 2004; Bigg and Leck, 2008; Leck and Bigg, 2005a,b; Leck and Bigg, 1999; 2010). Even if the co-occurrence of atmospheric organic material containing polysaccharides and biologically active marine waters has only up to date been confirmed for the high Arctic open lead waters, it has also more recently been documented for temperate waters (Faccini et al. 2008; Leck and Bigg, 2008; Russell et al. 2010). But the universality of such marine gels, both in the coastal and open-water regions of the Arctic Ocean and at lower latitude oceans, has not yet been confirmed. These new findings does not contradict but rather strengthening earlier results by O'Dowd et al., 2004; that during phytoplankton blooms (summer conditions), the organic contribution to the submicrometer airborne aerosol can be as high as 63%. REF: Bigg, E.K., C. Leck and L. Tranvik, 2004, Particulates of the surface microlayer of open water in the central Arctic Ocean in summer, Marin Chemistry, 91, 131-141. 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. Facchini M. C., M. Rinaldi, S. Decesari, C. Carbone, E. Finessi, M. Mircea, S. Fuzzi, D. Ceburnis, R. Flanagan, E. D. Nilsson (2008), Primary submicron marine aerosol dominated by insoluble organic colloids and aggregates, Geophys. Res. Lett., 35(17), L17814, doi:10.1029/2008GL034210.	Partly taken into account. The text did not claim that this was the first paper to discuss the maring biogenic organic matter, but rather to highlight that the given paper demonstrated a connection between marine POA emission and biological activity in the ocean. The text was modified and a couple of references have been added..

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Hawkins, L.N. and Russell, L.M., 2010. Polysaccharides, Proteins, and Phytoplankton Fragments: Four Chemically Distinct Types of Marine Primary Organic Aerosol Classified by Single Particle Spectromicroscopy. Advances in Meteorology, 2010. Leck, C., and Bigg, E. K. (1999). Aerosol Production Over Remote Marine Areas—A new Route. Geophys. Res. Letters 26:3,577–3,581. Leck, C., and E.K. Bigg, 2005a, Biogenic particles in the surface microlayer and overlaying atmosphere in the central Arctic Ocean during summer, Tellus 57B, 305-316. Leck, C., and E.K. Bigg, 2005b, Source and evolution of the marine aerosol—A new perspective, Geophys. Res. Lett., 32, L19803, doi:10.1029/2005GL023651 Leck, C. and K. Bigg, 2008, Comparison of sources and nature of the tropical aerosol with the summer high Arctic aerosol, Tellus, 60B (1), 118–126,doi:10.1111/j.1600-0889.2007.00315.x. Leck, C. and Bigg, E.K., 2010. New particle formation of marine biological origin. Aerosol Science and Technology, 44: 570-577. O'Dowd, C.D., M.C. Facchini, F. Cavalli, D. Ceburnis, M. Mircea, S. Decesari, S. Fuzzi, Y.J. Yoon, and J.P. Putaud, Biogenically-driven organic contribution to marine aerosol, Nature, doi:10.1038/nature02959,. 2004. Orellana M.V., P.A. Matrai, C. Leck, C. D. Rauschenberg, A. M. Lee, and E. Coz 2011, Marine microgels as a source of cloud condensation nuclei in the high Arctic, PNAS, 108 (33): 13612-13617. Ovadnevaite, J., D. Cerburnis, G. Martucci, J. Bialek, C. Monahan, M. Rinaldi, M. C. Facchini, H. Berresheim, D. R. Worsnop, and C. O´Dowd (2011), Primary marine organic aerosol: a dichotomy of low hygroscopocity and high CCN activity, Geophys. Res. Lett., 38, L21806, doi:10.1029/2011GL048869. Verdugo P., Marine Microgels (2012), Annu. Rev. Mar. Sci., 4, 375-400. [Caroline Leck, Sweden]	
7-809	7	26	42	26	46	Sea spray and primary biological material are in the submicrometer range! But Table 7.3 says PBAP are mainly in the coarse mode (what indeed is nonsense) [Ruprecht Jaenicke, Germany]	Taken into account. Table 7.3 has been modified.
7-810	7	26	43	23	44	Marine biological particles have earlier been rep rted by the Stockholm group (Leck, C. and Bigg, E. K. 1999. Aerosol Production over Remote Marine Areas - a New Route. Geophys. Res. Lett. 23, 3577-3581. Bigg, E. K. and Leck, C. 2001. Properties of the Aerosol over the Central Arctic Ocean. J. Geophys. Res. 106, 32,101-132,109. Bigg, E. K., Leck, C. and Tranvik, L. 2004. Particulates of the Surface Microlayer of Open Water in the Central Arctic Ocean in Summer. Mar. Chem. 91, 131-141. Leck, C. and Bigg, E. K. 2005. Biogenic Particles in the Surface Microlayer and Overlaying Atmosphere in the Central Arctic Ocean During Summer. Tellus 57B, 305–316. Leck, C. and Bigg, E. K. 2005. Source and Evolution of the Marine Aerosol - a New Perspective. Geophys. Res. Lett. 32, L19803, doi:19810.11029/12005GL023651. Lohmann, U. and Leck, C. 2005. Importance of Submicron Surface Active Organic Aerosols for Pristine Arctic Clouds. Tellus 57B, 261-268). [Jost Heintzenberg, Germany]	Partly taken into account. Marine POM is furthery discussed in this section with a couple of new references.
7-811	7	26	45			Natural number concentrations in the pristine marine atmosphere are not as uncertain as submitted here. as reported for decades their number is on the order of a few hundred per cc at sea level. [Jost Heintzenberg, Germany]	Partly taken into account. "large" has been changed to "significant" and an additional references has been added to mention in-situ measurements explicitly.
7-812	7	26	46			In general particle number concentrations cannot be constrained from space because the number-controlling particle sizes are significantly below the wavelength of optical satellite sensors [Jost Heintzenberg, Germany]	partly taken into account. The sentence has been modified.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7-813	7	26	46			"submicronic size range", jargon; better "submicrometer" and still better "submicrometer diameter range" [Stephen E Schwartz, United States of America]	corrected
7-814	7	26	48	26	54	Tratment of dust aerosol in climate models is a complex exercise, primarily because it arises from both natural and anthropogenic origin. Its size seggregation, and physico-chemical modfication through transport processes needs detailed study. [Panuganti, C.S. Devara, India]	Agreed. The discussion was slightly modified to highlight both natural and anthropogenic sources. IPCC does not make recommendation for future research.
7-815	7	26	51			In the region like India, significant amount of atmospheric dust is contributed by vehicular traffic often called as `ROAD DUST'. Ref:Bhaskar S and Sharma M 2008. Assessment and Characterization of Fugitive Road Dust Emission in Kanpur, India: A Note" Transportation Research Part D: Transport and Environment. Volume 13, Issue 6, 400-403. [Umesh Kulshrestha, India]	Taken into account. Road dust is now mentioned.
7-816	7	26	52	26	52	The fraction of dust from anthropogenic sources…. Please mention what are the dust anthropogenic sources [Elisabetta Vignati, Italy]	Taken into account. The text was modified to specify anthropogenic sources.
7-817	7	26	56	26	58	Strongly object to that PBAP over the remote ocean likely have a small contribution to the accumulation mode. See comment #14, #19 and #20. [Caroline Leck, Sweden]	Taken into account. The text makes it specific that this is about terrestrial PBAP.
7-818	7	26	57			the statement that PBAP are mostly in the coarse mode cannot be true. It probably doesn't take into account viruses that are very abundant and have sizes in the nm range. [Andrea Flossmann, France]	Partly taken into account. References are needed to back this up.
7-819	7	27	7	27	7	There are experimental evidences that soil humidity also affect biogenic emissions of some species that can be inhibited during ambient stress conditions.This effect is related to the diurnal physiological cycle of vegetation which closes its stomata as response to water and temperature stress. 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 environmental factor must be included in the text as follows: "BVOC emissions depend on the amount and type of vegetation, temperature, radiation and several environmental factors such as soil humidity (Plaza et al., 2005) or the ambient CO2 concentration (Grote and Niinemets, 2008; Pacifico et al., 2009)" [BEGONA ARTINANO, SPAIN]	Partly, taken into account. The influence of soil humidity was added to the text and a reference was added.
7-820	7	27	16	27	16	add "the" between "most of" and "atmospheric SOA". [Peter Irvine, Germany]	Corrected.
7-821	7	27	17			anthropogenic sources could be equally important at northern midlatitudes: could add Bahreini, R., et al., 2012. Gasoline emissions dominate over diesel in formation of secondary organic aerosol mass, Geophys. Res. Lett., 39, L06805, doi:10.1029/2011GL050718, where the authors show that in the afternoon fossol TC dominates non-fossil TC. [Urs Baltensperger, Switzerland]	Rejected. No need for an additional reference here.
7-822	7	27	18	27	21	To my knowledge, the first study that mentioned that SOA formation from BVOC is enhanced by pollution is ' Kanakidou M., K. Tsigaridis, F. J. Dentener and P.J. Crutzen, Human activity enhances the formation of organic aerosols by biogenic hydrocarbon oxidation, J. Geophys. Res., 105, 9243-9254, 2000. http://www.agu.org/pubs/crossref/2000/1999JD901148.shtml [MARIA KANAKIDOU, GREECE]	Accepted. The reference was added
7-823	7	27	20			.. facilitate transformation of VOCs to the particle phase: it's not the VOCs themselves but rather the oxidized VOCs (also called OVOCs that are transformed to the particle phase [Urs Baltensperger, Switzerland]	Agreed, corrected.
7-824	7	27	24	7	26	(section 7.3.2.1) "...but the split between primary organic aerosol (POA) and SOA has remained somewhat ambiguous due to atmospheric transformation processes affecting both these components (Jimenez et al., 2009; Robinson et al., 2007)." A major fraction of POA may evaporate upon dilution and later partitions back to the aerosol phase as SOA. The problem with POA is that it has traditionally been measured at very high concentrations and dilution ratios that are much lower than those encountered when emitted into the atmosphere. That is why old emission databases are often not valid. I would mention POA evaporation also (Robinson et al., 2007 emphasizes this). [Erik Swietlicki, Sweden]	POA evaporation is part of atmospheric transformation processes mentioned in the text and in SOD Figure 7.11.
7-825	7	27	25	27	25	please also mention NOx, if nitric acid is mentioned, sulfuric acid also be mentioned. [Junying Sun, China]	Agree, nitrogen oxides were added.

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7-826	7	27	25	27	25	I would say… Anthropogenic sources of aerosol particles (BC, POA)… [Elisabetta Vignati, Italy]	Corrected.
7-827	7	27	26	27	29	I think it should be mentioned, that this sentence (starting with "It should be noted..") is strictly valid only for mass properties. In the case of aerosol number concentraitons, and especially new particle formation, the mechanisms are so strongly interwoven that e.g. too low sink terms could in the end reduce new particle formation, which could in the end produce similar concentrations as observations, but for wrong reasons. [Ari Asmi, Finland]	Taken into account. The sentence was deleted. Refer to page 28.
7-828	7	27	29	27	30	Sort of an empty sentence about remote sensing. Delete to shorten the chapter [Daniel Murphy, United States of America]	Rejected. Some comments have asked for the utility of satellite to be mentioned in this section.
7-829	7	27	34			Section 7.3: Please change: embryos to nuclei [Government of Poland]	Partly taken into account. Changed to "molecular clusters"
7-830	7	27	39			.. low-volatile: replace by low-volatility. [Urs Baltensperger, Switzerland]	Corrected.
7-831	7	27	40	27	42	I'm not sure that reading the text people can understand what it means neutral or ion-induced nucleation [Elisabetta Vignati, Italy]	The text was modifed. The term "ion-induced" nucleation not used anymore.
7-832	7	27	42	27	42	Please add after end of sentence: The high Arctic (north of 80◦N) in summer is a region characterized by clean air and low abundances of pre- existing particles (Bigg et al., 1996). The relative frequent occurrence of new particle formation events is difficult to explain because of the limited availability of low-volatile vapors such as sulfuric acid (Leck and Bigg, 1999;2010; Karl et al., 2012a). Supported by observational evidence and model results obtained from the sectional multicomponent aerosol model MAFOR (Karl et al., 2012a) new particle formation in the high Arctic could only be explained by involving condensation of semi-volatile organic vapors onto nm-sized granules derived from marine nanogels (comment #20), (Karl et al., 2012b). REF. Bigg E. K., C. Leck, and E. D. Nilsson (1996), Sudden changes in arctic atmospheric aerosol concentrations during summer and autumn, Tellus, 48B, 254–271. Karl, M., C. Leck, A. Gross, and L. Pirjola, 2012a, A Study of New Particle Formation in the Marine Boundary Layer Over the Central Arctic Ocean using a Flexible Multicomponent Aerosol Dynamic Model. Tellus 64B, 17158, doi: 17110.13402/tel- lusb.v17164i17150.17158. Karl, M., E. Coz, and C. Leck, , 2012b, Marine nanogels as a source of atmospheric nanoparticles in the high Arctic, GRL (submitted). Leck, C., and Bigg, E. K. (1999). Aerosol Production Over Remote Marine Areas—A new Route. Geophys. Res. Letters 26:3,577–3,581. Leck, C. and Bigg, E.K., 2010. New particle formation of marine biological origin. Aerosol Science and Technology, 44: 570-577 [Caroline Leck, Sweden]	Rejected. There is, unfortunately, no space for discussion regional specifics associated with atmospheric new particle formation. The next paragraph already mentions that the growth of the smallest particles depends critically on the condensation of organic vapours.
7-833	7	27	44	27	45	it is not true that condensation is always the dominant process for growth to larger sizes. In case of high concentrations of particles, particularly close to high intensity sources coagulation can play that role [Elisabetta Vignati, Italy]	Agree, changes as "usually the dominant process".
7-834	7	27	44	27	49	How representative is the one cited study globally? This may be quite different in other environments? [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Two additional studies were added. Two of the three papers include global estimates.
7-835	7	27	45	27	45	The way this is written sounds like only coagulation affects mixing state. However, condensation and cloud processing also contribute. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted. The text was modified to avoid this potential problem.
7-836	7	27	45			"dominant process for growth to larger sizes" is in contradiction to the next para "main sink for smallest aerosol particles" [Andrea Flossmann, France]	Rejected. There is no contradiction, growth is not a sink for individual particles. Coagulation is a sink for the smallest particles but not for aerosol mass.In any

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							case, the text was modified to avoid confusion.
7-837	7	27	51	27	51	Typo: "… with one another and …" [Ralph Kahn, United States of America]	Corrected.
7-838	7	27	51	27	51	It is not correct to say that Brownian coagulation is the process for which aerosol particles collide and stick together. This process is "coagulation"; browian coagulation is the process taking place when the particles come into contact due to their Browian motion. The sentence should be corrected [Elisabetta Vignati, Italy]	Agreed, coaguation instead of Brownian coaguation is now used.
7-839	7	27	51	27	54	The paragraph on coagulation could be deleted to shorten the chapter. No clear follow-on to assessing climate. [Daniel Murphy, United States of America]	The paragraph was modified but not deleted. It is an important process that affects climatically-relevant properties of atmospheric aerosol populations.
7-840	7	27	51			Section 7.3: Please change: other to another [Government of Poland]	Corrected.
7-841	7	27	51			Section 7.3: Please change: termed to known as [Government of Poland]	Corrected. The text has been modified.
7-842	7	27	52	27	54	Coagulation is the main sink not only in the cases mentioned here, but also in case of high particles concentrations such as close to high intensity sources [Elisabetta Vignati, Italy]	Agree, the text was modified.
7-843	7	27	52			see point 38: the two statements are mutual exclusiv. Please decide which one. [Andrea Flossmann, France]	Rejected. No real contradiction (see answer to comment 7-836). Anyway, the text was modified.
7-844	7	27	57	27	57	add some references from China,such as （1）X. J. Shen, J. Y. Sun, Y. M. Zhang, B.Wehner, A. Nowak, T. Tuch, X. C. Zhang, T. T. Wang, H. G. Zhou, X. L. Zhang, F. Dong, W. Birmili, and A.Wiedensohler, 2011. First long-term study of particle number size distributions and new particle formation events of regional aerosol in the North China Plain, Atmos. Chem. Phys., 11, 1565–1580.（2）Wu, Z., M. Hu, S. Liu, B. Wehner, and S. Bauer,New particle formation in Beijing, China: Statisticalanalysis of a 1-year data set, Journal of Geophysical Research, 2007,112(D9), D09209. [Junying Sun, China]	Rejected. There is, unfortunately, space for studies concentrating on single locations here.
7-845	7	27	57	28	1	The recent report on neucleation event in free troposphere and the strong solar control by Moorthy et al. 2011 is to be cited here along with other similar works, because this is the only such report from south Asia. [K KRISHNA MOORTHY, INDIA]	Rejected. There is, unfortunately, space for studies concentrating on single locations here.
7-846	7	28	1	28	1	Please add Karl et al., 2012a;b. REF: Karl, M., C. Leck, A. Gross, and L. Pirjola, 2012, A Study of New Particle Formation in the Marine Boundary Layer Over the Central Arctic Ocean using a Flexible Multicomponent Aerosol Dynamic Model. Tellus 64B, 17158, doi: 17110.13402/tel- lusb.v17164i17150.17158. Karl, M., E. Coz, and C. Leck, , 2012, Marine nanogels as a source of atmospheric nanoparticles in the high Arctic, GRL (submitted). [Caroline Leck, Sweden]	Rejected. There is, unfortunately, insufficient space for studies concentrating on single locations or a few individual cases here.
7-847	7	28	1	28	5	I don’t believe this to be a good assessment of the literature. Other model studies suggest that CCN concentrations are very insensitive to nucleation. And citing Pierce and Adams (2009) in support of a “large impact” isn’t completely accurate. They say “These results show the importance of reducing uncertainties in primary emissions, which appear from these results to be somewhat more important for CCN than the much larger uncertainties in nucleation.“ I think there are two choices: Delete to shorten the chapter, or I think a more accurate assessment would be to keep lines 6-9, then go on to say “A given percentage change in nucleation induces only a much smaller percentage change in CCN. Nevertheless, some models show that those small percentage changes in CCN could still be important (refs) while other models show that changes in nucleation are less important than other uncertainties (refs, incuding Adams and Pierce)” [Daniel Murphy, United States of America]	Taken into account. The text has been modified and clarified although not quite as suggested by the reviewer.
7-848	7	28	1	28	5	See previous comment: "large impact on the cloud condensation nuclei (CCN) concentrations" shoud be qualified, because according to many studies (incl some of the ones cited), the influence of nucleation on CCN is between 5 and 20% of the total CCN population; this does not warrant the qualification "large" I would argue. [Bart Verheggen, Netherlands]	The text was modified. Especially, "large impact" was removed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7-849	7	28	5	28	9	This sentence may require rephrasing. [Peter Irvine, Germany]	Rejected. Clear as it is.
7-850	7	28	5			It should be noted here that near all previous assments of climate forcing by airborne BC are based on BC data derived from aethalometers that exhibit severe systematic errors which have not been resolved yet (e.g., Moteki, N., Kondo, Y., Nakayama, T., Kita, K., Sahu, L. K., Ishigai, T. and co-authors 2010. Radiative Transfer Modeling of Filter-Based Measurements of Light Absorption by Particles: Importance of Particle Size Dependent Penetration Depth. J. Aerosol Sci. 41, 401-412. Nakayama, T., Kondo, Y., Moteki, N., Sahu, L. K., Kinase, T., Kita, K. and co-authors 2010. Size-Dependent Correction Factors for Absorption Measurements Using Filter-Based Photometers: Psap and Cosmos. J. Aerosol Sci. 41, 333-343. Collaud Coen, M., Weingartner, E., Apituley, A., Ceburnis, D., Fierz-Schmidhauser, R., Flentje, H. and co-authors 2010. Minimizing Light Absorption Measurement Artifacts of the Aethalometer: Evaluation of Five Correction Algorithms. Atmos. Meas. Tech. 3, 457-474). [Jost Heintzenberg, Germany]	Rejected. This comment apparently relates to page 29, line 5. The quoted reference do not refer to aethalometer measurements.
7-851	7	28	7	28	27	Since AR4, there has been extensive research on SOA. In many urban locations and in urban plumes (e.g. Mexico City) OA and SOA concentrations were as much as an order of magnitude greater than expected. Lines of research have included the production of low volatility VOCs either from emissions, or the multi-step oxidation of more volatile VOCs, the role of chemistry in the aqueous and aerosol phase, and interactions between anthropogenic and biogenic compounds. As far as organic aerosols are concerned, has much has changed since AR4? In contrast, it seems like the subtleties of aerosol-cloud interactions, have been well described. [Government of United States of America]	Noted. this comment apparently relates to page 29, lines 7-27. A number of processes mentioned in this comment are already discussed in the SOD.
7-852	7	28	11	28	14	It is mentioned here that more detailed discussions to be presented in Section 7.4, but the importance of cloud-processed particles smeared out in Section 7.4 where aerosol-cloud interactions (e.g., Twomey and Albrecht effects) are discussed mostly. A little more discussion about the importance of cloud-processed particles may be presented here on Page 28 (e.g., Hoppel et al., 1994; Krämer et al., 2000; Jeong and Li, 2010; references provided in other comments). For instance, Jeong and Li (2010) showed that there is correlation between AOD and cloud cover which cannot be fully explained by aerosol humidification effect (e.g., Jeong et al., 2007) nor cloud contamination or synoptic scale convergence. Cloud-processed particles and newly formed particles are suggested as strong candidates to explain the frequently observed correlation between cloud cover and AOD (Jeong and Li, 2010). [Reference: Jeong, M.-J., Z. Li, E. Andrews, and S.-C. Tsay (2007), Effect of aerosol humidification on the column aerosol optical thickness over the Atmospheric Radiation Measurement Southern Great Plains site, J. Geophys. Res., 112, D10202, doi:10.1029/2006JD007176.] [Myeong-Jae Jeong, Republic of Korea]	The text was slightly modified and a few relevant references were added.
7-853	7	28	13	28	13	add another hyphen between "cloud" and "hydrometeor" [Peter Irvine, Germany]	Taken into account. "cloud" has been deleted as it is one hydrometeor.
7-854	7	28	16	28	29	Recommend citing the Textor et al (2006) paper here for the overall discussion of wet and dry removal of aerosols among Aerocom models. In addition, you could expand the discussion to mention the relative importance of wet and dry deposition between different aerosol species. [William Landuyt, United States of America]	Rejected. We focus on post-AR4 references.
7-855	7	28	20	28	22	Wet scavenging also depends on activation, which crucially depends on uncertain pdf of updrafts. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	nucleation scavenging now explicitly mentioned and text modified accordingly
7-856	7	28	21	28	21	"wet deposition" here seems only referring to "impaction scavenging"(or so-called rainout) while excluding "nucleation scavenging"? [Chien Wang, United States of America]	nucleation scavenging now explicitly mentioned and text modified accordingly
7-857	7	28	22	28	22	I would argue that in addition to (or even instead of) the AMOUNT of precipitation, the fraction of the gridbox AREA that is covered by precipitation (which is much smaller than the area covered by cloud, but which is not determined by any particularly physically-based method in GCMs), controls (or should control) the amount of wet deposition. [Anthony Del Genio, United States of America]	Partly taken into account. The text was modified.
7-858	7	28	22	28	22	Insert "and area" after "amount". [Steven Ghan, United States of America]	Taken into account. The text was modifed. Areal extent of precipitation is now included.
7-859	7	28	22	28	22	add commas around "to some extent" [Peter Irvine, Germany]	Corrected.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7-860	7	28	23	28	24	If the wet deposition discussed here only include impaction scavenging, then this statement of "For insoluble primary particles…" is inaccurate. Impaction scavenging (collision and collection of aerosols by cloud drops) is a kinetic process. [Chien Wang, United States of America]	Corrected, we refer to nucleation scavenging here.
7-861	7	28	24	28	24	You may want to add one of the few analysis of model intercomparison focusing on observed wet and dry deposition by Prospero et al. (2010). Although it is only for dust, it is quite informative to learn from Prospero et al. (2010) such a disparity between models and a total discrepancy in the ratio dry/wet removal. Prospero, J. M., W. M. Landing, and M. Schulz (2010), African dust deposition to Florida: Temporal and spatial variability and comparisons to models, J. Geophys. Res., 115, D13304, doi:10.1029/2009JD012773. [Paul Ginoux, United States of America]	Accepted. References added.
7-862	7	28	24	28	24	Please add Granat et al., 2009 and Coz and Leck, 2010. REF: 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. Granat, L., J.E. Engström, S. Praveen, and H. Rodhe, Light absorbing material (soot) in rainwater and in aerosol particles in the Maldives, J. Geophys. Res. 115, D16307, doi: 10.1029/2009JD013768 (2010). [Caroline Leck, Sweden]	Rejected. The proposed citation does not make that point.
7-863	7	28	26	28	29	So you think that global aerosol concentrations are sufficiently well constrained by observations that we can use them to constrain aerosol sources and sinks in models? I'm not sure I believe that. What is the dataset that tells us aerosol composition and concentration within and below clouds that we can use to constrain things like wet deposition? [Anthony Del Genio, United States of America]	Accepted. The sentence was deleted as it does not bring much to the section. Satellite constraints on sources is discussed in the previous section.
7-864	7	28	26	28	29	This is sentence is confusing. It could be deleted. [European Union]	Accepted. The sentence was deleted.
7-865	7	28	27			Section 7.3: Word term is wrongly used [Government of Poland]	Accepted. The sentence was deleted.
7-866	7	28	28			Reagarding the statement, "Uncertainties in our current understanding of CCN properties are associated with SOA mainly because OA is still poorly characterized", there could be some confusion. Charaterization of OA would be a necessary but not sufficient step in understanding SOA, no? If OA are not understood/characterized - then certainly SOA cannot be. But once they are understood - it seems as though there may be more to learn. This statement may constitute only a partial representation of SOA understanding and the authors mgiht consider expanding the text accordingly. [Government of United States of America]	Rejected. This comment apparently refers to page 30. The statement is right as it stands. Understanding of OA is prominently discussed in Section 7.3.3.1 and there is a lack of space to go in further detail here.
7-867	7	28	31	28	31	Replace "Progresses" with "Progress". [Steven Ghan, United States of America]	Accepted. Editorial.
7-868	7	28	34	28	36	I would suggest that aerosol size is just as important as several of these other properties. [Anthony Del Genio, United States of America]	TAgreed but no change is made. Aerosol number size distribution is already imentioned as a key climate relevant aerosol property on line 35.
7-869	7	28	36	28	36	Please replace “shape” with morphology. [Caroline Leck, Sweden]	Agreed. "Shape" is replaced with "morphology"
7-870	7	28	38	28	38	Replace "aerosols" with "aerosol". [Steven Ghan, United States of America]	Accepted. Editorial.
7-871	7	28	38	28	38	Some elaboration on 'key aerosols properties' need to be included here for better appreciation of statement. [Government of India]	Rejected. Key climate relevant aerosol properties are already listed in the same paragraph.
7-872	7	28	39			Section 7.3: Please remove: instance [Government of Poland]	Partly taken into account. Analysing individual particles is only one of the instrumental advances. Sentence has been slightly modified to make it clearer.
7-873	7	28	41	28	41	May I shamelessly suggest your cite the following paper after "optical properties"? Ghan, S. J., and S. E. Schwartz, 2007: Aerosol properties and processes: A path from field and laboratory measurements to global climate models. Bull. Amer. Meteorol. Soc., 88, 1059-1083. [Steven Ghan, United States of America]	Accepted. Suggested reference is cited.
7-874	7	28	47			Since BC is also emitted by biomass burning, the sentense needs modification. [Umesh Kulshrestha, India]	Accepted. The statement was revised.

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7-875	7	28	48	28	49	"..., BC and organics have received increasing attention". One main reason for this is that BC is included amongst the short lived climate forcers who has received much attention both scientifically and politically, due to their substantial contribution to RF (ref SPM page 8, line 16). We propose that you add text on why BC has received increased attention in this chapter. [Government of NORWAY]	Partly taken into account. Scientific reasons for the increased importance of BC is now provided in the statement. Political reasons are probably beyond the scope of this chapter.
7-876	7	28	49	28	49	Suggestion to replace 'organics' with 'organic aerosol' [Government of India]	Accepted. Editorial.
7-877	7	28	51	28	52	The 400 K temperature is too low. For most of the thermal measurement protocols the corresponding temperature is 550 K. To avoid these issues the specific temperature could be deleted. [European Union]	Accepted. This was a typographical mistake. 400K is replaced with 4000K.
7-878	7	28	51	28	52	(section 7.3.3.1) "The physical properties of BC (strongly light-absorbing, refractory with a vaporization temperature near 400 K,.." Surely, this must be degrees Centigrade, not Kelvin. [Erik Swietlicki, Sweden]	Accepted. This was a typographical mistake. 400K is replaced with 4000K.
7-879	7	28	51	28	55	"The physical properties of BC (strongly light-absorbing, refractory with a vaporization temperature near 400 K, aggregate in morphology, insoluble in most organic solvents) allow a strict definition at least in principle (Bond et al., 2012)". The physical properties should be given if this statement is to be included in the report. [Government of NORWAY]	The physical properties are listed in parenthesis. Further details are provided in the Bond et al. (2013) paper and are not repeated due to space limitation.
7-880	7	28	51			.. near 400 K: replace by near 4000 K [Urs Baltensperger, Switzerland]	Accepted. Corrected.
7-881	7	28	53	28	53	remove "a" from before "laser-induced" [Peter Irvine, Germany]	Accepted. Editorial.
7-882	7	28	53	28	55	(section 7.3.3.1) "Direct measurement of individual BC-containing particles is possible with a laser induced incandescence (also called SP2, Gao et al., 2007; Moteki and Kondo, 2010; Schwarz et al., 2008b), which has enabled accurate measurements of the size of BC cores, as well as total BC mass concentrations." When the SP2 unit is incorporated into the Aerodyne Aerosol Mass Spectrometer (SP-AMS) the laser (1064 nm) vaporizes the particles that are then ionized and entered into the MS for quantitative analysis of BC (soot) concentrations. T. B. Onasch, A. Trimborn, E. C. Fortner, J. T. Jayne, G. L. Kok, L. R. Williams, P. Davidovits & D. R. Worsnop (2012): Soot Particle Aerosol Mass Spectrometer: Development, Validation, and Initial Application, Aerosol Science and Technology, 46:7, 804-817. http://dx.doi.org/10.1080/02786826.2012.663948 [Erik Swietlicki, Sweden]	Rejected. It is not clear what the suggestion is about and why.
7-883	7	28	57	28	57	Please add Coz and Leck, 2010. REF: 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. [Caroline Leck, Sweden]	Rejected. The text is clear that it refers to a sample of references on this.
7-884	7	28	57			use "produce" or "create" instead of "make" [Andrea Flossmann, France]	Accepted. Changed to "produce".
7-885	7	29	5	29	5	To put the Kochand Schwarz studies in perspective, you could add a short sentence after the Schwarz citation saying, "This is important because the radiative forcing and climate response of black carbon is highly dependent on its altitude (Ban-Weiss et al. 2012). Ban-Weiss, G, Cao, L, Bala, G, Caldeira, K (2012) Dependence of climate forcing and response on the altitude of black carbon aerosols. Climate Dynamics. 38:897-911. [George Ban-Weiss, United States of America]	Accepted. Howver this was moved to section 7.3.1 instead/
7-886	7	29	7	29	8	(section 7.3.3.1) "Formation processes of OA remain highly uncertain, which is a major weakness in the present understanding and model representation of atmospheric aerosols (Hallquist et al., 2009; Kanakidou et al., 2005)." You might consider adding this reference as well: Paul J. Ziemann and Roger Atkinson, Kinetics, products, and mechanisms of secondary organic aerosol formation. Chem. Soc. Rev., 2012, 41, 6582–6605. DOI: 10.1039/c2cs35122f [Erik Swietlicki, Sweden]	Rejected. We can only cite a limited subset of the available literature.
7-887	7	29	9	29	12	The word "measurements" is repeated three times in 4 lines [Elisabetta Vignati, Italy]	Accepted. We have revised the statements.
7-888	7	29	14	29	14	SOA estimations from a semicontinuous series (> 2 years) at a suburban background site [BEGONA ARTINANO, SPAIN]	Rejected. Comment not clear.
7-889	7	29	14	29	17	There is a growing amount of literature suggesting that atmospheric "brown carbon compounds" can be	Partly taken into account. A long list of references is

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						produced in atmospheric multi-phase reactions between gas-phase, particulate and cloud micro-droplet constituents. Of particular interest are the light-absorbing nitrogen containing organics that can be formed by different chemical reactions relevant to the atmospheric environment. These advances might be worthy of reference and discussion here. Refs: Gelencser, A.; Hoffer, A.; Kiss, G.; Tombacz, E.; Kurdi, R.; Bencze, L., In-situ formation of light-absorbing organic matter in cloud water. Journal of Atmospheric Chemistry 2003, 45, (1), 25-33. Limbeck, A.; Kulmala, M.; Puxbaum, H., Secondary organic aerosol formation in the atmosphere via heterogeneous reaction of gaseous isoprene on acidic particles. Geophysical Research Letters 2003, 30, (19), 1996, doi:10.1029/2003GL017738. Galloway, M. M.; Chhabra, P. S.; Chan, A. W. H.; Surratt, J. D.; Flagan, R. C.; Seinfeld, J. H.; Keutsch, F. N., Glyoxal uptake on ammonium sulphate seed aerosol: reaction products and reversibility of uptake under dark and irradiated conditions. Atmospheric Chemistry and Physics 2009, 9, (10), 3331-3345. Noziere, B.; Dziedzic, P.; Cordova, A., Products and kinetics of the liquid-phase reaction of glyoxal catalyzed by ammonium ions (NH4+). Journal of Physical Chemistry A 2009, 113, (1), 231-237. Laskin, J.; Laskin, A.; Roach, P. J.; Slysz, G. W.; Anderson, G. A.; Nizkorodov, S. A.; Bones, D. L.; Nguyen, L. Q., High-resolution desorption electrospray ionization mass spectrometry for chemical characterization of organic aerosols. Analytical Chemistry 2010, 82, (5), 2048-2058. Updyke, K. M.; Nguyen, T. B.; Nizkorodov, S. A., Formation of brown carbon via reactions of ammonia with secondary organic aerosols from biogenic and anthropogenic precursors. Atmos Environ 2012, 63, 22-31. Bones, D. L.; Henricksen, D. K.; Mang, S. A.; Gonsior, M.; Bateman, A. P.; Nguyen, T. B.; Cooper, W. J.; Nizkorodov, S. A., Appearance of strong absorbers and fluorophores in limonene-O3 secondary organic aerosol due to NH4+-mediated chemical aging over long time scales. Journal of Geophysical Research 2010, 115, (D5), D05203, doi:10.1029/2009JD012864. Nguyen, T. B.; Lee, P. B.; Updyke, K. M.; Bones, D. L.; Laskin, J.; Laskin, A.; Nizkorodov, S. A., Formation of Nitrogen- and Sulfur-Containing Light-Absorbing Compounds Accelerated by Evaporation of Water from Secondary Organic Aerosols. Journal of Geophysical Research 2011, 117, D01207, doi: 10.1029/2011JD016944. Shapiro, E. L.; Szprengiel, J.; Sareen, N.; Jen, C. N.; Giordano, M. R.; McNeill, V. F., Light-absorbing secondary organic material formed by glyoxal in aqueous aerosol mimics. Atmospheric Chemistry and Physics 2009, 9, (7), 2289-2300. Trainic, M.; Riziq, A. A.; Lavi, A.; Flores, J. M.; Rudich, Y., The optical, physical and chemical properties of the products of glyoxal uptake on ammonium sulfate seed aerosols. Atmospheric Chemistry and Physics 2011, 11, (18), 9697-9707. De Haan, D. O.; Corrigan, A. L.; Smith, K. W.; Stroik, D. R.; Turley, J. J.; Lee, F. E.; Tolbert, M. A.; Jimenez, J. L.; Cordova, K. E.; Ferrell, G. R., Secondary Organic Aerosol-Forming Reactions of Glyoxal with Amino Acids. Environmental Science and Technology 2009, 43, (8), 2818-2824. Jacobson, M. Z., Isolating nitrated and aromatic aerosols and nitrated aromatic gases as sources of ultraviolet light absorption. Journal of Geophysical Research 1999, 104, (D3), 3527-3542, doi:10.1029/1998JD100054. Liu, S.; Shilling, J. E.; Song, C.; Hiranuma, N.; Zaveri, R. A.; Russell, L. M., Hydrolysis of Organonitrate	proposed to be cited without priotirising these. Several of these are published before AR4 and/or are not relevant in the paragraph. We have added the Shapiro et al reference. References on OA have been revised to cite more recent work.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Functional Groups in Aerosol Particles. Aerosol Science and Technology 2012, 46, (12), 1359-1369. Lu, J. W.; Flores, J. M.; Lavi, A.; Abo-Riziq, A.; Rudich, Y., Changes in the optical properties of benzo a pyrene-coated aerosols upon heterogeneous reactions with NO2 and NO3. Physical Chemistry Chemical Physics 2011, 13, (14), 6484-6492. Zarzana, K. J.; De Haan, D. O.; Freedman, M. A.; Hasenkopf, C. A.; Tolbert, M. A., Optical Properties of the Products of alpha-Dicarbonyl and Amine Reactions in Simulated Cloud Droplets. Environmental Science & Technology 2012, 46, (9), 4845-4851. Noziere, B.; Dziedzic, P.; Cordova, A., Formation of secondary light-absorbing "fulvic-like" oligomers: a common process in aqueous and ionic atmospheric particles? Geophysical Research Letters 2007, 34, (21), L21812, doi:10.1029/2007GL031300. [Government of United States of America]	
7-890	7	29	21	29	24	The following paper gives a further example of description of VOCs in a regional model: Athanasopoulou, E., Vogel, H., Vogel, B., Tsimpidi, A., Pandis, S. N., Knote, C., and Fountoukis, C.: Modeling meteorological and chemical effects of secondary organic aerosol during an EUCAARI campaign, Atmos. Chem. Phys. Discuss., 12, 21815-21865, doi:10.5194/acpd-12-21815-2012, 2012. [Andrew Ferrone, Germany]	Rejected. The reviewer does not provide a reason for citing this paper.
7-891	7	29	21			what is "multigenerational oxidation"? [Andrea Flossmann, France]	Rejected. Details are available in the cited references. Hence not repeated due to space limitation.
7-892	7	29	22			.. where semi- or non-volatile organic compounds (SVOC) are produced: replace where semi-volatile or low-volatility compounds (SVOC and LVOC, respectively) are produced. [Urs Baltensperger, Switzerland]	Accepted. Text has been changed.
7-893	7	29	24	29	24	(section 7.3.3.1) "Tsigaridis and Kanakidou, 2003" You might consider changing this reference to a newer one in which glyoxal chemistry and BSOA formation is incorporated in a global model: Myriokefalitakis, K. Tsigaridis, N. Mihalopoulos, J. Sciare, A. Nenes, K. Kawamura, A. Segers, and M. Kanakidou. In-cloud oxalate formation in the global troposphere: a 3-D modeling study. S. Atmos. Chem. Phys., 11, 5761-5782, 2011. http://www.atmos-chem-phys.net/11/5761/2011/acp-11-5761-2011.pdf [Erik Swietlicki, Sweden]	Partly taken into account. References on OA have been revised to cite more recent work.
7-894	7	29	29	29	38	This paragraph is not linked to the rest of the chapter. Why is internal mixing important? There is an answer about how adding, e.g., nitrate has different forcing depending on whether one adds nitrate to organic particles or one makes new nitrate particles. But this would take a lot more explanation. Also, this is just in my specialty and I have to say that the choice of references is not the best. My suggestion is not to expand this but just delete to shorten the chapter. [Daniel Murphy, United States of America]	Taken into account. State of mixing is extremely important to assess aerosol climate impact and hence cannot be deleted. A bit more explanation has been provided.
7-895	7	29	29	29	38	This section is supposed to discuss both composition and mixing state but instead mostly on the former. There are several recent attempts in global models to predict the mixing state of internal mixtures, including Kim et al. (2008; JGR, 113, D16309) and Bauer et al., (2008; ACP, 8, 6003-6008), rather than adopting the common assumption of either all external or all well-mixed internalmixtures, these works have tried to explicitly predict evolution of core-shell mixtures and differentiate it with other types of external or internal mixitures. I don't understand why these works have not been mentioned in this assessment report, even in this place supposed to discuss progresses and processes on this regard. [Chien Wang, United States of America]	Taken into account. A statement has been added and a couple of references have been added.
7-896	7	29	30			There are more data on external vs. internal mixtures (e.g., Cheng, Y.-F., Eichler, H., Wiedensohler, A., Heintzenberg, J., Zhang, Y.-H., Hu, M. and co-authors 2006. Mixing State of Element Carbon and Non-Light-Absorbing Aerosol Components Derived from in Situ Particle Optical Properties at Xinken in Pearl River Delta of China. J. Geophys. Res. 111, doi: 10.1029/2005JD006929. Okada, K. and Heintzenberg, J. 2003. Size Distribution, State of Mixture and Morphology of Urban Aerosol Particles at Given Electrical Mobilities. J. Aerosol Sci. 34, 1539-1553). [Jost Heintzenberg, Germany]	Taken into account We need to make a selection of studies here. The text makes it clear now that this is only a selection.
7-897	7	29	35	29	35	The Asmi et al, 2011, reference is to wrong Asmi et al paper that year. Correct is Atmospheric Chemistry and Physics, 11(11):5505–5538. [Ari Asmi, Finland]	Accepted. This comment refers to line 45. We corrected the mistake.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7-898	7	29	35	29	37	consider rephrasing this sentence [Peter Irvine, Germany]	Taken into account. Sentence was removed as this is mentioned earlier in the text.
7-899	7	29	37			should be "particles" instead of "particle" [Andrea Flossmann, France]	Rejected. Correct as it is.
7-900	7	29	40			Mention should be made of the observed values of scattering efficiency as done by Warrren White (1986); http://vista.cira.colostate.edu/improve/publications/Principle/NAPAP_SOS/Low%20Res/Chapter4.pdf This discussion of observed scattering efficiencies is missing from Ch.2 as well and would allow a comparison with models. [Robert Charlson, United States of America]	Partly taken into account. Scattering efficienccy is now mentioned, but there is little space to go into details.
7-901	7	29	42	29	42	Delete "one of". [Steven Ghan, United States of America]	Accepted. Statement revised.
7-902	7	29	42	29	43	"CCN properties" does not go along with "spectral optical properties" here. CCN is a subset of aerosols. Perhaps "activation probability" is a better term? [Chien Wang, United States of America]	Partly taken into account. "spectral" has been deleted.
7-903	7	29	42	30	7	These paragraphs are review more than assessment. Lines 42-49 could be deleted to shorten the chapter. [Daniel Murphy, United States of America]	Rejected. Sentences are retained.
7-904	7	29	42	30	18	This section on size distribution and particle properties might also mention the contributions satellites are now making. Under favorable retrieval conditions, MODIS provides fine-mode-fraction over water (e.g., Levy et al., ACP 2010), POLDER retrieves total and fine-mode AOD over land and water (e.g., Tanré et al., AMT 2011), MISR obtains aerosol type, based on the combination of size, shape, and SSA constraints (e.g., Kahn et al., JGR 2010). In general, total-column AOD must exceed about 0.15 or 0.2 for aerosol property retrievals from these instruments to be reliable. The results amount to an aerosol type classification that complements the much more detailed by far less extensive suborbital aerosol property products. Despite the limitations of the current operational satellite products, what I think is most important for the discussion here is the potential to map aerosol type over the entire globe, even with existing satellite data, based on the demonstrated ability to do so regionally (e.g., Dubovik et al., AMT 2011 for POLDER; Kahn and Limbacher, ACP 2012 for MISR), and the ability to do so in yet greater detail with next-generation instruments. [I can provide more input on the satellite contributions if you wish.] [Ralph Kahn, United States of America]	Taken into account. A statement on satellite retrieval of size distribution is added.
7-905	7	29	42			Section 7.3: Should be: parameters [Government of Poland]	Accepted. Editorial.
7-906	7	29	43	29	46	True; yet there are some information available from south Asia, which is worth mentioning.This further supports the point above(12). [K KRISHNA MOORTHY, INDIA]	Agreed but no change is made.
7-907	7	29	45			Asmi et al., 2011 is mentioned in the text. However, in the reference list a wrong Asmi et al. is mentioned should be Asmi A et al. (page 65) [European Union]	Accepted. Reference corrected.
7-908	7	29	46	29	49	This sentence should be rewritten because it contains two contradictory statements. [European Union]	Agreed and statement revised.
7-909	7	29	46			For the global marine aerosol a model/data comparison has bee done by Spracklen et al. (Spracklen, D., Pringle, K., Carslaw, K., Mann, G., Manktelow, P. and Heintzenberg, J. 2007. Evaluation of a Global Aerosol Microphysics Model against Size-Resolved Particle Statistics in the Marine Atmosphere. Atmos. Chem. Phys. 7, 2073-2090). [Jost Heintzenberg, Germany]	Noted, but we can only cite a subset of available references.
7-910	7	29	47	29	49	The reference to Smirnov et al, 2011 discusses ship-based sun photometer measurements rather than direct in situ measurements, and should be cited next to Dubovik et al. 2006 on line 47 rather than with the in situ papers on line 49. [Ralph Kahn, United States of America]	Taken into account. Reference removed.
7-911	7	29	51	30	18	You should also mention the asymmetry parameter. I would recommend mentioning the work by NOAA ESRL group they have been montoring for years scattering and absorption as well as asymetry paremeter (for dry aerosols) at the surface for years. These data have been unfortuanetly overlooked by modelers. The Andrews et al. (2011) provides a climatology of these properties at a dozen sites spread in the northwern hemisphere. Reference: E. Andrews, J.A. Ogren, P. Bonasoni, A. Marinoni, E. Cuevas, S. Rodríguez, J.Y. Sun, D.A. Jaffe, E.V. Fischer, U. Baltensperger, E. Weingartner, M. Collaud Coen, S. Sharma, A.M. Macdonald, W.R. Leaitch, N.-H. Lin, P. Laj, T. Arsov, I. Kalapov, A. Jefferson, P. Sheridan, Climatology of aerosol radiative properties in the free troposphere, Atmospheric Research, Volume 102, Issue 4, December 2011, Pages 365-393, ISSN 0169-8095, 10.1016/j.atmosres.2011.08.017. [Paul Ginoux, United States of America]	Rejected. Asymmetry parameter is not discussed due to space limitation. Suggested reference is cited appropriately in another paragraph.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7-912	7	29	53			One should be a bit more specific here. In the field, aerosol in-situ measurements are often performed under dry conditions (relative humidity RH<30–40%). Since ambient aerosol particles experience hygroscopic growth at enhanced RH, their microphysical and optical properties – especially the aerosol light scattering – are also strongly dependent on RH. Measurements with a humidified nephelometer.allow to determine this scattering enhancement for a given RH, which strongly depends on aerosol type. See e.g. Zieger, P., et al., 2011. Comparison of ambient aerosol extinction coefficients obtained from in-situ, MAX-DOAS and LIDAR measurements at Cabauw, Atmos. Chem. Phys. 11, 2603-2624. [Urs Baltensperger, Switzerland]	Rejected. Space limitation prevents us from a detailed description.
7-913	7	29	56			A better way to say this that more closely matches what the author wrote is "in some (but not all) regions, produced by an overestimate of…" [Government of United States of America]	Accepted. Statement revised.
7-914	7	29	57			AOD appears to be used only to pertain to the visible spectrum in this section, so the authors should consider defining it that way. [Government of United States of America]	Accepted. Revised and defined at mid-visible wavelength.
7-915	7	29		30		There have been numerous studies reporting progress in deriving aerosol absorptions based on data fusion by combining multi-platform satellite- and ground-based observations since AR4. I believe such progress needs be reported in AR5. Here are some references: a) Lee, K. H., Z. Li, M. S. Wong, J. Xin, Y. Wang, W.-M. Hao, and F. Zhao (2007), Aerosol single scattering albedo estimated across China from a combination of ground and satellite measurements, J. Geophys. Res., 112, D22S15, doi:10.1029/2007JD009077., b) Jeong, M.-J. and N. C. Hsu (2008), Retrievals of aerosol single-scattering albedo and effective aerosol layer height for biomass-burning smoke: Synergy derived from “A-Train” sensors, Geophys. Res. Lett., 35, L24801, doi:10.1029/2008GL036279., c) Zhu, L., J. V. Martins, and L. A. Remer (2011), Biomass burning aerosol absorption measurements with MODIS using the critical reflectance method, J. Geophys. Res., 116, D07202, doi:10.1029/2010JD015187., d) Wells, K. C., J. V. Martins, L. A. Remer, S. M. Kreidenweis, and G. L. Stephens (2012), Critical reflectance derived from MODIS: Application for the retrieval of aerosol absorption over desert regions, J. Geophys. Res., 117, D03202, doi:10.1029/2011JD016891. More references are therein. [Myeong-Jae Jeong, Republic of Korea]	Rejected. A long list of references is suggested but we have to make a selection of most suitable references here.
7-916	7	30	5			The authors should consider replace "7.3.1.2)" wiith "7.3.1.2 and Mishchenko et al. 2009)" [Government of United States of America]	Rejected. There is a discussion in 7.3.1.2 already.
7-917	7	30	6	30	25	You might consider including the work of Barahona and Nenes (ACP 2009a;b) in this section. [Ralph Kahn, United States of America]	Rejected. The paper was considered but we have to make a selection of studies. One of this paper is already cited in the section.
7-918	7	30	9	30	10	(section 7.3.3.2) “Aerosol absorption is another key climate-relevant aerosol property. Earlier in-situ methods to measure absorption suffered from important uncertainties (Moosmüller et al., 2009).” This is partly due to the lack of proper reference material for instrument calibration and development. This is addressed in this reference: D. Baumgardner, et al., Soot reference materials for instrument calibration and intercomparisons: a workshop summary with recommendations. Atmos. Meas. Tech., 5, 1869-1887, 2012. http://www.atmos-meas-tech.net/5/1869/2012/amt-5-1869-2012.pdf [Erik Swietlicki, Sweden]	Accepted. The suggested reference has been cited.
7-919	7	30	10	30	10	What is meant by 'important uncertainities'? Please include clarification. [Government of India]	Taken into account. "important" is replaced with "significant"
7-920	7	30	11	30	11	At the end of this line, add "Derivation of aerosol absorption or aerosol single scaterring albedo over large regions is feasible with the combination of atmospheric transmittance and reflectance measured by ground-based and space-borne sensors as was done across China (Lee et al. 2010)." [Zhanqing Li, United States of America]	Accepted. Suggested reference is cited appropriately.
7-921	7	30	12	30	12	"enhance" should be "enhances". [Steven Ghan, United States of America]	Accepted. Editorial.
7-922	7	30	12	30	13	Enhancement of absorption efficiency by coating of organic material over BC has been challenged in a recent filed study by Cappa et al , where field observations suggested much lower enhancement factor. Cappa, C. D.; Onasch, T. B.; Massoli, P.; Worsnop, D. R.; Bates, T. S.; Cross, E. S.; Davidovits, P.; Hakala, J.; Hayden, K. L.; Jobson, B. T.; Kolesar, K. R.; Lack, D. A.; Lerner, B. M.; Li, S. M.; Mellon, D.; Nuaaman, I.; Olfert, J. S.; Petaja, T.; Quinn, P. K.; Song, C.; Subramanian, R.; Williams, E. J.; Zaveri, R. A., Radiative Absorption	Taken into account. A statement based on this study has been included but with some reservations.

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						Enhancements Due to the Mixing State of Atmospheric Black Carbon. Science 2012, 337, (6098), 1078-1081. [Government of United States of America]	
7-923	7	30	12	30	13	(section 7.3.3.2) "Coating of soluble material over a primary aerosol such as a BC or dust core enhance the BC mass absorption efficiency by up to a factor of 2." There is a new study that claims otherwise: Christopher D. Cappa et al. Radiative Absorption Enhancements Due to the Mixing State ofAtmospheric Black Carbon. Science 337, 1078 (2012). DOI: 10.1126/science.1223447 [Erik Swietlicki, Sweden]	Taken into account. A statement based on this study has been included.
7-924	7	30	12	30	13	please delete dust in this sentence, otherwise it makes complicated to understand why this should enhance the BC mass abosprion [Elisabetta Vignati, Italy]	Accepted. Corrected.
7-925	7	30	13	30	13	I'm missing the following reference here: Schnaiter, M., C. Linke, O. Möhler, K. H. Naumann, H. Saathoff, R. Wagner, U. Schurath, and B. Wehner (2005), Absorption amplification of black carbon internally mixed with secondary organic aerosol, J. Geophys. Res., 110(D19). [Ottmar Möhler, Germany]	Rejected. Reference is relatevely older. Newer references are cited already.
7-926	7	30	14	30	15	The AERONET sky scan retrievals of aerosol absorption properties are considered of high quality only when the solar zenith angle is greater than 50° and the AOD at 440 nm is 0.4 or above [Dubovik et al., JGR 2000]. Another important subtlety is that although AERONET retrievals identify two size modes, they include only a single set of "column effective" indices of refraction; typically, in the common situation when there are two modes in the column, is actually be significantly more absorbing than the other. [Ralph Kahn, United States of America]	Taken into account. Corrected the mistake on AOD threshold value. Suggested reference cited. Detailed discussion on this topic was not possible due to space limitation.
7-927	7	30	15	30	15	The value of 0.2 is not substantiated, and the Dubovik paper that describes the inversion algorithm gives a threshold value of 0.4 at 440 nm wavelength. [John Ogren, United States of America]	Accepted. Corrected the mistake on AOD threshold value.
7-928	7	30	15	30	15	About the expression:"…in situations were AOD were larger than 0.2". This value is only valid for a given wavelength. So, it must be specified here and in the rest of this Report. [Rubén D Piacentini, Argentina]	Accepted. Wavelength is provided.
7-929	7	30	15			The authors should consider replacing "...where AOD is larger than 0.2." with "...where AOD is larger than 0.2 (Dubovik et al., 2002)." [Government of United States of America]	Accepted. Corrected the mistake on AOD threshold value (0.4 rather than 0.2 as indicated in the comment).
7-930	7	30	16	30	16	"aerosol AOD" should be "absorption AOD". [Steven Ghan, United States of America]	Accepted. Corrected.
7-931	7	30	18			The authors should consider adding the following sentence after "...climatology." : "Global satellite measurements of aerosol absorption would be extremely valuable, but remain a major challenge (e.g., Mishchenko et al., 2012)." [Government of United States of America]	Rejected. IPCC does not make recommendations for research. The reference is useful and cited elsewhere though.
7-932	7	30	20	30	20	CCN in section title is acceptable but some other abbreviations are less well known [Arnoud Apituley, The Netherlands]	Noted
7-933	7	30	20	30	20	Again, "CCN Properties of Atmospheric Aerosols" should be simply replaced by "CCN" or "Cloud Condensation Nuclei (CCN)". [Chien Wang, United States of America]	Changed
7-934	7	30	22	30	22	replace "acts" with "act" [Peter Irvine, Germany]	Rejected. It's correct as is
7-935	7	30	22	32	57	Please spell out all Rfari, Afari, Afaci,….. Otherwise chapter reading is very confusing [Elisabetta Vignati, Italy]	Taken into account. Done where appropriate.
7-936	7	30	23	30	26	consider rephrasing this sentence [Peter Irvine, Germany]	Done
7-937	7	30	25	30	25	What is 'Rfari'? Please include text to explain this term [Government of India]	Taken into account. Spelled out but Rfari is defined in chapter.
7-938	7	30	26	30	26	Please add: In 2011, researchers demosnstrated for the first time that the particles found in cloud water collected over the central Arctic Ocean behave as polymer nano-and microgels (marine gels) and originate in the upper most surface waters from biological secretions as of the activity of sea-ice algae, phytoplankton and bacteria (Orellana et al. 2011). REF: Orellana M.V., P.A. Matrai, C. Leck, C. D. Rauschenberg, A. M. Lee, and E. Coz 2011, Marine microgels as a source of cloud condensation nuclei in the high Arctic, PNAS, 108 (33): 13612-13617. [Caroline Leck, Sweden]	Accepted. Reference added.

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7-939	7	30	26	30	26	please delete the reference to table 7.3, no apparent link with the table in the text [Elisabetta Vignati, Italy]	Taken into account. TSU has made a mistake when renumbering Tables.
7-940	7	30	30	30	30	For clarify insert "of surfactants" after "partitioning". [Steven Ghan, United States of America]	done
7-941	7	30	32	30	34	In most cases the term "CCN" is referred to hygroscopic aerosols in cloud microphysics, thus size would certainly become the primary concern. It could be argued that for low hygroscopic aerosols as far as they are large enough, the activattion is still possible. However, such arguement ignores the fact that the required supersaturation is nearly impossible to form in real atmosphere as far as highly hygroscopic particles in presence. [Chien Wang, United States of America]	noted but no change is made to the text.
7-942	7	30	34	30	36	Delete to shorten chapter. One interesting but speculative result on hydrogels is not worth the space. [Daniel Murphy, United States of America]	Rejected. We kept that, but added a second reference.
7-943	7	30	36	30	36	Recent studies (Orellana et al, 2011) have shown that marine microgels play an important role in regulating ocean basinscale biogeochemical dynamics. In this paper, it is demonstrate that, in the high Arctic, marine gels with unique physicochemical characteristics originate in the organic material produced by ice algae and/or phytoplankton in the surface water. The polymers in this dissolved organic pool assembled faster and with higher microgel yields than at other latitudes. The reversible phase transitions shown by these Arctic marine gels, as a function of pH, dimethylsulfide, and dimethylsulfoniopropionate concentrations, stimulate the gels to attain sizes below 1 μm in diameter. These marine gels were identified with an antibody probe specific toward material from the surface waters, sized, and quantified in airborne aerosol, fog, and cloud water, strongly suggesting that they dominate the available cloud condensation nuclei number population in the high Arctic (north of 80°N) during the summer season. Knowledge about emergent properties of marine gels provides important new insights into the processes controlling cloud formation and radiative forcing, and links the biology at the ocean surface with cloud properties and climate over the central Arctic Ocean and, probably, all oceans.This reference should be cited: Orellana, M. n. V., Matrai, P. A., Leck, C., Rauschenberg, C. D., Lee, A. M. and Coz, E. (2011). Marine microgels as a source of cloud condensation nuclei in the high Arctic.Proccedings of the Nacional Academy of Sciences of USA (PNAS), doi: 10.1073/pnas.1102457108. [BEGONA ARTINANO, SPAIN]	Accepted. Reference added
7-944	7	30	36	30	36	Please replace "(hydrogels)" with "(marine gels)" and insert Orrelana et al., 2011 before Ovanevait et al., 2011 .REF: Orellana M.V., P.A. Matrai, C. Leck, C. D. Rauschenberg, A. M. Lee, and E. Coz 2011, Marine microgels as a source of cloud condensation nuclei in the high Arctic, PNAS, 108 (33): 13612-13617. [Caroline Leck, Sweden]	Accepted. Reference added.
7-945	7	30	38	30	39	(section 7.3.3.3) "The bulk hygroscopicity parameter "kappa"(Petters and Kreidenweis, 2007), has been introduced to provide a concise way to describe how effectively an aerosol particle functions as a CCN." This relates also to what is said in the previous paragraph that "The ability of an aerosol particle to take up water and subsequently activate is determined by its size and composition." In fact, what determines the CCN activity is the number of water-soluble entities (molecules and ions) that are present in the dry particle. This is expressed as the hygroscopicity parameter kappa, which is a scaled quantity of the number of water-soluble entities. See: Rissler, J., et al., An evaluation and comparison of cloud condensation nucleus activity models: Predicting particle critical saturation from growth at subsaturation, J. Geophys. Res., 115, D22208, doi:10.1029/2010JD014391, 2010. [Erik Swietlicki, Sweden]	Accepted. Reference added
7-946	7	30	38	30	48	The fact that atmospheric OA has a surprisingly small variability in the hygroscopicity parameter kappa (around 0.15) is worth mentioning here. See for example, the results of Engelhart et al. (ACP, 12, 7285-7293, 2012) and more importantly the references therein. [European Union]	Rejected. Not added because we do not refer to the kappa values of inorganics either
7-947	7	30	38	30	48	Hygroscopicity is described by Kappa value. It would be good give clear explanation what does it mean [European Union]	Rejected. It is explained in words and the reference where the formula is being discussed is cited
7-948	7	30	46	30	48	Delete to shorten chapter. The adsorption theory isn't necessary. One interesting but speculative result on hydrogels is not worth the space. [Daniel Murphy, United States of America]	Rejected. The sentence is kept as it is an interesting development.
7-949	7	30	46			The number of studies that have found closure should match the number of studies that did not find closure.	Accepted. The Kammermann reference has been

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						Add e.g. Jurányi, Z., et al., Measured and modelled cloud condensation nuclei concentration at the high alpine site Jungfraujoch, Atmos. Chem. Phys., 10, 7891-7906, 2010, and/or Kammermann, L., 2010. Subarctic atmospheric aerosol composition: 3. Measured and modeled properties of cloud condensation nuclei, J. Geophys. Res., 115, D04202, doi:10.1029/2009JD012447. [Urs Baltensperger, Switzerland]	added.
7-950	7	30	48	30	48	Please insert "marine" before gel-like. [Caroline Leck, Sweden]	Taken into account. The sentence has been deleted
7-951	7	30	50	30	50	IN in section title is not acceptable. The abbreviation needs to be explained in the text that follows immediately after the title, which it is not. [Arnoud Apituley, The Netherlands]	Accepted. Spelled out
7-952	7	30	50	30	50	Better to use expended form than 'IN' in the sub-title [Government of India]	Done
7-953	7	30	50	30	50	I think the acronym "IN" was defined as "ice nuclei" before. Here and in the following sections the acronym is used for both "ice nuclei" and "ice nucleation", which are different things. Though it is mostly clear from the context what IN refers to I would recommend to use IN either as acronym for "ice nuclei" or as acronym for "ice nucleation", but not for both. [Ottmar Möhler, Germany]	Accepted. Changed so that IN only refers to ice nuclei
7-954	7	30	50	30	50	Suggest to change the title to "IN" or "Ice Nuclei (IN)". [Chien Wang, United States of America]	Done where appropriate
7-955	7	30	50			Better not to use an acronym (IN) in the title here. In fact, it's better not to use IN at all as it's unnecessary and "ice nuclei" really doesn't take up that much room. Much easier for the reader if it is spelt out in full each time. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted. Spelled out in title
7-956	7	30		7	30	Section 7.3.3.3. Should there be also a mention of the nitric acid effect on the cloud activation properties? A relevant recent GCM simulation on this would be Makkonen et al, Atmos. Chem. Phys., 12, 7625-7633, doi:10.5194/acp-12-7625-2012, 2012. [Ari Asmi, Finland]	Accepted. We now explicitly refer to nitric acid as a CCN.
7-957	7	31	3			Note temperature regime for mixed phase clouds. [Andrew Gettelman, United States of America]	added
7-958	7	31	6	7	19	This paragraph reads like review. There is no strong connection to climate forcing. One particular problem is the sentence on BC, which is misleading. Cozic et al. actually showed comparable concentrations, not enrichment. Twohy et al. (2010) is a questionable result affected by contamination. Targino did show enrichment. My own results have consistently showed depletion, not enrichment. Rather than go through all the details or promote my own work, I'd just suggest deleting this sentence to shorten the chapter. Note that these lines sort of contradict 7-41 lines 7-8 "The IN ability of BC remains controversial." I like that statement better. [Daniel Murphy, United States of America]	Accepted. Most of the paragraph has been deleted
7-959	7	31	6	31	19	There is a growing amount of literature suggesting that semi-solid/glassy organic aerosols may be significant contributors to IN. [1-8] The observations suggest that ice nucleation may be not necessarily initiated by particles with highest IN propensities present at very low number concentrations, as commonly assumed. Instead, particles with mediocre IN propensity but with high number concentrations can play an equivalently important role. 1. Baustian, K. J.; Wise, M. E.; Jensen, E. J.; Schill, G. P.; Freedman, M. A.; Tolbert, M. A., State transformations and ice nucleation in glassy or (semi-)solid amorphous organic aerosol. Atmos. Chem. Phys. Discuss., 2012, 12, 27333-27366. 2. Knopf, D. A.; Wang, B.; Laskin, A.; Moffet, R. C.; Gilles, M. K., Heterogeneous nucleation of ice on anthropogenic organic particles collected in Mexico City. Geophysical Research Letters 2010, 37, L11803, doi: 10.1029/2010gl043362. 3. Murray, B. J.; O'Sullivan, D.; Atkinson, J. D.; Webb, M. E., Ice nucleation by particles immersed in supercooled cloud droplets. Chemical Society Reviews 2012, 41, (19), 6519-6554. 4. Murray, B. J.; Wilson, T. W.; Dobbie, S.; Cui, Z. Q.; Al-Jumur, S.; Mohler, O.; Schnaiter, M.; Wagner, R.; Benz, S.; Niemand, M.; Saathoff, H.; Ebert, V.; Wagner, S.; Karcher, B., Heterogeneous nucleation of ice particles on glassy aerosols under cirrus conditions. Nature Geoscience 2010, 3, (4), 233-237.	Accepted. Glassy materials are now mentioned

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						5. Schill, G. P.; Tolbert, M. A., Depositional Ice Nucleation on Monocarboxylic Acids: Effect of the O:C Ratio. Journal of Physical Chemistry A 2012, 116, (25), 6817-6822. 6. Wang, B.; Laskin, A.; Roedel, T.; Gilles, M. K.; Moffet, R. C.; Tivanski, A. V.; Knopf, D. A., Microscopy Study Of Heterogeneous Ice Nucleation And Water Uptake Below 273 K on Authentic Atmospheric Particles. . Journal of Geophysical Research, 2012, 117, D00V19, doi: 10.1029/2012JD017446. . 7. Wang, B. B.; Knopf, D. A., Heterogeneous ice nucleation on particles composed of humic-like substances impacted by O-3. Journal of Geophysical Research-Atmospheres 2011, 116, D03205 doi: 10.1029/2010jd014964. 8. Wang, B. B.; Lambe, A. T.; Massoli, P.; Onasch, T. B.; Davidovits, P.; Worsnop, D. R.; Knopf, D. A., The deposition ice nucleation and immersion freezing potential of amorphous secondary organic aerosol: Pathways for ice and mixed-phase cloud formation. Journal of Geophysical Research-Atmospheres 2012, 117, D16209, doI: 10.1029/2012jd018063. [Government of United States of America]	
7-960	7	31	7	31	7	The term "condensation nucleation" is inappropriate here. A few lines above one of the ice nucleation modes was introduced as condensation freezing, not condensation nucleation, which to my understanding is related to the process of purely liquid water condensation without instantaneous freezing. [Ottmar Möhler, Germany]	Sentence deleted
7-961	7	31	9	31	11	(section 7.3.3.4) "Bioaerosols initiate immersion freezing at the highest temperatures and are considered as very efficient IN, but their concentration in the upper troposphere is relatively low (Hoose et al., 2010a)." This is exactly why the source flux of PBAP is very important to constrain (better than 50-1000 Tg/yr in Table 7.2). [Erik Swietlicki, Sweden]	Sentence deleted
7-962	7	31	12	31	13	Siefert et al., JGR 2011 discusses the role of volcanic ash as IN. [Ralph Kahn, United States of America]	Accepted. Volcanic ash has been added
7-963	7	31	13	31	13	Seifert et al., 2010 – there are two more papers by (different) Seifert and the others in the reference list [Government of Poland]	Sentence deleted
7-964	7	31	16	31	17	The speculative nature of the statement is in stark contrast with its assertive tone here ("there must be some mechanism for BC to enter ice clouds, …"). Please re-word. [Yi Ming, United States of America]	Sentence deleted
7-965	7	31	28	31	44	place references in the text rather than in the figure caption; overloads the caption [Andrea Flossmann, France]	Figure deleted
7-966	7	31	42			replace "three papers exists" by "three published references exist" [Andrea Flossmann, France]	Figure deleted
7-967	7	31	42			there are two dashed lines in this figure 7.15; which one are you referring to? [Andrea Flossmann, France]	Figure deleted
7-968	7	31	48	32	23	This section is titled Aerosol-Radiative Interactions following the new terminology introduced by the AR5 and outlined in Figure 7.2 but then the text refers to the Direct Radiative Effect throughout this section. If RFari is broader than just the DRE then this needs to be explained. [Government of United Kingdom of Great Britain & Northern Ireland]	Agreed. Direct radiative effect is now termed "Radiative effect due to aerosol-radiation interactions", or REari, for consistency with the new terminology. To answer the second part of the comment, RFari is essentially the REari exerted by anthropogenic changes in aerosols since pre-industrial times.
7-969	7	31	51			"The DRE .. is close to being an observable quantity" => not sure this is the best expression here. Locally it might be close to observable, but globally it will be never. [Michael Schulz, Norway]	No changes made. The routine global-scale satellite retrievals of aerosol optical depth are based on the physics of REari and have been used in turn to produce global-scale estimates of REari. The adjective "observable" is therefore justified.
7-970	7	31	56	31	57	The effect of host model parameters on forcing uncertainties has been investigated in Stier et al., ACP(D), 2012, radiative transfer schemes have been assessed in Randles et al., ACPD, (2012). The pull through of our improved understanding of such effects to the overall uncertainty discussion in this chapter could be improved. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Agreed. Results of Stier et al., ACP, 2013 are now mentioned in a new sentence at the end of the section.

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7-971	7	31	57	31	57	Please replace atmospheric with vapour. [European Union]	Agreed. However, to avoid any confusion, "atmospheric molecules" has been replaced with "atmospheric gases".
7-972	7	31	59			Should "upscatter fraction" be replaced with "backscatter fraction"? Does this mean that DRE can become positive when less incoming solar radiation is reflected back to space... implying that "backscatter fraction" is meant? [Government of United States of America]	No changes made. Upscatter fraction is the fraction scattered by the aerosol particle into its upper hemisphere and is therefore relevant to REari (e.g. Chylek and Wong, 1995). Both forward- and back-scattered radiation contribute to the upscatter fraction, and their relative contributions depend on the solar zenith angle.
7-973	7	32	1	32	5	The influence of stratospheric aerosols on the earth-atmosphere radiation balance during diffuse/weakening, and multiple tropospause conditions needs further study. [Panuganti, C.S. Devara, India]	No changes made. The section is limited to more general statements.
7-974	7	32	4	32	5	The sea-salt aerosols have also significant positive RF in the longwave spectrum. [Government of Poland]	Agreed. Sea spray is now listed.
7-975	7	32	7	32	23	In this section some results of longwave radiative forcing should be added as well as a few references, for example: Ritter, C et al.. 2005, Markowicz et al.., 2003, Vogelmann et al.., 2003. [Government of Poland]	Agreed. A sentence on longwave REari has been added, using the more recent Bharmal et al. (2009) study for reference.
7-976	7	32	12	32	16	See my general note on the chapter of defining clear-sky DRE as including the clear-sky fraction or not The global ocean DRE is definitely not -4 to -6 W m-2 if the cloud fraction is included. Alternatives are to multiply by the clear-sky fraction to get a number about half this, or note in the text that this is the average for clear locations and the actual average is much less. I'm not sure which is better. [Daniel Murphy, United States of America]	The sentence has been modified slightly to clarify further that the estimates given are for cloud-free oceans only. The fact that including cloudy regions would make the estimates weaker is already stated in the previous paragraph.
7-977	7	32	14	32	14	After "locally." Add "Much progress has been made since AR4 in quantifying regional DRE and associated aerosol properties in some aerosol-laden regions such as east Asia, thanks to some major field experiments (Li et al. 2007, 2011a), which helps reduce uncertainties in the estimates of global DRE. [Zhanqing Li, United States of America]	No changes made. The references are relevant, but several examples of local studies pertaining to specific uncertain parameters in global estimates are already given.
7-978	7	32	14	32	15	"Estimates over land are more difficult as the surface is less well characterised" - this is not only a retrieval problem, as stated, but also reflected in the large albedo differences in the models used for forcing calculations. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	No changes made. The statement is made in the context of observational estimates. Consequences for model estimates are mentioned in a new sentence at the end of the paragraph, although not at this level of details.
7-979	7	32	15			Quote also Bierwirth, E., Wendisch, M., Ehrlich, A., Heese, B., Tesche, M., Althausen, D. and co-authors 2009. Spectral Surface Albedo over Morocco and Its Impact on the Radiative Forcing of Saharan Dust. Tellus B 61, 252-269. [Jost Heintzenberg, Germany]	No changes made. Although the suggested study is partly relevant, it is less focused on the problem of characterising surface reflectance in satellite aerosol retrievals than the studies already cited.
7-980	7	32	19	32	19	Before "Notable", add "Aersol absorprtion in a cloudy atmospheric column can be estimated from coincident surface and satellite measurementsin the visible spectral region where clouds have negligibal absoprtion (Li and Kou, 1998) [Zhanqing Li, United States of America]	No changes made. Different methods can be used to estimate aerosol absorption, and details are too technical for this report.
7-981	7	32	19	32	20	Ming et al. (2005) found that the DRE of organic aerosols could be positive over the Himalaya mountains and polar areas. Please cite the paper: Ming, Y., V. Ramaswamy, P. A. Ginoux, and L. W. Horowitz (2005), Direct radiative forcing of anthropogenic organic aerosol,J. Geophys. Res., 110, D20208, doi:10.1029/2004JD005573. [Yi Ming, United States of America]	No changes made. As noted in the suggested study, the existence of large positive radiative effects in the Himalaya depends on assumptions in organic aerosol refractive index, and is therefore not as well established as over the Atlantic stratocumulus deck and the Arctic.
7-982	7	32	19	32	20	In the sentence: "Notable areas of positive top-of-atmosphere DRE exerted by absorbing aerosols include the Arctic over ice surfaces (Stone et al., 2008) and seasonally off the shore of Namibia over stratocumulus clouds.", there is no reference to the last assessment: "and seasonally off the shore of Namibia over stratocumulus	Agreed. Chand et al. (2009) and de Graaf et al. (2012) are cited as references.

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						clouds.". Please, include the corresponding reference. [Rubén D Piacentini, Argentina]	
7-983	7	32	20	32	20	Please cite a reference for absorbing aerosol effect over Namibian stratocumulus. [Anthony Del Genio, United States of America]	Agreed. Chand et al. (2009) and de Graaf et al. (2012) are cited as references.
7-984	7	32	20	32	20	Mentioning clouds off the shore of Namibia is a little strange here (it appears as a very localized phenomenon). In any case a reference is needed. [European Union]	Agreed. "Off the shore of Namibia" has been replaced by "southeastern Atlantic" to highlight the large area concerned. Chand et al. (2009) and de Graaf et al. (2012) are cited as references.
7-985	7	32	20	32	20	Provide a reference for the positive DRE over Namimbia stratocumulus clouds, for example, Peters, K., Quaas, J., and Bellouin, N.: Effects of absorbing aerosols in cloudy skies: a satellite study over the Atlantic Ocean, Atmos. Chem. Phys., 11, 1393-1404, doi:10.5194/acp-11-1393-2011, 2011. [Lazaros Oreopoulos, United States of America]	Agreed, but Chand et al. (2009) and de Graaf et al. (2012) have been used instead.
7-986	7	32	20			There are no references given for the positive TOA DRE over the stratocumulus deck off the coast of Namibia. A good one to add, illustrating that observation-based estimates are now possible, would be: Chand, D., R. Wood, T. L. Anderson, S. K. Satheesh, and R. J. Charlson, 2009: “Satellite-derived direct radiative effect of aerosols dependent on cloud cover”, Nat. Geosci., doi:10.1038/NGE0437, 2, 181–184, doi:10.1038/ngeo437 [Government of United States of America]	Agreed. Chand et al. (2009) and de Graaf et al. (2012) are cited as references.
7-987	7	32	22	32	22	In addition to Loeb and Su, 2010, it might be appropriate to refernce McComiskey et al., JGR 2008. [Ralph Kahn, United States of America]	Agreed. The reference is added.
7-988	7	32	25	32	25	expand ARI [Peter Irvine, Germany]	Agreed, expanded
7-989	7	32	27	32	31	It is not clear what "adjustments" means. Please explain it [Elisabetta Vignati, Italy]	not expanded here but new box is now refered to
7-990	7	32	28	32	28	delete "the" between "where" and "associated", and add "of aerosols" after "semi-direct effect" [Peter Irvine, Germany]	agreed, text changed
7-991	7	32	29			The important "rapid adjustment" or feedback loop extending from surface wind via dust emission dust radiative transfer, atmospheric stability, surface wind back to dust emissions needs to be mentioned (cf. Heinold, B., Tegen, I., Bauer, S. and Wendisch, W. 2011. Regional Modelling of Saharan Dust and Biomass Burning Smoke - Part 2: Direct Radiative Forcing and Atmospheric Dynamic Response. Tellus 63B, 800-813. Heinold, B., Tegen, I., Schepanski, K. and Hellmuth, O. 2008. Dust Radiative Feedback on Saharan Boundary Layer Dynamics and Dust Mobilization. Geophys. Res. Lett. 35, L09804, doi:09810.01029/02008GL033654). [Jost Heintzenberg, Germany]	considered in Feedback section
7-992	7	32	33	32	33	Insert "observational" before "studies". [Steven Ghan, United States of America]	accepted
7-993	7	32	35	32	37	The following paper also compares the effects of aersols on the 2m temperatures for a cloudy and a cloud-free episode and shows the effect of more complicated pictures in cloudy environments: Vogel, B., Vogel, H., Bäumer, D., Bangert, M., Lundgren, K., Rinke, R., and Stanelle, T.: Thecomprehensive 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]	rejected. Appropraite examples are already given and this paper is more about a modeling system than understanding of the response.
7-994	7	32	36	32	37	Persad et al. (2012) studied the rapid cloud adjustments to absorbing aerosols at different heights, and should be cited here. The reference is Persad, G., Y. Ming, V. Ramaswamy, 2012: Tropical Tropospheric-Only Responses to Absorbing Aerosols. J. Climate, 25, 2471–2480. [Yi Ming, United States of America]	it could be cited but it is an idealised GCM study and really reconfirms earlier work. The current references are examples and apropraitely span the literature. It is added in the next paragraph instead
7-995	7	32	39	32	44	This paragraph deals with the change in the atmospheric stability due to aerosol absorption. In this context, there is a recent observation over India, where the concurrent measurements of BC concentration and environmental lapse rate have provided strong indication of aerosol induced heating leading to changes in the environmental lapse rate in the free and upper troposphere (Babu et al. 2011, GRL). [K KRISHNA MOORTHY, INDIA]	agreed, study now included as observation of static stability change

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7-996	7	32	39	32	44	The article mentioned in the comment above is Babu, S. S., Moorthy K. K., Manchanda,R. K., P. R. Sinha, S. K. Satheesh, D. P. Vajja, S. Srinivasan, and V. H. A. Kumar,2011. Free tropospheric black carbon aerosol measurements using high altitude balloon: Do BC layers build "their own homes" up in the atmosphere?, Geophysical Research Letters, 38: L08803, doi:10.1029/2011GL046654. - To be inserted on Page 65 after line 30. [K KRISHNA MOORTHY, INDIA]	agreed, study now included as observation of static stability change
7-997	7	32	39			Even without clouds the stabilising effect of absorbing aerosol can have severe effects on boundary layer stability and ventialtion of air pollution (cf. Wendisch, M., Hellmuth, O., Ansmann, A., Heintzenberg, J., Engelmann, R., Althausen, D. and co-authors 2008. Radiative and Dynamic Effects of Absorbing Aerosol Particles over the Pearl River Delta, China. Atmos. Environ. 42, 6405-6416). [Jost Heintzenberg, Germany]	agreed, study now included as observation of static stability change
7-998	7	32	40	32	41	Persad et al. (2012) studied the rapid cloud adjustments to absorbing aerosols at different heights, and should be cited here. The reference is Persad, G., Y. Ming, V. Ramaswamy, 2012: Tropical Tropospheric-Only Responses to Absorbing Aerosols. J. Climate, 25, 2471–2480. [Yi Ming, United States of America]	agreed, study now included as it helps explain physical processes
7-999	7	32	40			Dependence on aerosol and cloud altitude was known much earlier than suggested here - e.g. see Cook and Highwood (2004). [Government of United Kingdom of Great Britain & Northern Ireland]	agreed - could go back to Hansen et al. 1997 but only recent studies highlighted here. E.g added though
7-1000	7	32	54	32	54	delete "s" from "enhances" [Peter Irvine, Germany]	agreed edit made
7-1001	7	32	55			Cite e.g., Heintzenberg, J. and Wendisch, M. 1996. On the Sensitivity of Cloud Albedo to the Partitioning of Particulate Absorbers in Cloudy Air. Contr. Atmos. Phys. 69, 491-499). [Jost Heintzenberg, Germany]	many factors influence RF from cloud drop inclusions. This reference is one of these. This paragraph doesn't go into details of the mechaism but refers to global forcing studies only. So the reference is not added
7-1002	7	32		34		Given the spread in forcings of the several constituents among different modeling groups in Fig 7.18, I would have little confidence in the change in any of these forcings with change of climate state. [Stephen E Schwartz, United States of America]	Noted. This is consistent with our conclusion in the synthesis subsection 7.3.5.4.
7-1003	7	33	8	33	10	Transport processes are a main driver of aerosol variability. 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. Env., 44, 3976-3984, 2010. [Loretta Mickley, United States of America]	Rejected. "wind speed" has been listed as one of physical drivers of changes in aerosols, and transport processes are implicitly accounted for in the "etc". Although transport processes drive a lot of the aerosol variability there is no published paper that presents evidence that it is a major driver on the long timescales considered in this section.
7-1004	7	33	9	33	29	"(-10 to -20%) increae or descrease", should be "(10-20%) increase or decrease"? [Chien Wang, United States of America]	Accepted. Changed as suggested.
7-1005	7	33	17	30	25	Any change in wind speed effecting the sea spray source strenght will NOT ONLY give a resonse in sea-salt but also in both primary organic aerosol (POA) and primary biological aerosol particles (PBAP), see comment #12. As of comment #19, in terms of mass, sea-salt seems likely to be major constituent of supermicron aerosol in the marine atmosphere but certainly not in terms of number. As both POA and PBAP in sizes <200 nm diameter (accumulation mode) seems to dominate over sea-salt and they can thus, over remote oceans, relative sea-salt comprimise a significant fraction of the marine CCN population. [Caroline Leck, Sweden]	Noted but no change is made to the text. The text refers to sea spray generically (eg in the section title, lines 12 and 19), which indeed includes marine PBAP. Since available model studies only dealt with sea-salt, the sentence on line 15 refers specifically to sea-salt.
7-1006	7	33	17			attention: on page 26 sea spray depends on stability, here it depends on precipitation; stay coherent!! [Andrea Flossmann, France]	taken into account. "atmospheric stability" has been added here to be consistent with descriptions on page 26.
7-1007	7	33	21	33	25	In recent years, compact, gymbol-mounted, ship-based, coherent Doppler wind lidars have been developed to determine three-dimensional wind vectors over oceans. These lidars work in all-weather conditions and they measure simultaneous aerosols, clouds and wind field (within and outside the coluds) at reasonably good spatiotemporal resolution. Such lidars can be utilized for studying aerosol-cloud interactions over oceans. [Panuganti, C.S. Devara, India]	Noted however no change to the text is made. This paragraph is focusing on sea spray-climate interactions, rather than details of aerosol and wind measurements. Moreoevr IPCC does not make any recommendation for work.
7-1008	7	33	27	33	30	About the sentence: "Studies of the effects of climate change on dust loadings give a wide range of results.	This has been clarified. See responses to comment

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						Woodward et al. (2005) found a factor of 3 increase in dust burden in 2100 relative to present-day because of a large increase in bare soil fraction. A few studies reported moderate (−10 to −20%) increases or decreases (e.g., Jacobson and Streets, 2009; Liao et al., 2009; Tegen et al., 2004).". Note that if you include the word "increases" in the expression "moderate (−10 to −20%) increases or decreases", then at least the higher limit must be possitive in the parenthesis. [Rubén D Piacentini, Argentina]	#1004.
7-1009	7	33	27	33	32	One issue that may be worth mentioning here is the recently suggested effect of increased relative change in polar aerosol concentrations. A moderate rise in emissions in mid- and low latitudes may increase polar concentrations substantially, as proposed in Lambert et al., accepted in Nature Climate Change. [Fabrice Lambert, Republic of Korea]	Rejected. How dust may change in the future is fairly uncertain and the section does not go into the details of regional climate response to the aerosol focing.
7-1010	7	33	29	33	30	This sentence is confusing to us. How can "-10 to -20%" indicate both an increase and a decrease? [Thomas Stocker/ WGI TSU, Switzerland]	This has been clarified. See responses to comment #1004.
7-1011	7	33	29			Typo in parenthesis [Jost Heintzenberg, Germany]	This has been clarified See resonse to comment #1004.
7-1012	7	33	30			An explanation why a double CO2 concentration leads to a drcease in the dust burden should probably be given. [Urs Baltensperger, Switzerland]	Accepted. It is now explained as follows: "Mahowald et al. (2006b) found a 60% decrease under double CO2 concentration due to the effect of CO2 fertilization on vegetation."
7-1013	7	33	31	33	31	some recent work can be cited here on dust and potential role in climate change, change "double CO2 concentration. The large range" to "double CO2 concentration.In a study focusing on dusts influence on Australian rainfall, Rotstayn et al. (2011) found that dust may have important interactions with climate variability, especially on ENSO time-scales and the response of ENSO to climate change is an active research question. The large range". Reference are Rotstayn, L.D., Collier, M.A, Mitchell, R.M., Qin, Y., Campbell, S.K., Dravitzki, S.M., 2011: Simulated enhancement of ENSO-related rainfall variability due to Australian dust. Atmos. Chem. Phys., 11, 6575-6592. [Mark Collier, Australia]	Rejected. The study of Rotstayn et al. (2011) compared two 160-year coupled atmosphere-ocean simulations of MODERN-DAY climate (one with interactive dust and one without dust). This section instead assesses the relevance and strength of aerosol-climate feedbacks on long timescales in the context of future climate change scenarios as stated at the begining of section 7.3.5.
7-1014	7	33	36	33	39	consider rephrasing this sentence [Peter Irvine, Germany]	partly taken into account. The sentence was broken in two parts for clarity.
7-1015	7	33	36	33	40	Over the US, daily variation in meteorology explains up to 50% of the variability in surface aerosol. Temperature, relative humidity, precipitation, and circulation are the most important predictors. Surface concentrations over the US are on average 2.6 mg m -3 higher on stagnant vs. non-stagnant days. 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. Env., 44, 3976-3984, 2010. [Loretta Mickley, United States of America]	Rejected. The study by Tai et al. (2010) looks at daily variations in PM2.5 and meterological parameters. This section instead the relevance and strength of aerosol-climate feedbacks in the context of future climate change scenarios as stated at the begining of section 7.3.5.
7-1016	7	33	36	34	26	There are no references given for the positive TOA DRE over the stratocumulus deck off the coast of Namibia. A good one to add, illustrating that observation-based estimates are now possible, would be: Chand, D., R. Wood, T. L. Anderson, S. K. Satheesh, and R. J. Charlson, 2009: "Satellite-derived direct radiative effect of aerosols dependent on cloud cover", Nat. Geosci., doi:10.1038/NGE0437, 2, 181–184, doi:10.1038/ngeo437 [Government of United States of America]	Rejected. This comment is irrelevant to aerosol response to climate change and feedback. Maybe it is ill placed?
7-1017	7	33	37			Regarding the phrase, "mixed layer depth" is it clear that "ocean mixed layer depth" is meant? This could be interpreted as mixed layer depth in the atmosphere, as well. [Government of United States of America]	Accepted. "mixed layer depth" has been replaced by "ocean mixed layer depth".
7-1018	7	33	39	33	43	Please get the timing of the questioning of CLAW correct: Observations from the Arctic was the first to question the key role attributed to DMS in the CLAW hypothesis (Leck and Bigg 2007). In the emerging picture of the Arctic atmosphere, DMS concentration will determine the mass of the particles by producing material for their growth. But it is the number of airborne microgels (marine gels, see comment #20), dictated by the number of airborne particles originating in the surface microlayer of the ocean, that will primarily influence the	Accepted. The study of Leck and Bigg (2007) is now cited to explain how the original CLAW hypothesis has been challenged.

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						number of CCN and the resulting optical properties of the cloud droplets. Indeed, research during the past two decades – reviewed last year in Nature (Quinn and Bates 2011) – does not corroborate the CLAW hypothesis for other regions as well. REF: Leck, C. and K. Bigg, 2007, A modified aerosol–cloud–climate feedback hypothesis. Environmental Chemistry, 4, 400–403, doi:10.1071/EN07061. Quinn P K and Bates T S (2011) Nature 480: 51-56, doi:10.1038/nature10580. [Caroline Leck, Sweden]	
7-1019	7	33	39	33	43	Please consider the possibility ICE-MICROORGANSM-AEROSOL-CLOUD-FEEDBACK in a future climate, see comment#7 [Caroline Leck, Sweden]	Rejected. There are many possible feedback loops and there needs to be more support in the literature before it can be discussed here.
7-1020	7	33	40	33	40	Consider adding the following detailed study of the CLAW hypothesis in an AOGCM with interactive aerosol cycles and ocean biogeochemistry: Kloster, S., K. D. Six, J. Feichter, E. Maier-Reimer, E. Roeckner, P. Wetzel, P. Stier, and M. Esch (2007), Response of dimethylsulfide (DMS) in the ocean and atmosphere to global warming, J. Geophys. Res., 112, G03005, doi:10.1029/2006JG000224. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted. The paper by Kloster et al. is now cited.
7-1021	7	33	47	33	47	(section 7.3.5.2) "In the atmosphere chemical production of sulfate increases with temperature…" It may be worth noting that there are possibly other important oxidation mechanisms that need to be quantified. R. L. Mauldin et al., A new atmospherically relevant oxidant of sulphur dioxide. Nature 488(2012)193. doi:10.1038/nature11278 [Erik Swietlicki, Sweden]	Rejected. This comment is about the detailed chemistry mechanism of SO2 oxidation, which is more relevant to section 7.3.3.
7-1022	7	33	47	33	48	Over the US, model results indicate that most of the correlations of surface aerosol with temperature do not arise from direct dependence but from covariation with synoptic transport. 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 in the United States: Implications for PM2.5 sensitivity to climage change, Atmos. Chem. Phys., 12, 3131-3145, 2012a. [Loretta Mickley, United States of America]	Rejected. The study by Tai et al. (2010) was looking at daily variations in PM2.5 and meterological parameters. This section deals with the relevance and strength of aerosol-climate feedbacks in the context of future climate change scenarios as stated at the begining of section 7.3.5.
7-1023	7	33	47	33	52	future precipitation increases or decreases? This paragraph is confusing [Andrea Flossmann, France]	Accepted. We have revised the sentence to clarify.
7-1024	7	33	48	33	52	Presumbably the discussed sulfate burden changes reflect the case of constant emissions? This is not entirely clear from the text. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	The revised sentence now describes clearly that sulfate burden changes reflect the case of constant anthropogenic emissions.
7-1025	7	33	54	33	54	Use "vapor pressure" in place of Clausius-Clapeyron to reduce jargon. [Daniel Murphy, United States of America]	Accepted. It has been revised as "Changes in temperature have a large impact on nitrate aerosol formation through shifting gas-particle equilibria". The equilibrium between gas-phase HNO3 and aerosol phase nitrate is sensitive to temperature; warmer future temperatures lead to the shift of gas-aerosol equilibrium toward the gas phase.
7-1026	7	33	54	33	55	(section 7.3.5.2) "Changes in temperature have a large impact on nitrate aerosol formation through the Clausius-Clapeyron relation for HNO3." This is not the whole story. HNO3 is a strong acid that dissociates in aqueous solutions, considerably adding to its solubility. NH3 or other alkaline substances will further help to increase HNO3 partitioning to the particle solution. [Erik Swietlicki, Sweden]	This sentence has been revised. See responses to comment #1025.
7-1027	7	33	55	33	56	(section 7.3.5.2) "…HNO3. There is some agreement among global aerosol models that climate change alone will contribute to a decrease in the nitrate concentrations…" There are still many global models that do not include nitrate. [Erik Swietlicki, Sweden]	Partly taken into account. We added a qualifier to say that this is correct in models that include nitrate aerosols.
7-1028	7	33				Section 7.3.5.1.While mentioning dust aerosols, findings of the following reports also need to be included- Refs:Sharma D, Singh D and Kaskaoutis D G. Impact of Two Intense Dust Storms on Aerosol Characteristics and Radiative Forcing over Patiala, Northwestern India Advances in Meteorology	Rejected. These two studies are not about the dust-climate feedbacks that we are focued on here.

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						Volume 2012 (2012), Article ID 956814, 13 pages. doi:10.1155/2012/956814. Prasad A K and Singh R P. Changes in aerosol parameters during major dust storm events (2001–2005) over the Indo-Gangetic Plains using AERONET and MODIS data. JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 112, D09208, 18 PP., 2007, doi:10.1029/2006JD007778. [Umesh Kulshrestha, India]	
7-1029	7	34	1	34	2	May say "ammonium nitrate" instead of just "nitrate". Ammonium may be the more limiting precursor (Bauer et al., ACP, 2007). Pye et al. (JGR, 2009, 10.1029/2008JD010701) has a good discussion of the future nitrate-ammonium-sulfate system for the US. [Daniel Murphy, United States of America]	Rejected. The Bellouin et al. (2011) study didn't exlicitly considered modelled ammonium. The Pye et al. (2009) study is alredy cited twice.
7-1030	7	34	2	34	2	Bauer et al., 2007 and Shindell et al. 2012 are two other potential references here [Gunnar Myhre, Norway]	Partly taken into account. We have revised the sentence to say that all of these modeling studies (including Bauer et al., 2007) have resported that changes in precursor emissions are likely to increase nitrate concentrations in the future. Shindell et al. (2012) mentioned about nitrate forcing in the future but did not report explicitly the future changes in nitrate burden.
7-1031	7	34	6	34	6	first use of term "carbonaceous" is in this title, should this be introduced earlier? [Peter Irvine, Germany]	Rejected. Section 7.3.1.1 has the definition of carbonaceous aerosols.
7-1032	7	34	8	34	9	"There is evidence that future climate change could lead to increases in the occurrence of wildfires because of changes in fuel availability, readiness of the fuel to burn and ignition sources". These are "indigenous" factors for fireburn. External factors include heat waves which arrive from distant sources, an example being the La-Nina generated NW-SE winds which affected the 2009 Victorian fires. [Andrew Glikson, Australia]	Rejected. Heat waves influence "the readiness of the fuel to burn", which is already account for in this sentence.
7-1033	7	34	8	34	9	"There is evidence that future climate change could lead to increases in the occurrence of wildfires because of changes in fuel availability, readiness of the fuel to burn and ignition sources". These are "indigenous" factors for fireburn. External factors include heat waves which arrive from distant sources, an example being the La-Nina generated NW-SE winds which affected the 2009 Victorian fires. [Government of Australia]	Rejected. This comment is the same as the previous one. See reply to comment #1032.
7-1034	7	34	8	34	10	Using the observed sensitivity of aera burned and meteorology together with climate and chemistry transport models, Spracklen et al. (2009) found that 2000-2050 climate change could increase summertime organic carbon aerosol concentrations over the western United States by 40% and elemental carbon concentrations by 20%. Spracklen, D. V., L. J. Mickley, J. A. Logan. R. C. Hudman, R. Yevich, M. D. Flannigan, and A. L. Westerling, Impacts of climate change from 2000 to 2050 on wildfire activity and carbonaceous aerosol concentrations in the western United States, J. Geophys. Res., 2009. [Loretta Mickley, United States of America]	Partly taken into account. Spracklen et al. (2009) is now cited. Because the results from this study are limited to western United States, we do not describe them in details.
7-1035	7	34	15	34	16	It would be better to have the list in order isoprene, monoterepens and sesquiterpenes [European Union]	Accepted. Changed as suggested.
7-1036	7	34	15	34	26	It is very unclear what will happen for BVOC emissions and concentration in changing climate [European Union]	This is true. It is already said that estimates of future changes in BVOC emissions still have large uncertainties.
7-1037	7	34	17	34	21	Wu et al. (2012) performed a series of model experiments combining a climate model with a dynamic global vegetation model and an atmospheric chemistry transport model. Their results indicate that climate and CO2-induced changes in vegetation led to a 10% increase in global concentrations of secondary organic aerosol by 2050, and a 20% increase by 2010. 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., 12, 15469-15495, 2012. [Loretta Mickley, United States of America]	Accepted Wu et al. (2012) result is within the range of future changes in SOA. It is now cited.
7-1038	7	34	17	34	24	Estimates of the change in global SOA burden are more uncertaint than listed here: span from -6% to almost double (100%) -references are appropriate in the text. [MARIA KANAKIDOU, GREECE]	Changed as suggested.
7-1039	7	34	21	34	21	There are experimental evidences that under a higher temperature scenario, not all plants would behave in the same manner. Higher temperatures and/or water vapor deficit could lead to vegetation emissions be inhibited	Rejected. The study of Plaza et al. (2005) was for monoterpene emissions in Mediterranean, which did

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						as a response of plants to stress conditions. This effect is related to the diurnal physiological cycle of vegetation which closes its stomata as response to water and temperature stress. In the Mediterranean, this behaviour has been observed and experimentally documented (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 possible effect could be included in the text as follows: "Some BVOC emissions can however be inhibited if temperature and water vapor deficit reach stress conditions (Plaza et al., 2005)". [BEGONA ARTINANO, SPAIN]	not give any quantitative estimate how global monoterpene emissions change as temperature and water vapor deficit reach stress conditions. However we now cite a review paper on the subject by Penuelas and Staudt.
7-1040	7	34	21	34	22	This effect suggested by Arneth et al (2007), has been evaluated in GCM (doi:10.5194/acp-12-10077-2012), giving surprisingly small changes in global CCN concentrations with estimated 2100 BVOC emissions. This suggested that even though the total emissions were very much reduced, the spatial and temporal patterns of the emissions seem to balance this effect somewhat. [Ari Asmi, Finland]	Partly taken into account. The paper by Makkonen et al. (2012) is now cited but conclusions remain the same.
7-1041	7	34	28	34	39	This secion perhapsis could be better discussed based on the two terms that determine aerosol life cycle, i.e., source and sink. Much of the problem in handling the feedbac discussed here in global models raises from either precipitation prediction (the dominant sink of aerosols) and emissions estimations (sources). [Chien Wang, United States of America]	Rejected. The role of precipitation as a driver for aerosol sinks is discussed briefly in Section 7.3.5 and is mentioned again in the synthesis on line 33. There is little analysis in the literature to discuss extensively the relative roles of sources versus sinks in aerosol-climate feedbacks.
7-1042	7	34	28			the chapter "synthesis" is not really helping, as it gives positive and negative feedback. However, the important issues were not adressed: is humidity increasing? This would increase the size of the particles, and thus radiation feedback. Is the number concentration increasing or decreasing? This is also not adressed. [Andrea Flossmann, France]	Rejected. The feedback factors summarized in this synthesis represent the overall effect of aerosol-climate interactions (including the effect of future changes in temperature, precipitation, humidity, etc., on aerosol mass and number concentrations). As far as aerosols is concerned, it is mostly relative rather than absolute humidity that matters, and relative humidity does not change much with climate change at first order.
7-1043	7	34	28			good synthesis paragraph [Daniel Murphy, United States of America]	Noted. Thanks.
7-1044	7	34	30	34	33	Tai et al. (2012b) examined 21st century trends in the meteorological modes driving aerosol variability over the US, using the CMIP3 archive of data from fifteen AR4 climate models. They used observed sensitivities of aerosol to meteorology to calculate the 2000-2050 changes in surface aerosol concentrations as projected by these models. They found that circulation-driven changes in the US are likely to be small, on the order of 0.3 μg m−3 at most. Tai, A.P.K., L.J. Mickley, and D.J. Jacob. 2012. Impact of 2000-2050 climate change on fine particulate matter (PM2.5) air quality inferred from a multi-model analysis of meteorological modes. Atmos. Chem. Phys. Discuss., 12, 18107-18131, 2012b. [Loretta Mickley, United States of America]	Rejected. No aerosol-climate feedback factor can be calculated from the Tai et al. (2012b) study. The estimate of feedback factor requires coupled aerosol-climate simulations.
7-1045	7	34	30	34	39	Section 7.3.5.4 What are the implications of this uncertainty for mitigation simulations? Is this discussed anywhere else in IPCC WG1? [European Union]	Rejected. This is not the place for discussing the implications of this uncertainty for climate mitigation. The uncertainty in biogeochemical feedbacks would increases (slightly) the uncertainty in permissible CO2 emissions for a temperature target.
7-1046	7	34	30	34	39	Very nice summary! [Gunnar Myhre, Norway]	Noted. Thanks.
7-1047	7	34	37	34	37	“smaller feedback” => smaller negative feedback [Paul Ginoux, United States of America]	Accepted. Changed as suggested.
7-1048	7	34	37	34	37	The smaller feedback is in fact a negative feedback, and this should be indicated explicitly. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Changed as suggested.
7-1049	7	34	37			The text notes the feedback reported by Bellouin is smaller than that reported by Liao. It is also worth noting that the feedback is also of opposite sign. [Government of United States of America]	Accepted. We have changed "smaller feedback" to "smaller negative feedback".

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7-1050	7	34	41			Section 7.4: There is very little discussion specifically on aerosol impacts on deep convective clouds (including associated anvil cirrus) in this section, even though the authors emphasize a regime-based context for analyzing aerosol effects (e.g., p. 7-40, lines 48-52), and specifically discuss these effects for different regimes such as stratocumulus, shallow cumulus, and cold clouds including Arctic stratus. While a separate sub-section for effects on deep convection may not be warranted, especially given length restrictions for the text, a bit more emphasis may be helpful to balance the presentation given the prevalence of deep convection and importance of anvil cirrus on LW radiation. Several recent CRM studies have looked at such aerosol effects, including from the standpoint of radiative-convective equilibrium (e.g., Grabowski 2006; van den Heever et al. 2011) and in regional-scale models (e.g., Morrison and Grabowski 2011; Fan et al. 2012); global impacts on deep convective clouds using GCMs has been investigated by Lohmann (2008). Aerosol effects in these clouds remain uncertain because of uncertainties related to ice microphysical processes in cirrus anvils, especially poor understanding of ice initiation mechanisms, and uncertainty related to interactions between convective updrafts, detrainment of condensate, and anvil cirrus properties, but this may be a point worth mentioning in terms of contributing to overall uncertainty in global aerosol effects. [Hugh Morrison, United States]	Accepted. Section 7.4 focuses on aerosol-cloud interactions in non- or weakly precipitating clouds. We have now added a few brief subsections and paragraphs discussing correlations between aerosol and ice particle size, as well as potential effects of "invigoration" on anvils. Discussion of aerosol effects on deep convective clouds are deferred to section 7.6 (as stated on 7-35, lines 5-8).
7-1051	7	34	41			Section 7.4, general comments: I am reviewing this section as a climate modeller, whose own research has (of necessity) moved away from these detailed process-related aspects in recent years. I found it to be a valuable review, which helped me to catch up on some of what I've missed since AR4. It is very well written, and I would like to congratulate the authors on their efforts. As such, I have not attempted to critically evaluate the process-related science, though I have a few minor comments below. [Leon Rotstayn, Australia]	Noted
7-1052	7	34	45			The text states that this section assesses our understanding of "aerosol-cloud-precipitation" interactions. But the title of the section only references Aerosol-Cloud interaction as do the subsequent sub-section titles and content. This section does not address precipitation with the minor exception of a brief reference on page 36, line 23 near the beginning of section 7.4.1.3. Therefore, the authors might consider revising the title of the section. [Government of United States of America]	Accepted. Minor word changes made
7-1053	7	34	47	34	47	To me, "cloudiness" implies cloud fraction. But (a) this section is about all possible effects of aerosols on clouds (and vice-versa), not just their effect on cloudiness, and (b) the effect on cloudiness is much more uncertain (if it exists at all) than the effect on cloud particle size and thus albedo. So I would change "cloudiness" to "cloud properties." [Anthony Del Genio, United States of America]	Accepted. "Cloudiness" is meant in a broader sense of "presence of cloud" but we change to "cloud properties" to avoid ambiguity.
7-1054	7	34	47	34	49	It's not clear how or whether this sentence relates to the previous sentence. Anthropogenic aerosols may contribute a strong forcing on the climate system even if they don't change cloudiness, namely via changes in cloud properties. I suggest skipping this sentence. [Jón Egill Kristjánsson, Norway]	Accepted. We have changed "cloudiness" to "cloud properties" to make this clear.
7-1055	7	34	52	34	52	Tao et al (2012; Review of Geophysics; in the current reference list) should be cited here. [Chien Wang, United States of America]	Rejected. Tao et al, a review paper, is cited elsewhere where it is more appropriate.
7-1056	7	34	55	34	55	"clouds" is a bit ambiguous here. Cloud properties, abundance, occurance? [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted. Change "clouds" to "cloud properties"
7-1057	7	34		44		Section 7.4 : I enjoyed very much reading this section. It synthesizes very well the recent areas of progress in modelling, observing and understanding of cloud-aerosol interactions without introducing too much jargon. [Sandrine BONY, France]	Noted
7-1058	7	35	5			This is a very minor point, but the microphysics of aerosol-cloud interactions is also discussed for (pure) ice clouds (e.g., cirrus) in addition to liquid and mixed-phase clouds as stated in this section. [Hugh Morrison, United States]	Accepted - text revised
7-1059	7	35	7	35	7	should ice clouds be included in this list? [Peter Irvine, Germany]	Accepted - text revised
7-1060	7	35	12	35	30	The aerosol-cloud interactions and associated cloud macro- and micro-physical parameters, especially between below-cloud/cloud edges (including broken clouds and cloud overlaps), in-cloud environments need to be better understood through measurements, models and theory. The forcing due to thermodynamical effects (latent heat release) in the RFaci and AFaci need to be quantified. Added, models delineating the interactions between clouds and large-scale environment need further development. Accurate determination	Rejected. These are all themes covered in this chapter.

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						of albedo of the underlying surface and location of aerosols (aborbing aerosol in particular) at the cloud periphery are essential for better estimations of radiative forcing. [Panuganti, C.S. Devara, India]	
7-1061	7	35	18			It seems to me that RFaci is part of AF: the aerosols lead to CCN that lead to the Twomey effect: there is an adjustment of CCN due to aerosols. How do you separate effects due to more CCN on albedo from further effects? (More and smaller drops sediment differently, uptake meager differently, etc.) [Andrew Gettelman, United States of America]	Accepted. The revised text now recognises this by explaining that all interactions are adjustments, and by deemphasising Rfaci
7-1062	7	35	25			This statement is okay (AFaci subsumed RFaci), but doesn't seem to make sense in the overall scheme of RF and AF. I suggest 'Twomey' effects be given another name as a subset of AFaci: maybe it is albedo interactions: AFaai or AFaae (aerosol albedo interactions or effects) [Andrew Gettelman, United States of America]	Accepted. The revised text now recognises this by de-emphasising RF.
7-1063	7	35	25			I find the description of AFaci to be rather confusing (see also general comments on Chapter 7 above). It is stated that AFaci does not include adjustments associated with aerosol-radiation interactions, which is reasonable as a way to address the problem if “aerosol-radiation interactions” is strictly defined as the impact of aerosols directly on radiation. But what about adjustments due to changes in radiation resulting from aerosol-induced changes in cloud macro- or microstructure? It should be made clear that AFaci also includes such adjustment (as I think the authors intend). The inclusion of such adjustment as part of AFaci is especially confusing because of later statements on aerosol impacts on precipitation on p. 7-52, which seem to suggest that this adjustment is actually not included (see comment #23 below). [Hugh Morrison, United States]	Accepted. Text clarified. Radiative responses are part of Afaci. Afari is strictly associated with interactions between radiation and the aerosol.
7-1064	7	35	32	35	39	figure 7.16: In the figure itself and in the caption: it should read drop nucleation and ice nucleation (compare point 7 above) [Andrea Flossmann, France]	Accepted. The most correct terms are aerosol activation and ice nucleation.
7-1065	7	35	32			Figure 7.16: Caption needs information on different colours used in the graphic (blue, red etc.) [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.The caption has been updated.Color code is now explained in the caption.
7-1066	7	35	33	35	33	Insert "of" between "myriad" and "aerosol-cloud-precipitation" [Jón Egill Kristjánsson, Norway]	Rejected. myriad is an adjective or a noun.
7-1067	7	35	33			A GCM grid box is large enough to allow for internal feedback processes in which e.g., an aerosol modified by passage though cloud n will affect cloud n+1 (Heintzenberg, J. 2012. The Aerosol-Cloud-Climate Conundrum. International Journal of Global Warming 4, 219-241). [Jost Heintzenberg, Germany]	Rejected. This topic is addressed on [old text] pg 36, lines 1-6.
7-1068	7	35	37	35	37	Isn't heterogeneous nucleation just as much an issue for cirrus as homogeneous nucleation? Specifically, isn't our remaining uncertainty in which aerosol types effectively nucleate cirrus, what processes control the vertical distribution of these particles, and the fact that where both homogeneous and heterogeneous nucleation can occur, heterogeneous wins out an important thing? [Anthony Del Genio, United States of America]	Accepted. Changed figure caption to include heterogeneous and homogeneous. Recent work suggests that heterogeneous may be more important in cirrus than homogeneous.
7-1069	7	35	41	36	18	I have serious concerns on the section “7.4.1.2 Advances and Challenges in Observing Aerosol-Cloud Interactions” First of all, the majority of the papers cited in this section are not concerned directly with aerosol-cloud interactions. Rather they deal with some “boundary” problems such as contamination of aerosol retrievals. They are relevant, but not the core. Among the tens of citations, only a handful (no more than 5) are really concerned with the main theme of aerosol-cloud interactions, and even less (merely 2-3) are since AR4. I’d recommend the responsible lead author to refer to the recent review article (Tao et al. 2012) which provides a much more extensive, updated and critical review (Ch 3 and 4 are devoted to observation-based studies). I know that IPCC is not charged to do an extensive literature review. On the other hand, however, if IPCC fails to refer to a reasonable number of studies since AR4, one would think we didn’t make any significant progress since AR4, which is not the case at all. In fact, it has been a major hot topic with ample studies conducted and published. [Zhanqing Li, United States of America]	Accepted. The text has been restructured and revised to address this criticism.
7-1070	7	35	41	37	20	A difficulty in observation based studies is how to identify the aerosol plume that actually entered clouds. Many remote sensing studies suffered also from cloud comtamination as well.These all make the connection between observed aerosol properties with targeted cloud changes difficult. On the other hand, majority of models including cloud-resolving and large-eddy simulation models still adopt the assumpotion that aerosol depletion does not matter to the cloud particle nucleation, thererfore, run an open system for the aerosols, i.e.,	Rejected. Cloud containimation is discussed in 7.4.1.2. Many results summarised in ths section rely on models that do keep track of the aerosol after activation. This is too detailed to include in the text.

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						however many aerosols the activation need the model will have them onset. This assumption is apparently used in many of the works freqently cited in this chapter particularly those dealing with long-term response of cloud systems. These obvious shortcomings in both obsrvations and modeling should be indicated clearly somewhere in this section. [Chien Wang, United States of America]	
7-1071	7	35	43	35	43	Probably mean "satellite retrievals" [Daniel Murphy, United States of America]	Accepted.
7-1072	7	35	43	35	51	This is the first paragraph in a section on cloud-aerosol interactions and describes the difficultites in observing such interactions. In fact it describes challenges in observing aerosols in the presence of clouds. There have been significant advances in understanding the limits of remote sensing the relevant cloud properties as well (a short list might include dois 10.1029/2007JD009095, 10.1029/2010GL044094, 10.1029/2012JD017655, 10.1175/JCLI-D-11-00267.1). [Robert Pincus, United States of America]	Accepted. Text restructured so that we don't jump right into challenges/problems. Added reference to one of diGirolamo's papers.
7-1073	7	35	43	36	18	Section 7.4.1.2: It should be mentioned here that one of biggest challenges in observing aerosol-cloud interactions is that it is extremely difficult to observe aerosols when the aerosols are near or inside clouds. In cases involving absorbing aerosols, some recent studies infer the presence of aerosols inside the clouds using other observable gaseous pollutants as a “proxy” for aerosol. One such aerosol proxy is carbon monoxide (CO). Incomplete combustion produces both CO and absorbing aerosols (e.g. black carbon) that occurs, for example, in forest fires, coal burning power plants, and fossil fuel powered automobiles. CO in the upper troposphere (pressure < 300hPa) can be measured by the Microwave Limb Sounder (MLS) inside ice clouds, because the typical cloud particle sizes are much smaller than the microwave wavelength. A number of recent studies (e.g. Jiang et al. 2008;2009) have used MLS observed CO as a proxy of aerosols to classify ice clouds as polluted or clean. Another challenge in observing the aerosol-cloud interaction is that cloud properties, such as the mean cloud particle size, could also be influenced by large scale dynamics, such as convection (e.g. Jiang et al. 2011). References: Jiang, J.H., H. Su, M. Schoeberl, S.T. Massie, P. Colarco, S. Platnick, and N. Livesey, "Clean and polluted clouds: relationships among pollution, ice cloud and precipitation in South America," Geophys. Res. Lett. 35, L14804, doi:10.1029/2008GL034631, 2008. Jiang, J.H., H. Su, S.T. Massie, P.R. Colarco, M.R. Schoeberl, and S. Platnick, "Aerosol-CO relationship and aerosol effect on Ice cloud particle size: Analyses from Aura Microwave Limb Sounder and Aqua Moderate Resolution Imaging Spectroradiometer observations," J. Geophys. Res. 114, D20207, doi:10.1029/2009JD012421, 2009. Jiang, J.H., H. Su, C. Zhai, S.T. Massie, M.R. Schoeberl, P.R. Colarco, S. Platnick, Y. Gu, and K.N. Liou, "Influence of convection and aerosol pollution on ice cloud particle effective radius," Atmos. Chem. Phys. 11, 457-463, doi:10.5194/acp-11-457-2011, 2011. [Government of United States of America]	Accepted. The difficulty of measuring aerosol in the vicinity of cloud is mentioned in 7.4.1.2. The use of CO as a proxy is now added.
7-1074	7	35	49	36	49	Insert "Chand et al., 2012" after "Twohy et al., 2009". Chand, D., R. Wood, S. J. Ghan, M. Wang, M. Ovchinnikov, P. J. Rasch, S. Miller, B. Schichtel, and T. Moore, 2012. Aerosol optical depth increase in partly cloudy conditions, J. Geophys. Res., 117, D17207, doi:10.1029/2012JD017894. [Steven Ghan, United States of America]	Accepted.
7-1075	7	35	53	35	55	In order to overcome certain technical problems, combination of ground-based spectrophotometers and lidars in networkmode could be a better tool for such studies. Information of this kind over land would help not only for reliable estimation of radiative forcing but also to quantify the compensating effects. [Panuganti, C.S. Devara, India]	Accepted. Added text on the surface remote sensing view of the world.
7-1076	7	35	55	35	55	After "Stephens et al., 2002)" add "and the general problems of aerosol-cloud-precipitation interactions (e.g. Niu and Li, 2012)" [Zhanqing Li, United States of America]	Rejected. Paper is too specific; Added Winker et al. (2010) instead.

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7-1077	7	35	55			The authors mgiht want to consider a better reference illustrating the capabilities of new active space-based remote sensing to observe aerosol layers above and below clouds, such as: Winker, D. M., J. Pelon, J. A. Coakley, Jr., S. A. Ackerman, R. J. Charlson, P. R. Colarco, P. Flamant, Q. Fu, R. Hoff, C. Kittaka, T. L. Kubar, H. LeTreut, M. P. McCormick, G. Megie, L. Poole, K. Powell, C. Trepte, M. A. Vaughan, B. A. Wielicki, 2010: “The CALIPSO Mission: A Global 3D View Of Aerosols And Clouds”, B. Am. Meteorol. Soc., 91, 1211–1229, doi:10.1175/2010BAMS3009.1 [Government of United States of America]	Accepted.
7-1078	7	35	55			In addition to the references provided, the authors might consider adding reference to Chand, D., T. L. Anderson, R. Wood, R. J. Charlson, Y. Hu, Z. Liu, and M. Vaughan (2008), Quantifying above-cloud aerosol using spaceborne lidar for improved understanding of cloudy-sky direct climate forcing, J. Geophys. Res., 113, D13206,doi:10.1029/2007JD009433. [Government of United States of America]	Accepted.
7-1079	7	35	57			The authors might consider reference an important paper by Hasekamp 2010 by replacing: "(Deuzé et al., 2001; Mishchenko et al., 2007)" with "(Deuzé et al., 2001; Hasekamp, 2010; Mishchenko et al., 2007)" [Government of United States of America]	Accepted
7-1080	7	35	58	35	58	after "Wood, 2011b)", add ", especially long-term experiment like the ARM (Li et al. 2011b)". [Zhanqing Li, United States of America]	Accepted.
7-1081	7	35				Section 7.4.1.1: I have trouble in this chapter with the invention of a new classification, which does not seem to make sense. I continued to struggle with this throughout the chapter. I understand this is probably related to consistency with other chapters, but I do not understand how 'Twomey' indirect effects get called RF? I would suggest that for clarity, more references to traditional terminology in the literature be used throughout the chapter. Perhaps I am simply 'old fashioned' in liking the existing definitions, but I don't think the new definitions make sense. It might be because they have only one small letter different in some cases (AFaci, AFari) or refer to different summations and residuals of each other in different places. But as a reader I am very confused. [Andrew Gettelman, United States of America]	Rejected. We're attempting to make a break from the old world view. Figure 7.2 is our effort to convey the shift. Effort has been made to clarify the nomenclature throughout.
7-1082	7	36	4	36	5	Coalescence does not only affect the size distribution but also the mixing state. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted.
7-1083	7	36	4	36	6	really old references here. There exist newer ones. [Andrea Flossmann, France]	Accepted. Added reference to Ervens et al. 2011
7-1084	7	36	6	36	8	Regarding the statement: "As a result, our understanding of the distribution and properties of the aerosol in the vicinity of clouds continues to improve apace with an appreciation of the limits of this understanding (Anderson et al., 2009),":the authors might consider adding a sentence about what 'the limits' are? [Government of United States of America]	Accepted. Reworded phrase.
7-1085	7	36	8	36	24	It would be good to add HNO3 effect on cloud droplet activation (e.g. Makkonen et al., 2012 ACP) [European Union]	Rejected. Not changed in the interests of brevity.
7-1086	7	36	9	36	18	Grandey and Stier, ACP, (2010) have shown that some previous satellite-based studies used too large regions for calculating aerosol-cloud correlations, potentially introducing artifacts. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reference added later when referring (even if obliquely) to scale
7-1087	7	36	10	36	18	These sweeping negative comments about observation-based investigations of aerosol indirect effects are unwarranted. It makes me feel “the baby being thrown out with the bath water”. Solving any natural scientific problem usually requires theory, observation and analysis. Each method has its own merits and limitations. While a simple correlation alone cannot reveal causality, meticulously designed and well-constrained correlation analyses have been proven to be a valid and effective approach to gain insights into a complex system. It is often a gateway leading to major discoveries. It does suffer from certain limitations and so does a modeling approach. The most obvious one is that a model is based on "what we have known already" and thus modeling results may, or may not be true in nature. Given that any significant correlation is a manifestation or a “smoking gun” of the working of some physical processes. Unless such processes are fully understood so that an observed correlation can be modeled, any un-producible relationships warrant full attention. As far as AIE is concerned, there are many such correlations that should not be undermined, as they	Accepted. Text revised to be more positive about accomplishments. Exact text suggested not adopted because of space restrictions.

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						may, if aren't yet, lead to major advancements. I'd revise the paragraph as follows: Various observation-based approaches were attempted by to study aerosol-cloud interactions. Due to the dominant influences of meteorology, a key is to single out (amplify) the "signals" of the interactions by suppressing "noises". These approaches include selection of dramatic aerosol events to enhance the singles relative to the background environment, such as ship tracks (Ferek et al., 2000; Coakley et al., 2000), fire smoke (Kaufman and Fraser, 1997; Rosenfeld, 1999; Koren et al., 2004; Andreae et al., 2004], heavy air-pollution [Rosenfeld, 2000], and dust storms (Rosenfeld et al., 2001). Another approach is to track complete aerosol episodes/cycles during which aerosol loading goes up and down accompanied by changes in cloud microphysics (e.g., effective radius) (Schwartz et al., 2002; Feingold et al., 2003). A third approach is to use a large ensemble of cases to try to suppress or filter out the influences of fast atmospheric dynamics in order to single out the effects of aerosols which are essentially partial derivatives of cloud/precipitation with respect to changes in aerosol properties under the same meteorological setting (in an ensemble sense). This is illustrated using long-term ground data (Qian et al. 2009, Li et al., 2011b) and global satellite data (e.g. Nakajima et al. 2001; Koren et al., 2008; Yuan et al. 2008, Lebsock et al., 2008, Niu and Li, 2012). The observational challenge of inferring causality from correlation remains a large and limiting one. Because the aerosol is a strong function of air-mass history and origin, and is strongly influenced by cloud and precipitation processes (Anderson et al., 2009; Clarke et al., 1999; Petters et al., 2006), and both are affected by meteorology (Engström and Ekman, 2010), simple correlations between the aerosol and cloud, or precipitation, cannot be taken as generally indicating a cloud response to the aerosol (e.g., Painemal and Zuidema, 2010). If controlling factors governing any relationships between aerosols, clouds and meteorological variables can be identified and constrained, conditional correlation analyses may help single out the effects of aerosols. This is a non-trivial task, due to inadequate understanding of cloud controlling factors (air-mass history or cloud dynamical processes) in the first place (Anderson et al., 2009; Siebesma et al., 2009; Stevens and Brenguier, 2009). Without properly constraining the influential factors, these problems may undermine confidence in observation-based inferences of aerosol effects on clouds and precipitation Observation-based findings can, however, provide a lead for modelers to pursue a process-oriented (modeling) study to attempt to reproduce an observed relationship or a phenominon during which the relationship may be confirmed or negated as a genuine indicator/measure of aerosol and cloud interactions (Li et al. 2011b, Fan et al. 2012). [Zhanqing Li, United States of America]	
7-1088	7	36	13	36	13	Consider adding the following reference: Grandey, B. S., P. Stier, T. M. Wagner, R. G. Grainger, and K. I. Hodges (2011), The effect of extratropical cyclones on satellite!retrieved aerosol properties over ocean, Geophys. Res. Lett., 38, L13805, doi:10.1029/2011GL047703. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Rejected. Interesting paper but a little off topic here.
7-1089	7	36	16			I cannot agree with the sentence "lack of understanding of cloud controlling factors". The conditions, controlling cloud formation and evolution, incl. precip are relatively well known. Maybe it is not possible to simulate them in all models, but they are known. [Andrea Flossmann, France]	Accepted. We are referring here to the largescale environment. Changed to "largescale cloud controlling factors"
7-1090	7	36	17	36	18	"Greatly undermine confidence" sounds like too much of an opinion. I agree there are problems but think there are still valid inferences when people are really, really careful. The intro statement on line 10 is excellent and doesn't read like opinion. [Daniel Murphy, United States of America]	Accepted. Change as suggested.
7-1091	7	36	20			Section 7.4.1.3: I am in complete agreement that the inability of GCMs to resolve cloud dynamics and especially vertical velocity over the key scales of motion remains a huge challenge. However, one promising approach to couple sub-grid dynamics and microphysics in a more consistent, physically-based way is the so-called assumed PDF method (e.g., Golaz et al. 2002; Larson and Giffin 2012). While considerable uncertainty remains in specific features such as assumed PDF shape of vertical velocity and various source/sink terms for predicted PDF moments, this provides the potential for an improved framework for driving microphysics compared to the ad-hoc methods used currently. See also comment #3 above. [Hugh Morrison, United States]	Accepted. Added reference to PDF approach Golaz alongside MMF.
7-1092	7	36	22	36	32	again the authors imply that only work has been done concerning marine stratocumumuls (compare my points 16 and 21) [Andrea Flossmann, France]	Rejected. vanZanten 2011 is a trade cumulus study. Not changed.

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7-1093	7	36	23			When citing these studies, the authors should consider adding an "e.g.", since these aren't the only two studies where aerosol-cloud-precipitation interactions have been modeled. [Government of United States of America]	Accepted. Change as suggested.
7-1094	7	36	26	36	26	Not at all clear what is meant by "that gave rise to the original idea". What idea? [Jón Egill Kristjánsson, Norway]	Accepted. The original albedo and lifetime effects.
7-1095	7	36	26	36	26	(section 7.4.2.1) "The cloud albedo effect (Twomey, 1977),…" You may consider noting in the Report the following sad news (from Eric A. Betterton, Professor, University of Arizona): "Sadly, Sean Twomey passed away in Tucson on Saturday, October 27, 2012, after a short battle with leukemia. Sean retired from the Department of Atmospheric Sciences in 1990. He was 85 years old. Sean was a giant in the fields of mathematical inversion techniques, and atmospheric aerosols, literally having written the book in both areas. But he will perhaps best be remembered for the Twomey effect (the indirect effect of aerosols on cloud albedo) which he first described in a seminal paper published nearly 40 years ago: Pollution and Planetary Albedo, Atmospheric Environment, 8, 1251 (1974), and which is now a component of most major climate models. Sean's work was recognized with the Rossby Medal (1980), the highest honor bestowed by the American Meteorological Society, and with the Haagen-Smit Prize (2004). We plan to notify the broader scientific community by means of an obituary to be published in Eos so please send me any Twomey recollections you may have. [Erik Swietlicki, Sweden]	Editorial. Thank you. But this is not the place for stating this. No text changes.
7-1096	7	36	26			which original idea? [Andrea Flossmann, France]	Accepted. (see also 7-1094)
7-1097	7	36	30	36	32	Regarding the statement: "As a result it is likely that the physical system is less sensitive to aerosol perturbations than are large-scale models, which do not represent all of these compensating processes.", it can be concluded that large-scale models do not represent all of these compensating processes might not be so bad. Is this the message the reader is to get? [Government of United States of America]	Rejected. No because GCMs tend to represent a few effects such as albedo and lifetime effects as strong effects. No change.
7-1098	7	36	32	36	34	(section 7.4.2.1) "In the Arctic, anthropogenic aerosols may influence the longwave emissivity of thin liquid clouds and generate a positive forcing at the surface (Garrett and Zhao, 2006; Lubin and Vogelmann, 2006)." You may consider also this reference on this topic: T. Mauritsen, et al., An Arctic CCN-limited cloud-aerosol regime Atmos. Chem. Phys., 11, 165-173, 2011 [Erik Swietlicki, Sweden]	Accepted. Add reference
7-1099	7	36	34	36	34	I am not sure if the "at larger scales" or "regional scale" is sufficiently defined. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Rejected. Specificity not required in this context.
7-1100	7	36	34	36	42	. As stated in general comments on Chapter 7 above, I appreciate the emphasis on the importance of aerosol effects in different cloud and meteorological regimes. However, there are some instances where the distinction of regimes could be made clearer. One example is the discussion in the paragraph on lines 34-42. This describes studies using larger-scale regional models to look at cloud-aerosol interactions, and seems to present a counterpoint to the preceding paragraph on lines 22-32 discussing more idealized studies using smaller-scale models (LES). However, this seems to be a bit of an apples-to-oranges comparison, because the particular studies cited in the paragraph on lines 22-32 simulated stratocumulus and shallow cumulus, while the studies in the following paragraph on lines 34-42 with the exception of Bangert et al. (2011) simulated deep convection (I do note that Seifert et al. 2012 didn't focus on deep convection per se, but simulated summertime conditions over Germany for which deep convection is likely to be key). This is important since aerosol impacts may be expected to differ significantly between these regimes. There are other examples of recent studies using regional-scale models to assess aerosol effects on stratocumulus/shallow cumulus, such as Yang et al. (2012) who used WRF-CHEM with horizontal grid spacing of 9 km to investigate cloud-aerosol interactions during VOCALS. A related point is that the ability to resolve "fine scale cloud processes" (p. 7-36, line 38) depends in part on the regime being studied, and how "fine scale" is defined. Relevant scales of motion driving droplet activation and microphysics will vary in different regimes, which could be mentioned. Many (but not all) of the studies cited in the paragraph on lines 34-42 used horizontal grid spacing of order 1 km, allowing deep convection to be at least marginally resolved (even though smaller-scale turbulence is unresolved). This presents a much different picture than regional-scale models with a grid spacing of order 1-10 km simulating stratocumulus or shallow convection, in which the key	Accepted. References expanded. However, this section lays out different tools (models that address different scales) rather than different cloud types. Thus this is not an apple to oranges comparison because we're not comparing cloud types but different modeling approaches.

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						scales of motion driving the clouds cannot be resolved. [Hugh Morrison, United States]	
7-1101	7	36	48			replace "activation" by "nucleation" [Andrea Flossmann, France]	Accepted.
7-1102	7	36	49	36	49	Improper sentence. Either replace the comma before "however" with a semicolon, or replace it with a period and capitalize "However". [Steven Ghan, United States of America]	Accepted. Changed to semicolon
7-1103	7	36	50	36	52	As far as I'm aware, there are no GCM parameterizations that directly relate aerosol influences to cloud amount (or liquid water amount), unlike what is implied in this sentence: "…parameterizations of aerosol influences on cloud amount cannot account for known non-monotonic responses…". GCM representations are almost certainly overly simplistic and of course cannot resolve cloud dynamics or coupling of cloud dynamics and microphysics, but I don't think it's correct to imply that models have parameterizations that directly relate cloud amount to aerosols. This comment is similar to comments #15 and #17 above. [Hugh Morrison, United States]	Accepted. In response to this and other comments we have changed the text to clarify. Although GCMs don't explicitly connect increases in aerosol with increases in cloud lifetime and amount, this is the net result of the GCM representation of lifetime effects. See e.g., Quaas et al. 2009, ACP.
7-1104	7	36	51	36	51	In this context, "non-monotonic" seems a bit cryptic, even with the cross-reference to Section 7.4.3.2. Can a phrase or sentence be added to explain what this means? Perhaps all it needs is something like "non-monotonic responses, which in some regimes can cause cloud amounts to decrease as aerosol concentrations increase". [Leon Rotstayn, Australia]	Accepted.
7-1105	7	36	56	37	2	I would put it more positively:" the challenge for GCMs is to represent the nonlinear influence of unresolved cloud-scale updraught velocities and associated cooling rates on aerosol-cloud interactions, even for models with grid sizes on the order of kilometres. Schemes based on probability density functions for cloud and updraught velocity (e..g., Guo et al., 2010) show some success". Guo, H., J.-C. Golaz, L. J. Donner, V. E. Larson, D. P. Schanen, and B. M. Griffin, 2010: A dynamic probability density function treatment of cloud mass and number concentrations for low level clouds in GFDL SCM/GCM. Geosci. Model Dev., 3, 475–486. [Steven Ghan, United States of America]	Accepted. Text rewritten. Guo paper referred to.
7-1106	7	37	4			replace "formidable" by "substantial" e.g. [Andrea Flossmann, France]	Accepted. Changed to daunting
7-1107	7	37	13	37	20	I would suggest to add a recently published highly-relevant paper on this subject: Wang et al. (2012, GRL). This paper combined the modeling and observational approaches to constrain cloud lifetime effects. The global cliamte models are used to link cloud lifetime effects with precipitation frequency susceptiblity to aerosol perturbations. Wang, M., S. Ghan, X. Liu, T. L'Ecuyer, K. Zhang, H. Morrison, M. Ovchinnikov, R. Easter, R. Marchand, D. Chand, Y. Qian, and J. Penner (2012), Constraining cloud lifetime effects of aerosols using A-Train Satellite observations, Geophys Res Lett, 39. [Minghuai Wang, United States of America]	Accepted. Add reference and few words.
7-1108	7	37	16	37	17	"These include inversions of the observed historical record using large-scale modelling studies". One or more references of this type of study should be provided. [Lazaros Oreopoulos, United States of America]	Accepted. Reference added.
7-1109	7	37	18	37	18	Consider adding Quaas et al., ACP (2010). [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Rejected. If this is the Quaas, Stevens, Stier, Lohmann paper then it has a different flavour. Therefore the reference is not added.
7-1110	7	37	19			The authors should consider adding an 'e.g' in front of Ackerman et al., 2009 [Government of United States of America]	Accepted.
7-1111	7	37	20	37	20	After Quass et al., 2009 add reference to Wang et al. (2012): Wang, M., Ghan, S., Liu, X., L'Ecuyer, T., Zhang, K., Morrison, H., Ovchinnikov, M., Easter, R., Marchand, R., Chand, D., Qian, Y., and Penner, J.E., 2012. Strong constraints on cloud lifetime effects of aerosol using satellite observations. Geophys. Res. Lett., 39, 15, doi:10.1029/2012GL052204. [Steven Ghan, United States of America]	Accepted.
7-1112	7	37	22			Section 7.4.2 and Section 7.4.1.1. There is obviously a need to classify forcings in order to discuss them, but I am uncomfortable with the classification of the cloud-albedo effect (formerly the first indirect effect) as RF. This does not seem right to me because it is not instantaneous. The change to the droplet number and size distributions may be rapid when aerosol is added, but it is an adjustment. It is more obvious, I think, to distinguish between (instananeous) RF and everything which involves some adjustment. By this argument, all	Accepted. Changes will be made to reflect the fact that all processes are essentially adjustments (albeit with different timescales).

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						aerosol-cloud interaction is a forcing adjustment and counted in AF but not RF. This also fits better with the former classification, in that the direct effect effect is RF and all the indirect effects are AF. The more general idea of adjustment thus puts aerosol forcing into the same scheme as forcings due to other agents. I wonder if you and ch8 could reconsider this classification, or if not, give a clear rationale for it. I have made a similar comment on section 8.3.4. [Jonathan Gregory, United Kingdom]	
7-1113	7	37	26	37	29	Perhaps should also indicate another factor that the extinction coefficient is nearly a constant with particle size within the typical cloud droplet size range. [Chien Wang, United States of America]	Rejected. This is detail that we cannot include.
7-1114	7	37	27			even though "liquid cloud albedo" is understandable, still it is jargon; avoid [Andrea Flossmann, France]	Accepted. Text changed
7-1115	7	37	28	37	28	It will be unclear to the casual reader that an increase in number concentration is associated with a decrease in particle effective radius, and that this in turn explains the increase in total surface area. This should be stated explicitly. [Anthony Del Genio, United States of America]	Accepted.
7-1116	7	37	33	37	35	Alterskjær et al., JGR (2010) may be a paper to cite if you discuss this further [Gunnar Myhre, Norway]	Rejected since we don't discuss further.
7-1117	7	37	40	37	41	In my opinion, this sentence seems to understate uncertainty associated with entrainment. I would argue that considerable uncertainty remains in terms of homogeneity of mixing and the role of entrainment and evaporation on evolution of the droplet spectra. This was highlighted as a key uncertainty in model simulations in earlier work (Grabowski 2006), although more recent work using more sophisticated representations of cloud microphysics have suggested a much smaller impact of homogeneity of mixing on cloud optical properties and macrophysical structure (Hill et al. 2009; Slawinska et al. 2012). [Hugh Morrison, United States]	Rejected. The evidence is somewhat conflicting. This is a level of detail we cannot go into here.
7-1118	7	37	40			How do you define "cloud effective radius"? [Jost Heintzenberg, Germany]	Accepted. Changed to cloud drop effective radius
7-1119	7	37	41	37	43	the concept of "adabaticity" is old and has been proven wrong in numerous studies. I cannot understand why the work of the problems of spectra broadening in adiabatic air parcels is coming up periodically, even though it is of no significance for real clouds. Those are NOT adiabatic!!! [Andrea Flossmann, France]	Rejected. We clarify that we are not assuming that clouds are adiabatic but that we need to take into account the degree of subadiabaticity if we are to achieve drop concentration closure.
7-1120	7	37	43	37	45	The discussion of three-dimensional radiative transfer as it relates to cloud-aerosol interactions is unfounded. The Zuiduma et al., 2008 paper in fact reports that three-dimensional transport normally has a trivial impact on the aci. There is an enormous body of literature on how three-dimensional transport affects the radiation field in inhomogeneous clouds but interactions with aerosols are orthogonal to this. [Robert Pincus, United States of America]	Accepted. Text removed since it did not add clarity.
7-1121	7	37	44	37	45	This sentence is quite vague, e.g., "under certain conditions". Also, it's nothing new that the two-stream approximation has its limitations. Rephrase. [Jón Egill Kristjánsson, Norway]	Accepted. Text removed since it did not add clarity.
7-1122	7	37	49	38	4	Before discussing observational evidence for aerosol impacts on clouds you must define what you mean be "aerosol" in line 49. AOD is not the same as CCN, and even CCN at a particular supersaturation is not necessary representative of the number that are activated in updrafts. At a minimum you must state your assumptions here. You can cite Andrea's 2009 ACP paper if you want to link AOD to CCN, but that relationship doesn't necessarily hold at higher supersaturations. [Steven Ghan, United States of America]	Accepted. The term "aerosol" is used broadly here to describe suspended particulates. The aerosol is being quantified by AOD (since aerosol concentrations or CCN were not measured in these studies).
7-1123	7	37	49	38	4	Section 7.4.2.2: The authors should consider including a few studies providing recent observational evidence for aerosol influence on ice cloud: Using MLS observed CO as a proxy of aerosols to classify ice clouds as polluted or clean. Jiang et al. [2008] found polluted clouds are associated with smaller ice cloud particles (roughly consistent with the “Twomey” effect) and weaker precipitation than clean clouds over South America during the biomass fire season. Jiang et al. [2009] extended this analysis to various regions over the globe and quantified the relationships between CO and aerosol optical thickness. They showed that high CO is generally a good indicator for anthropogenic combustion and biomass fire aerosols. Su et al. (2011) also found that aerosols lifted by convection into the upper troposphere tend to influence the ice cloud and water vapor amount near the tropopause and thus could influence the amount of water vapor go into the lower stratosphere.	Accepted. Added mention of CO as an aerosol proxy.

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						Jiang, J.H., H. Su, M. Schoeberl, S.T. Massie, P. Colarco, S. Platnick, and N. Livesey, "Clean and polluted clouds: relationships among pollution, ice cloud and precipitation in South America," Geophys. Res. Lett. 35, L14804, doi:10.1029/2008GL034631, 2008. Jiang, J.H., H. Su, S.T. Massie, P.R. Colarco, M.R. Schoeberl, and S. Platnick, "Aerosol-CO relationship and aerosol effect on Ice cloud particle size: Analyses from Aura Microwave Limb Sounder and Aqua Moderate Resolution Imaging Spectroradiometer observations," J. Geophys. Res. 114, D20207, doi:10.1029/2009JD012421, 2009. Su, H., J.H. Jiang, X. Liu, J.E. Penner, W.G. Read, S.T. Massie, M.R. Schoeberl, P. Colarco, N.J. Livesey, and M.L. Santee, "Observed Increase of TTL Temperature and Water Vapor in Polluted Clouds over Asia," J. Climate 24, 11, 2728-2736, doi:10.1175/2010JCLI3749.1, 2011. [Government of United States of America]	
7-1124	7	37	49	38	4	Section 7.4.2.2: There are surface-based observations of aerosol-cloud interactions, (McComiskey et al JGR 2009, for example); the authors might consider discussing them briefly? [Government of United States of America]	Accepted. Include brief mention of ACI from the surface.
7-1125	7	37	49	38	24	It needs to be remarked that all the so-called observational studies do not proivde a measure of the albedo change and thus no measure of RF and AF. Moreoever most studies immediately jump to the conclusion that there is a signficant radiative effect by equating this to an observed particel size (or equivalently concentration) change. The recent paper of Chen et al (PNAS) 2012 is very relevant here as it takes the ship track data base of Christensen and Stephens and adds aircraft data from EPEACE and shows pretty convincingly that the albedo change fundamenally tracks the cloud liquid water change such that the latter is the principal determinant of albedo change. They also show that for 25% of the entire data base, the albedo is reduced in ship tracks and not increased and that only a small fraction of teh ship tracks sampled had anything like a Twomey effect (ie a non varying water budget - see comment 10). This is also a very relevant and sobering result for the geoengineering cloud brightening discussion. [Graeme Stephens, United States of America]	Accepted. Paper is an ACP paper with Seinfeld, Sorooshian et al. (not PNAS)
7-1126	7	37	50	37	50	"Resulting in" implies causality. Do the observations demonstrate causality or merely correlation? This is the sticking point in virtually all observational "evidence" for aerosol indirect effects on clouds. [Anthony Del Genio, United States of America]	Rejected. Causality is intended. Statement would have been problematic if referring to drop size response.
7-1127	7	37	54	37	56	At high aerosol number concentration, droplet number also tends to saturate (Verheggen et al., doi:10.1029/2007JD008714, 2007 "Aerosol partitioning between the interstitial and the condensed phase 3 in mixed-phase clouds" JGR 2007). Suggestion: "At high AOD and with high aerosol number concentration, droplet N conc tends to saturate (...)" [Bart Verheggen, Netherlands]	Accepted.
7-1128	7	37	55	37	56	I think this is a demonstration of the minor definitional difficulty that the authors have. I read "if the aerosol is absorbing there may be reductions in ... cloudiness" as being a semi-direct issue, and yet this is a section on aci. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. Clarified that this is an ari.
7-1129	7	37	56			"This effect"? Which effect? RFaci? [Andrew Gettelman, United States of America]	Accepted.
7-1130	7	38	1	38	3	Good statement [Daniel Murphy, United States of America]	Noted.
7-1131	7	38	1	38	4	These sentences form the crux of the argument and should begin the paragraph. [Robert Pincus, United States of America]	Accepted
7-1132	7	38	2	38	2	Rather than "hypothetical", is it "artificial"? [Leon Rotstayn, Australia]	Rejected. We believe that hypothetical is appropriate.
7-1133	7	38	6			the chapter 7.4.2.3 is not discussing the potential role of aerosol particle processing. It has been proven that clouds are not adiabatic. They are a "machine" that pumps through air and particles. The resident times in cloud are relatively short. Thus, there is constant mixing with the environment and possibly aerosol particles leave and enter clouds repeatedly. During each cloud pass they are modified, generally increased in size and more hygroscopic. And there is some indication that a cloud field can process the particles until they become suitable for precipitation development, e.g.If this could be proven more universally, it could severely limit the importance of aerosol particles for clouds, as the clouds will finally smooth them out (reduce number, increase size, increase hygroscipicity). [Andrea Flossmann, France]	Rejected. There is no reference to adiabatic clouds here. Nevertheless, the topic under discussion here is activation, which mostly takes place close to cloud base in adiabatic regions of the cloud. The issue of cloud processing raised by the reviewer is reflected in the brief phrase "The relative unimportance of composition is partially because aging makes particles more hygroscopic".

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7-1134	7	38	8	38	24	Section 7.4.2.3:Are citations available for this section (specifically for why aerosol composition is not all that important to RF_aci) [Government of United States of America]	Accepted. The most comprehensive study is Ervens et al. (2005) and supersedes an earlier paper by Feingold (2003). Added McFiggans (2006**)**
7-1135	7	38	10	38	10	The diameter is related to supersaturation and chemical composition, why say as 60nm? [Junying Sun, China]	Rejected. This is an attempt to be brief. The 60 nm number is approximate and reasonable for shallow clouds. No change.
7-1136	7	38	14	38	17	This "self-regulation" seems having little to do with why composition is secondary comparing to the size, also the sentences "(e.g., …)" are vague if not incorrect. [Chien Wang, United States of America]	Rejected. Composition is what might favour activation in the first place but the dynamical response that we describe, and stand by, make it less important.
7-1137	7	38	18	38	24	I appreciate any attempt to clarify complicated issues and I think the conclusion may be correct, but I find the logic hard to follow. How does it follow from the first sentence ("Since both ….") that "aerosol number concentration and size distribution" matter much more than "composition"? [Jón Egill Kristjánsson, Norway]	Accepted. Text clarified. In the general sense, if activation were only weakly sensitive to a certain parameter, but that parameter varied over a huge range, then the lack of sensitivity would be compensated for by the large range. This is the difference between a relative sensitivity and an absolute sensitivity.
7-1138	7	38	22	38	22	"cloud amount" is not an adequate quantity. [Chien Wang, United States of America]	Accepted. Changed to cloud condensate.
7-1139	7	38	24			It should be stressed here that as most fine particulate matter is secondary, atmospheric chemistry determines the composition and thus the size of the particles. Therefore, simulation of the atmospheric particle number concentration and size distribution by atmospheric models requires the simulation of their composition. [European Union]	Rejected. Chemistry is involved in aerosol formation and any process that affects aerosol loading is important. E.g., SOA formation represents a missing source of aerosol in some GCMs. However that is not what this text is discussing. The issue under discussion here is the importance of composition vis a vis drop activation. Given a certain size distribution of aerosol (i.e., same mass and number concentration), a change in composition is typically unimportant.
7-1140	7	38	32	38	33	Again, "cloud amount" should be replaced with more specific quantities. [Chien Wang, United States of America]	Accepted. Changed to cloud condensate.
7-1141	7	38	37			Section 7.4.3: The authors might consider adding a citation for the traditional view, such as Albrecht 1989 [Government of United States of America]	Reference to Albrecht (1989) added.
7-1142	7	38	44	38	46	This sentence (the third in the paragaph) is redundant with the first two. [Robert Pincus, United States of America]	Accepted.
7-1143	7	38	51			The authors should be careful about the use of the word feedback here, as it has a certain meaning in this chapter. [Government of United States of America]	Accepted. Changed feedback to adjustment
7-1144	7	38	52			Regarding the phrase: "hypothesized cloud amount effects" i.e. the traditional view? There could be other cloud amount effects...decreasing cloud amount through increased entrainment efficiency, for example [Government of United States of America]	Rejected. This is exactly our point. The traditional view only allows for increases in cloud amount.
7-1145	7	38	53	38	54	What is the basis for this statement? I know climate models, and I don't know of any that assume aerosol affects cloud amount. Many simulate very small increases in cloud cover with increasing aerosol, because they don't have direct physics for it. On the other hand, most simulate a large response (probably too large) of liquid water path to increasing aerosol, because their physics almost requires it. Unless appropriate citations can be provided, I recommend dropping this statement, or replacing with more discussion of aerosol effects on liquid water path. [Steven Ghan, United States of America]	Accepted. Text changed. The main point is that although GCM parameterizations do not include a direct link between aerosol and cloud amount, the autoconversion parameterization does generate this link.
7-1146	7	38	53	38	54	Climate models may be missing relevant processes, but they do not assume an “a priori” link between CCN and “cloud lifetime”. [Jean-Christophe Golaz, United States of America]	Accepted. Text changed. The main point is that although GCM parameterizations do not include a direct link between aerosol and cloud amount, the

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							autoconversion parameterization does generate this link.
7-1147	7	38	53	38	54	Is it really true that "Many climate models assume such an effect a priori"? I doubt it. What the models typically do is that they have an autoconversion parameterization that depends on cloud droplet size and thereby cloud droplet number concentration. This leads to an increase in LWP with increasing CCN concentration due to suppression of precipitation, and that may well lead to an increase in cloudiness. Isn't the problem rather that the models (especially the climate models) are missing other parts of the puzzle, e.g. they don't have sufficient vertical resolution to properly resolve the inversion, their turbulence schemes to not account properly for clouds and they don't resolve cloud dynamical effects (e.g., open vs. closed cells) which may in some cases cancel the "Albrecht effect" (Wood, 2007; Wang & Feingold 2009)? [Jón Egill Kristjánsson, Norway]	Accepted. Text changed. The main point is that although GCM parameterizations do not include a direct link between aerosol and cloud amount, the autoconversion parameterization does generate this link.
7-1148	7	38	53	38	54	This sentence seems a bit strong in terms of shortcomings in climate model parameterizations: “Many climate models assume such an effect a priori, which likely influences their forcing estimates.” This is not strictly true; most climate model microphysics and macrophysics parameterizations do not have a lifetime or cloud amount effect “built in” a priori. Instead, they generally include representations of autoconversion and accretion similar to finer-scale models (although these parameterizations generally must be tuned or otherwise modified to account for large grid spacing). Thus, I would argue that what is lacking is the inability of large-scale models to resolve small-scale cloud features and in particular coupling of microphysics and cloud dynamics, not that they assume lifetime and cloud amount effects a priori. [Hugh Morrison, United States]	Accepted. Text changed. The main point is that although GCM parameterizations do not include a direct link between aerosol and cloud amount, the autoconversion parameterization does generate this link.
7-1149	7	38	54	38	54	"climate models assume such an effect a priori". I question whether this is the most accurate way to describe this problem. It reads a bit like the statement from some climate skeptics that climate models "assume" certain feedbacks, such as the positive water vapour feedback, and we know that isn't correct. The problem with the GCMs is that they do not resolve many of the subtle effects that are discussed in Section 7.4.3. If they attempt to treat the "cloud lifetime effect", most GCMs do so via an autoconversion parameterization, in which the threshold and/or conversion rate depends on cloud droplet number, broadly in the way discussed in the first paragraph on page 40. Since increasing aerosols generally lead to increasing cloud droplet concentration (via an empirical or "mechanistic" parameterization), the only possible result in a GCM is that increasing aerosols cause a decrease of autoconversion, and hence an increase in cloud liquid-water content, since all the other interactions and possible interactions are not resolved. (Increasing liquid-water content may, in turn, cause cloud amount to increase, though the extent of this effect is parameterization-dependent.) Perhaps this statement about GCMs should be removed, and a couple of sentences added at the end of Section 7.4.3.3, preferably as a short paragraph. I don't see that the problem is that GCMs "assume", but that they only attempt to treat a subset of the physics, and this subset forces the effect to act in one direction. [Leon Rotstayn, Australia]	Accepted. Text changed. The main point is that although GCM parameterizations do not include a direct link between aerosol and cloud amount, the autoconversion parameterization does generate this link.
7-1150	7	39	4	39	22	It should be pointed out that there is NO observational evidence of increased cloud albedo on regional scale! Due to aerosol pollution [Henning Rodhe, Sweden]	Editorial. This is covered by Petters 2011.
7-1151	7	39	5	39	22	ship tracks are very popular to demonstarte that aerosol particles influence clouds. But it should be mentioned also that ships not only emit particles but also heat and water vapor. And a convincing study that separates the effects in missing in the text. So a discussion in this sense should be added. [Andrea Flossmann, France]	Rejected. The MAST experiment (1995) looked at this issue and showed no significant effect. In the interest of conciseness we don't discuss.
7-1152	7	39	5	39	22	The ship track field experiments frankly have been disappointing and NONE of them attempt to measure the cloud albedo despite the fact ALL are motivated by indirect radiative effects. This is clearly a grave omission and makes them much less relevant to the topic. An assessment ought to make such a statement. [Graeme Stephens, United States of America]	Accepted. Text changed to reflect this.
7-1153	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, United States of America]	Noted. Probably fair to say that they have more predictive capability in representing aci in the marine boundary layer.
7-1154	7	39	10			The text states that "cloud water responses can be of different sign". I'm not clear what that means. Does it mean that there are several responses that are of different sign from each other? The previous example does not specifically mention total cloud water - but droplet size and albedo - so it's not clear what this reference to	Accepted. Text changed

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						a different sign is referring to. This was reiterated by several reviewers. [Government of United States of America]	
7-1155	7	39	11	39	11	The term "A-train" was not previously introduced. [Robert Pincus, United States of America]	Accepted. Reference added.
7-1156	7	39	11	39	12	Surely, these numbers are merely averages, so please insert "on average" before "liquid water" [Jón Egill Kristjánsson, Norway]	Accepted. Text changed as suggested
7-1157	7	39	11	39	14	When the "A-Train" satellite is introduced here for the first time, the authors should consider inserting a reference to tell the readers what is "A-Train" satellite (e.g., L'Ecuyer and Jiang, 2010). Reference for "A-Train": L'Ecuyer, T.S., and J.H. Jiang, "Touring the atmosphere aboard the A-Train," Physics Today 63, 7, 36-41, 2010. [Government of United States of America]	Accepted. Reference added. But "A-Train" terminology is now removed.
7-1158	7	39	14	39	14	Typo: "A-Train satellite studies…" [Ralph Kahn, United States of America]	Accepted. But "A-Train" terminology is now removed.
7-1159	7	39	14	39	14	Replace "satellites studies of long-term" by "satellite studies of the influence of long-term" [Jón Egill Kristjánsson, Norway]	Acceted. Change as suggested
7-1160	7	39	14			The reference to volcanic outgassing effects in the middle of a paragraph otherwise about the effects of ship tracks seems out of place. It would seem more natural to deal with the ship tracks and then comment on the volcano effect. [Government of United States of America]	Accepted. Moved to end of paragraph.
7-1161	7	39	16	39	16	doi:10.5194/acp-11-7119-2011 also demonstrates the effect of volcanic emissions on clouds, and quite nicely. [Robert Pincus, United States of America]	Rejected. This section is on stratocumulus. We refer to this paper in the section on trade cumulus.
7-1162	7	39	16	39	16	Should this be adjusted forcing? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. Changed to ERFaci
7-1163	7	39	18	39	20	Peters et al (2011b) analysed three years of satellite data (2005 - 2007) and not two years as mentioned in the text. [Karsten Peters, Australia]	Accepted.
7-1164	7	39	21	39	21	Replace "are" with "is". [Steven Ghan, United States of America]	Accepted
7-1165	7	39	21	39	21	Typo: "…broken cloud scenes is also …" [Ralph Kahn, United States of America]	Accepted
7-1166	7	39	21	39	21	I found "significantly" a little unhelpful - could you be quantitative? A factor of 2 would not be much, but a factor of 10 would make me sit up [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. However, we have to be careful because local values can be higher but this doesn't mean global values will be.
7-1167	7	39	32	39	36	The situations being discussed seem a bit ideal, namely daytime stratus decks. Should mention also be made of what happens through the diurnal cycle and with decks that are not uniform. Perhaps this is mentioned elsewhere, but it seemed as if it might need to be mentioned here as well. For example, do all of the processes work through the diurnal cycle and stay balanced in the same way? [Michael MacCracken, United States of America]	Rejected. This is more detail than we can go into here.
7-1168	7	39	32	39	36	These two sentences are confusing. The first sentence does not mention aerosols, but the second sentence deals with aerosol perturbaiton. These need some clarification. [Minghuai Wang, United States of America]	Accepted. clarified.
7-1169	7	39	38	39	38	Change "Trade" to "Trade wind", because "Trade-cumulus" has no meaning [Jón Egill Kristjánsson, Norway]	Rejected. The AMS glossary of meteorology lists both. Shorter version maintained.
7-1170	7	39	39	39	39	Again "trade cumuli" should be "trade wind cumuli" [Jón Egill Kristjánsson, Norway]	Rejected. The AMS glossary of meteorology lists both. Shorter version maintained.
7-1171	7	39	39	39	39	"… shallow convective clouds…" followed by "… trade wind cumuli…" sounds repetitive [Jón Egill Kristjánsson, Norway]	Accepted. Changed as suggested
7-1172	7	39	41	39	41	Again "trade cumuli" should be "trade wind cumuli" [Jón Egill Kristjánsson, Norway]	Rejected. The AMS glossary of meteorology lists both.

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							Shorter version maintained.
7-1173	7	39	43	39	46	Observations of trade-wind cumulus show cloudiness tends to increase with precipitation amount, as noted. It'sProcessess that favor precipitation development also favor clouds, as noted. The deep clouds that support precipitation, reaching 4-5 km, encounter wind shear capable of shearing off upper-level cloud, thereby increasing overall cloud fraction. This is evident in Zuidema et al. (2012) but not noted here. Colliding outflows are also shown within Zuidema et al. (2012) to regenerate precipitation, but this isn't the immediate cause of the cloud fraction increase. [Paquita Zuidema, United States of America]	Accepted. Text changed as suggested.
7-1174	7	39	44	39	44	Replace "clouds" by "cloud development" [Jón Egill Kristjánsson, Norway]	Accepted.
7-1175	7	39	50			In describing the method of Schmidt et al, 2009, the text says that the technique involves comparison of a measured irradiance field with the same field calculated in a fine-scale model …" by "the same field" do the authors just mean the irradiance field in the model ? Or is the irradiance field somehow constrained toward the measured field? The text could use clarifying in this regard. [Government of United States of America]	Accepted. the authors mean the irradiance field in the model. Clarified.
7-1176	7	40	1	40	13	If clouds process the aerosol particles during their life time, then the relation between autoconversion and a fixed drop concentration is pointless. The drop concentration will probabaly decrease with time [Andrea Flossmann, France]	Rejected. The reviewer is correct. But the intent here is to show a functional dependence of rainrate on drop number.
7-1177	7	40	3	40	13	This paragraph discusses some interesting points concerning the roles of autoconversion and accretion in rain formation. However, I think this could be a bit confusing because the scaling of rain formation due to autoconversion applied in some GCMs (e.g., Khairoutdinov and Kogan 2000) is compared with the overall scaling of rain formation from theoretical and observational studies that includes both accretion and autoconversion implicitly or explicitly. This may overstate differences in the scaling between the GCMs and theoretical/observational studies. This is not to say that GCMs have a correct balance of autoconversion and accretion; in fact there is evidence to suggest that they do not, which could lead to an incorrect overall scaling of rain formation with droplet number (e.g., Posselt and Lohmann 2009; Wang et al. 2012). But this presents a somewhat different picture than implying that the scaling associated with the autoconversion itself is wrong. This is supported by Wang et al. (2012), who use the same autoconversion/accretion formulations (Khairoutdinov and Kogan 2000) in two different models, in particular, one with prognostic and one with diagnostic treatment of precipitation, and also using different grid resolutions (MMF vs. CAM). This yields large differences in autoconversion/accretion balance and overall sensitivity to aerosols and droplet concentration between the models despite using the same microphysics process formulations (Wang et al. 2012). [Hugh Morrison, United States]	Accepted. The focus in 7.4.3.3 is on the process level. The reviewer raises valid points but the question of how autoconversion and accretion are represented in a GCM is a separate issue. Some of the differences between the scaling with N from observations might in fact be due to a different in the balance between autoconversion and accretion. **T**he difference in precipitation formation in a GCM has much more to do with the model infrastructure (e.g., standard model vs MMF) than with the actual parameterization. We now add text to this effect in 7.4.3.4, which deals with large scale modeling studies and refer to the Wang et al. 2012 study.
7-1178	7	40	4	40	13	The discussion about autoconversion and accretion misses the most important finding that puts it all in perspective: It was already acknowledged in lines 3 and 4 of this page that autoconversion starts when the cloud drop effective radius exceeds a certain threshold. This text has to be complemented with the observations that the effective radius increases with height above the convective cloud base, and it reaches the autoconversion threshold at a height that is linear with the number concentration of nucleated cloud drops (Freud E., and D. Rosenfeld, 2012: Linear relation between convective cloud drop number concentration and depth for rain initiation. J. Geophys. Res., 117, D02207, doi:10.1029/2011JD016457; Konwar M., R.S. Maheskumar, J. R. Kulkarni, E. Freud, B. N. Goswami and D. Rosenfeld, 2012: Aerosol control on depth of warm rain in convective clouds. J. Geophys. Res., Early online release). Accretion can occur only after autoconversion initiates the rain drops that fall from the cloud top. [Daniel Rosenfeld, Israel]	Rejected. The linear dependence of depth of onset of rain on drop number concentration is a level of detail that we cannot afford to go into. Threshold behaviour of drop effective radius, the underpinning of this idea, is however mentioned.
7-1179	7	40	4			The reference of Wang et al., 2011a is brought to support the existence of a threshold effective radius for initiation of autoconversion. This reference does not make this point. Instead, it should be replaced with the reference: Rosenfeld D., Wang H., and Rasch P. J., 2012: The roles of cloud drop effective radius and LWP in determining rain properties in marine stratocumulus. Geophys. Res. Lett., 39, L13801, doi:10.1029/2012GL052028, 2012. There are many more references for the existence of this threshold. Therefore, e.g. should be added before the references. [Daniel Rosenfeld, Israel]	Accepted. Changed as suggested.
7-1180	7	40	12			The text states that "clouds generate rain via accretion of cloud drops by raindrops". Would it be more correct to say that the accretion intensifies or amplifies the rain? If there are already raindrops, then the accretion is not generating the rain. [Government of United States of America]	Accepted. Changed as suggested

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7-1181	7	40	15	40	17	Regarding the statement, "The balance of evidence suggests that autoconversion likely scales with approximately the inverse square root of droplet concentration and that liquid water path has significantly more leverage over precipitation than does droplet concentration.": If the report is going to say this in the end, the detailed description of studies regarding precipitation susceptibility in the previous paragraph seems unnecessary. [Government of United States of America]	Rejected. This statement is factual - providing perspective rather than suggesting that the aerosol plays no role at all.
7-1182	7	40	15	40	17	This statement is a bit unclear. There are several autoconversion parameterizations with time constant slower or faster than that to bring the autoconversion to the inverse square root law, so it is better to give references on which this statement is based. [Teruyuki Nakajima, Japan]	Rejected. The references are in the paragraph above.
7-1183	7	40	15	40	17	The reason for the sensitivity to LWP is in fact the sensitivity to cloud depth. Rosenfeld, Wang and Rasch (2012) have shown that, for clouds with drops that exceed the effective radius autoconversion threshold near their tops, the rainrate is proportional to H squared, where H is the cloud depth. This is because accretion dominates, and maximum potential LWP is proportional to H squarred. For clouds that do not exceed that threshold, the rain rate is proportional to H^2 Nd^-5/3, where Nd is cloud drop number concentration. Nd comes into play only when the autoconversion threshold is not exceeded, and becomes unimportant beyond it. This is fundamentally important, and needs to be stated in such an overview summary. [Daniel Rosenfeld, Israel]	Accepted. The main issue is the relative importance of autoconversion vs accretion since the former is drop concentration dependent and the latter is not.
7-1184	7	40	15	40	19	GCM predicted AFaci is very sensitive to details of the autoconversion. Golaz et al. 2011 (doi:10.1175/2010JCLI3945.1) showed that Afaci can vary by 1 W/m2 depending on autonversion assumptions. [Jean-Christophe Golaz, United States of America]	Accepted. Note: We do not discuss in detail here because this is text on process-level understanding. Reference to Golaz is now in 7.4.3.4
7-1185	7	40	15	40	19	The role of autoconversion in determing aerosol indirect effecdts are also clearly disucssed in the aforementioned paper (Wang et al., 2012). It can further strength the points made in this paragraph. [Minghuai Wang, United States of America]	Accepted. 7.4.3.3. deals with process level understanding. We now add statements on sensitivity in GCMs in 7.4.3.4
7-1186	7	40	17	40	19	My previous comment, which pertains to lines 15-17 in the same page, can explain the regimes where R is sensitive to cloud drop number concentration and when the sensitivity vanishes. The description in the referenced comment (for lines 15-17) explains this simply and fundamentally. [Daniel Rosenfeld, Israel]	Rejected. We simply can't digress to depth issues since they are also tied to LWP issues. However, we do make it clear that rain is a stronger function of LWP and cloud depth than drop concentration.
7-1187	7	40	21	40	22	Satellite based correlations of cloud amount and AOD are cited here without reference to the very likely retrieval issues (e.g. Quaas et al., 2010) [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Rejected. Uncertainties have been clearly stated in 7.4.1.2. Multiple lines of evidence are used (Satellite, small-scale modeling). For shiptracks, we don't need to measure aerosol.
7-1188	7	40	21	40	23	Regarding the statement, "Small-scale studies...tend to confirm two responses of the cloud liquid water to increasing aerosol" the authors might consider noting that the system is more complex. Cloud base height (Wood, JAS 2007) and the initial cloud amount (Turner et al BAMAS 2007) are just two examples of other possible modulating factors. [Government of United States of America]	Accepted. Added a few qualifying words to express even greater nuance.
7-1189	7	40	21	40	26	This paragraph uses "cloud amount" and "cloud liquid water" as if they are synonymous. They are not. The statement "nor will it be in the foreseeable future" is not only impossible to defend but also unture. Replace it with "but emerging approaches (Guo et al., 2010) show promise." Guo, H., J.-C. Golaz, L. J. Donner, V. E. Larson, D. P. Schanen, and B. M. Griffin, 2010: A dynamic probability density function treatment of cloud mass and number concentrations for low level clouds in GFDL SCM/GCM. Geosci. Model Dev., 3, 475–486. [Steven Ghan, United States of America]	Accepted. change cloud amount to cloud water. Added mention of PDF approach since Guo study is showing nuanced response.
7-1190	7	40	21			Add missing relevant publication here: Bretherton et al. 2007 (doi:10.1029/2006GL027648) [Jean-Christophe Golaz, United States of America]	Accepted. Added reference
7-1191	7	40	24	40	25	Please provide a brief physical explanation for why "Under non-precipitating conditions, clouds tend to thin in response to increasing aerosol" [Jón Egill Kristjánsson, Norway]	Accepted. Few words added.
7-1192	7	40	24	40	25	Lines 24-25: I think that “tend to thin” should read “tend to be thin” [Kuo-Nan Liou, U.S.A.]	Rejected. Thin is used here as a verb. No change
7-1193	7	40	25			Please explain why in a sentence. The second statement (thinning of non-precip clouds) does not seem	Accepted. Few words added.

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						obvious. [Andrew Gettelman, United States of America]	
7-1194	7	40	26			However, some modern high-order clouds and turbulence parameterizations can reproduce the dual responses of the cloud liquid water to increasing aerosols (Guo et al 2011, doi:10.1029/2011GL048611). [Jean-Christophe Golaz, United States of America]	Accepted. Added mention of PDF approach since Guo study is showing nuanced response.
7-1195	7	40	31	40	33	This subsection is under the section for adjustment processes of liquid water clouds, not only ship trail clouds or low level clouds off the west coast of continents. The other side of the ocean tends to have clouds which do not respond so sensitively to the CCN injection as shown by a SBM simulation (Iguchi et al., JGR, VOL. 113, D14215, doi:10.1029/2007JD009774, 2008). [Teruyuki Nakajima, Japan]	Accepted. Paper referred to in 7.4.1.3
7-1196	7	40	32	40	32	What do you mean by "cloud-free shadows"? [Jón Egill Kristjánsson, Norway]	Accepted. changed to cloud-free downdraft "shadows"
7-1197	7	40	34	40	35	Section 7.4.3.3 : « These underscore the imprudence of applying simplistic rules for aerosol-cloud-precipitation interactions in GCMs » : it is an important statement. Are these simplistic rules actually in use in all CMIP5 models ? or just in some of them ? [Sandrine BONY, France]	Editorial. Text revised based on other comments.
7-1198	7	40	34	40	35	Again, I somewhat disagree that GCMs apply simplistic rules regarding interaction of aerosols-clouds-precipitation. GCM microphysics schemes are now applying microphysical formulations of complexity similar to high-resolution models; the problem in my view is the inability of GCMs to resolve the cloud dynamics and coupling of cloud dynamics and microphysics. This is a bit different than saying that the models assume these interactions and build them into the parameterizations a priori. This comment is very similar to comment #15 above. [Hugh Morrison, United States]	Accepted. We now add a few sentences that highlight the issue of model infrastructure vs microphysical parameterization
7-1199	7	40	34	40	35	This section seems unnecessary negative. It is easy to criticise GCMs and clearly certain simplistic parameterisations are unsatisfactory. However, I do not see the point of referring with such a vague reference to "simplistic rules" as imprudent. I would suggest to ommitt or clearly specify what is referred to as imprudent. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted. We now add a few sentences that highlight the issue of model infrastructure vs microphysical parameterization
7-1200	7	40	39	40	52	Albeit the role of aerosols in Afaci is relatively less predominant as compared to their pre-cursors globally, the regional effects and thereby their proper representation in regional models is highly essential in climate change projections. [Panuganti, C.S. Devara, India]	Noted. We agree but it is not clear what change the reviewer is requesting.
7-1201	7	40	40	40	40	Add reference to Yang et al. (2012): Yang, Q., W. J. Gustafson, Jr., J. D. Fast, H. Wang, R. C. Easter, M. Wang, S. J. Ghan, L. K. Berg, L. R. Leung, and H. Morrison, 2012: Impact of anthropogenic and natural aerosols on stratocumulus and precipitation in the Southeast Pacific: A regional modeling study using WRF-Chem, Atmos. Chem. Phys., 12, 8777–8796, doi:10.5194/acp-12-8777-2012. [Steven Ghan, United States of America]	Rejected. Yang 2011 already referred to and Yang et al. 2012 doesn't add any added value to the discussion.
7-1202	7	40	41	40	41	Need to insert "regional" after "Some of these" [Jón Egill Kristjánsson, Norway]	Accepted. Changed as suggested
7-1203	7	40	42	40	42	Climate models also undergo regular evaluation (see Chapter 9!). I presume you mean that the forecasts can be assessed routinely and often against observations [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. "regular" changed to "routine"
7-1204	7	40	45	40	45	After Painemal and Zuidema, 2010 add reference to Wood et al. (2012): Wood, R., D. Leon, M. Lebsock, J. Snider, and A. D. Clarke (2012), Precipitation driving of droplet concentration variability in marine low clouds, J. Geophys. Res., 117, D19210, doi:10.1029/2012JD018305. [Steven Ghan, United States of America]	Rejected. This reference uses a heuristic model, not a regional model and so doesn't fit here.
7-1205	7	40	48	40	52	This passage is out of place, as it applies to adjustments in cold clouds as well. The authors might consider revising the text accordingly. [Government of United States of America]	Accepted. Moved to 7.4.5 (Synthesis)
7-1206	7	40	48	40	52	This text is too vague to be of any value. Skip it or rewrite it. [Jón Egill Kristjánsson, Norway]	Accepted. Moved to 7.4.5 (Synthesis)
7-1207	7	40	48	40	52	Clearly compensation could be important. However, this conclusion does not seem to be sufficiently backed up by evidence. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Rejected. Evidence summarized in Stevens and Feingold (2009)
7-1208	7	40	54	42	40	The definition of "cold clouds" and "mixed-phase clouds" could be made clearer in this section. Traditionally cold or warm clouds is defined by cloud base temperature. Is this the cloud type that is meant to be adopted	Rejected. We understand the points but we believe we have added the appropriate qualifiers e.g. "mixed

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						and discussed here. For the mixed-phase clouds, because of the currently popular topic of a special case in arctic, the usage of this term is not always clear. Actually, mixed-phase region (not necessarily a separate cloud) commonly exist inside deep convection, and some works cited here were actually studies on this type of cases. [Chien Wang, United States of America]	phase clouds, containing both liquid water and ice…" or "cirrus (ice-only)". In addition our discussion of mixed phase Arctic stratus makes it clear that we are in the Arctic regime.
7-1209	7	41	1	41	2	Format: -38°C should be kept on one line or the other. [Robert Kandel, France]	Accepted.
7-1210	7	41	3	41	3	"homogeneously" is of course correct, but I was expecting to see the word "spontaneously" in this introductory sentence. [Jón Egill Kristjánsson, Norway]	Editorial. Homogeneously retained.
7-1211	7	41	5	41	5	"… (on the order of 1 1-1)…" Need units here. If this is really 0.1, it would be clearer to use decimal rather than scientific notation in this case. [Ralph Kahn, United States of America]	Accepted. Units are explicit (1 per litre). But due to the similarity between 1 and l we now spell out litre.
7-1212	7	41	9	41	10	This ("soluble matter can hinder glaciation …") contradicts to the text on lines 18-20 on page 42. Perhaps put these two opposing effects next to each other? [Jón Egill Kristjánsson, Norway]	Rejected. No contradiction. In the earlier case we are referring to soluble material suppressing freezing whereas later we refer to insoluble IN aiding nucleation.
7-1213	7	41	14	41	21	The BF process lets ice particles grow at the expense of the liquid water. However, it only works in dynamically weak clouds. In clouds with high vertical velocities, you can sustain a state where both ice and liquid grow, as the humidity is maintained in the supersaturated state with respect to ice AND liquid. [Andrea Flossmann, France]	Accepted. Changed text to include this nuance (7.4.4.4).
7-1214	7	41	14			grammar lesson: never start a sentence with "because". Replace by "as". [Andrea Flossmann, France]	Editorial. Rejected.
7-1215	7	41	15	41	16	"at the expense of the liquid water": As shown by Korolev (2007 in JAS), this is only true if the updrafts are rather weak. In the case of strong updrafts, both cloud droplets and ice crystals will grow, so the Wegener-Bergeron-Findeisen effect does not kick in. [Jón Egill Kristjánsson, Norway]	Accepted. Change text to include this nuance. (7.4.4.4)
7-1216	7	41	16			Cite earlier experimental evidence, e.g. Schwarzenböck, A., Mertes, S., Heintzenberg, J., Wobrock, W. and Laj, P. 2001. Impact of the Bergeron Findeisen Process on the Release of Aerosol Particles During the Evolution of Cloud Ice. Atmos. Res. 58, 295-313 and Schwarzenböck, A., Mertes, S., Heintzenberg, J. and Wobrock, W. 1999. First in Situ Evidence for Bergeron-Findeisen Process. In: Proceedings of the European Aerosol Conference, Prag, 1999, S131-S132. [Jost Heintzenberg, Germany]	Accepted. Added first reference.
7-1217	7	41	17	41	17	"This favours the depositional growth of large ice crystals, which may sediment away". Doesn't it favour all ice crystals, so that they rapidly grow to large sizes? [Jón Egill Kristjánsson, Norway]	Accepted. Changed text to clarify that larger crystals have larger fall velocities.
7-1218	7	41	19	41	21	Not just cloud amount, but also cloud optical thickness (both because of the greater tendency for ice to sediment out and because of the different optical properties of ice crystals vs. liquid droplets, e.g., their different phase functions). [Anthony Del Genio, United States of America]	Accepted.
7-1219	7	41	23	41	23	It is the phase change between liquid and ice that provides enthalpy to the environment, not the ice phase itself. Replacing enthalpy with energy would be less exact but substantialy more communicative. [Robert Pincus, United States of America]	Accepted.
7-1220	7	41	23	41	24	"the ice-phase provides enthalpy to the environment, which influences cloud dynamics": I would have found it more intuitive to say that the latent heat of freezing influences cloud dynamics. [Jón Egill Kristjánsson, Norway]	Accepted. Changed wording (see comment above)
7-1221	7	41	23			"ice phase provides enthalpy" is wrong. The phase transition provides enthalpy, not the ice phase itself. Maybe rather call it latent heat, easier to understand for the non-thermodynamicists. [Andrea Flossmann, France]	Accepted. Changed wording (see comment above)
7-1222	7	41	29			I believe the field programs being referred to here occurred in 2008 and not 2009 as stated. [Hugh Morrison, United States]	Accepted. Changed to 2008
7-1223	7	41	30			There are additional long-term monitoring sites besides Barrow that are now being used for cloud	Accepted. change to "higher northern latitude stations

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						observational studies, such as at Eureka, Canada (de Boer et al. 2011). [Hugh Morrison, United States]	(e.g. Shupe et al.)
7-1224	7	41	39	41	41	"If ice indeed forms ….": I do not find this entirely clear. First of all, what exactly is the "feedback" here? Secondly, the WBF process only works in rather weak updrafts. Thirdly, doesn't the WBF process rapidly deplete the cloud, rather than sustain it? Is there a reference for this hypothesis? [Jón Egill Kristjánsson, Norway]	Accepted. Wording clarified/ Reference Morrison 2012
7-1225	7	41	39	41	41	the last sentence is not clear and needs clarification. I do not understand why when ice forms, it will restrict further ice furmaiton. [Minghuai Wang, United States of America]	Accepted. See if wording clarified/ Reference Morrison 2012
7-1226	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 decrease as well as increase in some clouds. [Robert Wood, United States of America]	Rejected. This section is discussing ice-phase processes and not warm phase where shiptracks would be relevant. Coakley's work is adequately referred to earlier.
7-1227	7	41	46	41	46	Not clear what is meant by "abovementioned field experiments" [Jón Egill Kristjánsson, Norway]	Accepted. Referring to 2004 and 2008 experiments (MPACE and ARCTAS/ARCPAC/ISDAC). Added words to help link back.
7-1228	7	41	48	41	48	What do you mean by "a level of understanding of very detailed processes"? A high level of understanding, perhaps? [Jón Egill Kristjánsson, Norway]	Accepted.
7-1229	7	41	49	41	49	I'd suggest explaining on the term "particle habit" [Peter Irvine, Germany]	Accepted. changed to crystal shape
7-1230	7	41	49	41	50	I find it very difficult to follow "similar influence … to the choice of different nucleation mechanism". Please rephrase! [Jón Egill Kristjánsson, Norway]	Accepted. small mods made to clarify
7-1231	7	42	1	42	1	Insert "developing" before "heterogeneous freezing" [Jón Egill Kristjánsson, Norway]	Rejected. briefer form retained.
7-1232	7	42	1	42	9	This paragraph seems to conflate the Bergeron process with ice nucleation processes, which may be confusing. The first sentence in the paragraph discusses heterogeneous freezing parameterizations. The second discusses the Bergeron process. The third sentence then discusses nucleation parameterizations in more detail, but beings with “Other parameterizations…”. The treatment of the Bergeron process is distinct from the treatment of ice nucleation, and concerns the growth of existing ice particles but not the nucleation of new ice particles. This could be reworded to make the distinction clearer. [Hugh Morrison, United States]	Accepted. text changed. Removed Korolev and WBF discussion.
7-1233	7	42	8	42	9	Ekman et al. (2007; Q. J. Roy. Meteor. Soc., 133B, 1439-1452) should be cited. The paper discussed the infleunce between heteorogeneous and homogeneous nucleation regarding convective cloud development and anvil features, and supported by observational evidence. The design of simulations is also better in comparison, the model has better described aerosol cycle including nucleaion and impaction scavenging, i.e., the aerosol is not assumed driven by an open system. [Chien Wang, United States of America]	Accepted
7-1234	7	42	11	42	11	Replace "less" by "lower" [Jón Egill Kristjánsson, Norway]	Accepted. Changed
7-1235	7	42	11	42	11	Here the threshold temperature is set at -35C, but on the previous page (line 9) it was -38C. Please harmonize! [Jón Egill Kristjánsson, Norway]	Rejected. Probability issue? -38 is not absolute
7-1236	7	42	13	42	13	Before Barahona and Nenes, cite Liu and Penner, 2005: Liu, X., and J. E. Penner (2005), Ice nucleation parameterization for global models, Meteorol. Z. [Berlin], 14(4), 499–514. [Steven Ghan, United States of America]	Accepted
7-1237	7	42	23	42	40	This section is not well constructed and organized, especially the second paragraph (lines 31-40). Though the title of the section is about advancs in large-scale modeling studies, I am not sure why the paragraph keeps talking about how bacteria and biological particels affect ice clouds. [Minghuai Wang, United States of America]	Accepted. Reworked lines 31-40
7-1238	7	42	26	42	26	Insert "in models" after "freezing processes" [Jón Egill Kristjánsson, Norway]	Accepted.
7-1239	7	42	27	42	27	Insert "for" before "mixed-" at the end of the line [Jón Egill Kristjánsson, Norway]	Accepted.

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7-1240	7	42	31	7	36	My opinion is that the biological material is too controversial and uncertain to merit this much space in the assessment. For example, lines 35-36 about anthropogenic changes to the biosphere, while true, is really pretty far into speculation. [Daniel Murphy, United States of America]	Accepted. Text removed.
7-1241	7	42	33			Typo [Jost Heintzenberg, Germany]	Rejected. Unclear what the typo is. Not changed.
7-1242	7	42	38	42	38	What is meant by "background cirrus"? [Jón Egill Kristjánsson, Norway]	Accepted. Clarified.
7-1243	7	42	39	42	39	What is meant by "its effect"? [Jón Egill Kristjánsson, Norway]	Accepted. "its" changed to "the"
7-1244	7	42	42	44	18	Although this may be dealt with in some of the papers cited, I see no explicit mention in this section of latitude dependence, on the one hand for GCR penetrating the magnetosphere and atmosphere, on the other hand, as a function of altitude for relative importance of LW and SW CRE. [Robert Kandel, France]	Noted and correct but no change made to the text. Since the changes in cloudiness canot be attributed to cosmic rays, it is not needed to go in details as to what latitude and altitude the ionization rate from cosmic ray is higher.
7-1245	7	42	42	44	18	Comments on 7.4.5 Impact of cosmic rays on aerosols and clouds This chapter reviews the recent progress of study for the cosmic ray impact on aerosols and clouds. This issue is still highly debatable. Although many papers have been published recently, we do not yet have a clear conclusion on whether and how galactic cosmic ray affects climate, because the results of different approaches are not consistent with each other. The laboratory experiments (e.g. Kirkby et al. 2011) demonstrated that the enhancement of nucleation due to ionization is possible, if some conditions of temperature and chemical composition are satisfied. However, the field measurements do not show clear evidence that the ion-induced nucleation can work as the major process of CCN formation. Therefore, we should conclude that the influence of cosmic ray on cloud is still open to be solved. However, the statement in Executive Summary that the effect of cosmic ray is too weak to have any climate influence (lines 30 to 36 in page 139) seems to be too hasty and a little biased, I think. In addition, some references suggesting the cosmic ray influence are not cited in this chapter. For instance, although the influences on cloud and climate of Forbush decreases of cosmic ray have been pointed out not only by Svensmark et al. (2009) but also by Pudovkin and Veretenenko (1995), Artamonova and Veretenenko (2011), Dragic et al. (2011), they are not cited in sec. 7.4.5.1. Hong et al. (2011) is also related to this topic. These references should be included. References Artamonova, Irina; Veretenenko, Svetlana, Galactic cosmic ray variation influence on baric system dynamics at middle latitudes, Journal of Atmospheric and Solar- Terrestrial Physics, Volume 73, Issue 2-3, p. 366-370, DOI:10.1016/j.jastp.2010.05.004 Dragić, A.; Aničin, I.; Banjanac, R.; Udovičić, V.; Joković, D.; Maletić, D.; Puzović, J., Forbush decreases - clouds relation in the neutron monitor era, Astrophysics and Space Sciences Transactions, Volume 7, Issue 3, 2011, pp.315-318, DOI:10.5194/astra-7-315-2011 Hong, Peng K.; Miyahara, Hiroko; Yokoyama, Yusuke; Takahashi, Yukihiro; Sato, Mitsuteru, Implications for the low latitude cloud formations from solar activity and the quasi-biennial oscillation, Journal of Atmospheric and Solar-Terrestrial Physics, Volume 73, Issue 5-6, p. 587-591, DOI: 10.1016/j.jastp.2010.11.026 Pudovkin, M. I.; Veretenenko, S. V., Cloudiness decreases associated with Forbush decreases of galactic cosmic rays., Journal of Atmospheric and Terrestrial Physics, Vol. 57, No. 11, p. 1349 – 1355, [Kanya Kusano, Japan]	noted but no change to the text is made. The SOD discusses both the laboratory experiments and the lack of clear evidence from field measurements. The rationale for the statement in the executive summary is clearly laid out in the synthesis subsection. FInally we can only cite a sample of available references in the published literature. The suggested references have been assessed and do not add to the chosen selection.
7-1246	7	42	42	44	18	In AR5, it is great that the space weather effect on the climate is involved. Unfortunatley, only the galactic cosmic ray effect (GCR) was discussed and only the statistic work of GCR effect on cloud cover was discussed. Another large part of work with high evidence was missed, which indicated that spaceweather, including GCR, solar proton, relativisitic electron flux, interplanet magnetic field change, can impact on tropospheric circulation as reviewed by Tinsley (2008) and (2012). This means the cloud CCN change by GCR	Rejected. Section 7.4.5 already includes a paragraph to discuss the possibility of GEC pathway. The literature on the subject is too embryonic to include further text.

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						or GEC at special region can significantly influence the tropospheric circulation. I suggest AR5 adding the discussion of these work and adding more contributing author to revise this section such as Dr. Brian Tinsley (USA) and Dr. Limin Zhou (China) or Dr. Gile Harrison (UK). [Limin Zhou, China]	
7-1247	7	42	42			Section 7.4.5: In the review instructions, it was suggested in particular that we come up with ways to reduce the length of the text. I might suggest reducing the length of the section on cosmic ray influences. This section is certainly interesting, but is such a detailed review necessary given the synthesis statement (p. 7-44, lines 13-15): while “there is some evidence that ionization from cosmic rays may enhance aerosol nucleation in the free troposphere, there is medium evidence and high agreement that the cosmic ray-ionization mechanism is too weak to influence global concentrations of CCN or their change over the last century or during a solar cycle in any climatically significant way”? [Hugh Morrison, United States]	Rejected. Thank you for the suggestion, but we feel it is important to lay out a clear argumentation of our assessment.
7-1248	7	42	44	42	44	Section 7.4.5 : activity (typo). [Sandrine BONY, France]	Editorial. Corrected.
7-1249	7	42	44	42	44	"High solar acti0vity" [Eimear Dunne, Finland]	Editorial. Corrected.
7-1250	7	42	44	42	44	“acti0vity” =>activity [Paul Ginoux, United States of America]	Editorial. Corrected.
7-1251	7	42	44	42	44	Is “acti0vity” should be “activity” [Government of Poland]	Editorial. Corrected.
7-1252	7	42	44	42	44	remove 0 from "acti0vity" [Peter Irvine, Germany]	Editorial. Corrected.
7-1253	7	42	44	42	44	Typo: "High solar activity…" [Ralph Kahn, United States of America]	Editorial. Corrected.
7-1254	7	42	44	42	44	Misspelling: "acti0vity" should be "activity" [Jón Egill Kristjánsson, Norway]	Editorial. Corrected.
7-1255	7	42	44	42	44	Line 44: Typo “acti0vity” [Kuo-Nan Liou, U.S.A.]	Editorial. Corrected.
7-1256	7	42	44	42	44	High solar activity … [Ottmar Möhler, Germany]	Editorial. Corrected.
7-1257	7	42	44	42	44	acti0vity -> activity [Raimund Muscheler, Sweden]	Editorial. Corrected.
7-1258	7	42	44	42	44	Please correct: "High solar acti0vity …." by "High solar activity …" [Rubén D Piacentini, Argentina]	Editorial. Corrected.
7-1259	7	42	44	42	44	"activity" is mis-spelled. [Robert Pincus, United States of America]	Editorial. Corrected.
7-1260	7	42	44	42	44	typo in "activity" [Leon Rotstayn, Australia]	Editorial. Corrected.
7-1261	7	42	44	42	44	Change "acti0vity" to "activity". [Chien Wang, United States of America]	Editorial. Corrected.
7-1262	7	42	44			.. acti0vity: replace by activity [Urs Baltensperger, Switzerland]	Editorial. Corrected.
7-1263	7	42	44			“High solar activi0ity ...” should be “High solar activity ...” [James Coakley, United States of America]	Editorial. Corrected.
7-1264	7	42	44			typo 'acti0vity"!! [Andrea Flossmann, France]	Editorial. Corrected.
7-1265	7	42	44			Typo: activity [Andrew Gettelman, United States of America]	Editorial. Corrected.
7-1266	7	42	44			typo: extra () [European Union]	Editorial. Corrected.
7-1267	7	42	44			7.4.5. : This section seems to be the only part to treat Galactic Cosmic Rays (GCR) effects on climate in WG1, AR5. The recent paleoclimatological study such as Yamaguchi et al. (2010, PNAS, 107, 48, 20697-702) revealed some findings. Therefore, it would be better to assess GCR effects on the climate under much cleaner environment in pre-industrial time. [Government of Japan]	Noted. This comment does not produce a rationale as to why it would be better to assess GCR effects in a cleaner environment. The motivation of this section is to assess whether GCR can induce changes in cloudiness in the present-day atmosphere and climate. We can only cite a few studies in the introduction to this subsection.
7-1268	7	42	44			(Noted by three reviewers) "acti0vity" should be "activity" [Government of United States of America]	Editorial. Corrected.
7-1269	7	42	44			typo ‘High solar acti0vity’ should read ‘High solar activity’ [Benjamin Laken, Spain]	Editorial. Corrected.

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7-1270	7	42	44			Typo: "acti0vity" [Forrest Mims, United States of America]	Editorial. Corrected.
7-1271	7	42	46	42	46	"as GCR is" replace with "as GCR are"? [Peter Irvine, Germany]	Editorial. Corrected.
7-1272	7	42	46	42	46	Cosmic rays are the primary source of ionization in the upper troposphere and stratosphere. In the mesosphere and thermosphere, and in the atmosphere as a whole, solar uv produces more charged particles, by more than an order of magnitude (e.g., Marsh and Svensmark, Space Science Reviews 2000). [Ralph Kahn, United States of America]	Taken into account. We now state that cosmic rays are the primary source of ionization in the troposphere.
7-1273	7	42	47	42	47	Replace "relative" by "relatively" [Jón Egill Kristjánsson, Norway]	Editorial. Corrected.
7-1274	7	42	47	42	47	"relatively" [Leon Rotstayn, Australia]	Editorial. Corrected.
7-1275	7	42	50	42	50	Replace "in" by "into" [Jón Egill Kristjánsson, Norway]	Editorial. Corrected.
7-1276	7	43	1	43	2	Many empirical relationships have been reported between GCR or cosmogenic isotope archives and some aspects of the climate system (e.g., Bond et al., 2001; Dengel et al., 2009; Ram and Stolz, 1999). This is slightly misleading since e.g. Bond et al. report a link between solar activity and climate but not necessarily cosmic rays and climate (cosmogenic isotopes are proxies for cosmic ray and solar activity variations). [Raimund Muscheler, Sweden]	partly taken into account. Sentence has been rephrased and moved to the chapeau.
7-1277	7	43	1	43	5	The Second Order Draft acknowledges strong evidence of solar forcing beyond TSI but still needs to take account of the implications In a huge improvement over the First Order Draft, the SOD acknowledges strong evidence for a solar forcing more powerful than the slight variance in Total Solar Insolation. Both drafts cite a few papers that find correlations between solar activity and climate, but the SOD now adds the following sentence (p. 7-43, lines 2-4): "The forcing from changes in total solar irradiance alone does not seem to account for these observations, implying the existence of an amplifying mechanism such as the hypothesized GCR-cloud link." This important acknowledgment requires corresponding changes throughout the report that have still not been made. The main conclusion of the entire report, stated in the first line of the Executive Summary, is that advances since AR4 "further strengthen the basis for human activities being the primary driver in climate change" (p.1-2, lines 3-5, unchanged from the FOD). This conclusion is a direct result of the Chapter 8 assertion that: "There is very high confidence that natural forcing is a small fraction of the anthropogenic forcing." (Page 8-4, line 54.) As the next three lines in Chapter 8 explain, this assertion is arrived at by comparing anthropogenic forcings only to TSI and volcanic aerosols (p. 8-4, line 55-57): "In particular, over the past three decades (since 1980), robust evidence from satellite observations of the TSI and volcanic aerosols demonstrate a near-zero (–0.04 W m–2) change in the natural forcing compared to the anthropogenic AF increase of ~1.0 ± 0.3 W m–2." But as the SOD now acknowledges, there is strong evidence for solar forcing more powerful than TSI. That evidence invalidates any comparison between natural and anthropogenic forcings that does not include any solar effects but TSI. Thus the chapter 8 premise needs to be altered: there can be no "high confidence" that natural forcing is a small fraction of the anthropogenic forcing. And the Executive Summary conclusion must be similarly altered: the accumulation of evidence for a solar forcing beyond TSI does not "further strengthen the basis for human activities being the primary driver in climate change," but weakens it considerably. How much? As I noted in my FOD comments, dozens of studies have found correlations of .4 to .7 between solar activity and various measures of climate (temperature, rainfall, etcetera). That is, solar activity "explains" in the statistical sense something like half of all past temperature change and there is no reason to think that	Rejected. Most of the comment refers to other parts of the report (chapter 5 and chapter 10 in particular). Only the PS refers to this chapter. Note that the Kirkby (2007) reference is still in the SOD.

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						the last century was any different. Solar activity was persistently high and the planet did a modest amount of warming. Now that the sun has gone quiet, warming seems to have stopped. The rough outlines fit well with a solar explanation. If this temperature history is half due to the sun, that cuts in half the amount of warming that could be attributable to CO2, diminishing the implied climate sensitivity accordingly, and in the absence of a hypersensitive climate there is absolute nothing to fear from CO2. Any modest amount of warming is good. The only actual danger would be runaway warming, driven by a high climate sensitivity, and any substantial solar-warming effect takes that possibility completely off the table. The real danger is that solar variation might well turn out to be a MORE powerful climate driver than the human contributions to atmospheric CO2, in which case our now quiescent sun portends a period of global cooling, which we know from the planet's history of glaciation really can feed on itself in runaway fashion and really is something to worry about. What has become clear since AR4 is 1) that there has been a cessation of warming (whether temporary or long term) and 2) the growing mountain of evidence for some mechanism of solar forcing far more powerful than the slight variation in TSI. AR5 recognizes that evidence, now it needs to also recognize the implications: we can have NO confidence that anthropogenic forcings are greater than solar forcing and hence no confidence that human activity is the primary driver in climate change. If the report is to be honest, this needs to be stated right in the first line of the Executive Summary. P.S. On page 7-43 of the SOD, lines 1-3, the Kirkby 2007 reference that had appeared at this point in the FOD should be put back in. It's good that you added the reference to Bond et al. 2001 in the SOD but that paper only provides one data point wheras Kirkby 2007 is a survey paper, reviewing much of the evidence for an enhanced solar driver of climate, including Bond 2001, so it is not a sensible switch. AR5 really ought to describe the evidence itself, offering readers a full sense of the strength and scope of the correlations that have been found, but failing that, at least include a citation that actually refers to a good portion of the evidence, instead of just a little bit of it. [Alec Rawls, United States of America]	
7-1278	7	43	1	43	27	The fundamental question of cosmic rays acting as nuclei needs further study. Studies in this direction are sparse and the basic results need to be attributed with caution because of their less reliability and large uncertainty. Hence, this issue needs to be established whether the influence of cosmic rays on climate change is significant. [Panuganti, C.S. Devara, India]	noted. This is the purpose of this section. The IPCC report cannot make recommendation as to what research topic needs further study.
7-1279	7	43	2			some old references could be replcaed by newer ones. E.g., Eichler, A., et al., 2009. Temperature response in the Altai region lags solar forcing, Geophys. Res. Lett., 36, L01808, doi:10.1029/2008GL035930. [Urs Baltensperger, Switzerland]	Accepted.
7-1280	7	43	4	43	4	"such as the hypothesized GCR-cloud link": This is too narrow. There can be other amplifying mechanisms, e.g., involving UV in the stratosphere and thereby an influence on planetary-wave breaking". [Jón Egill Kristjánsson, Norway]	Accepted. Text changed.
7-1281	7	43	5			Currently reads ‘between GCR and aerosols and cloud properties’, may be better to change an and for a comma, and include an additional serial comma: ‘between GCR, aerosols, and cloud properties.’ [Benjamin Laken, Spain]	Editorial. Accepted.
7-1282	7	43	17			“...diffuse fraction...” This term is incomplete. I think that what was meant was the “diffuse fraction of the total incident solar irradiance.” Whatever the diffuse fraction is should be spelled out. [James Coakley, United States of America]	Accepted with a slightly different wording.
7-1283	7	43	17			There is a sudden switch to the use of ‘cosmic rays’ in the text in place of GCR, consistency should be maintained. Actually, this point warrants some consideration, as the specific term ‘Galactic Cosmic rays’ (GCR) refers to cosmic ray particles of extra-solar origin, (non solar-cosmic rays), and is also somewhat distinct in meaning from (galactic) cosmic ray flux; the appendage of the word flux indicating a quantity over	accepted

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						time. While none of the uses are wrong, they are imprecise. It is best to be clear about what is specifically meant, and I would suggest that the term 'cosmic ray flux' is probably what is required most of the time: meaning, a rate of flow of high-energy ionizing particles of unspecified origin (including extra-galactic, extra-solar, and solar origins) impinging upon the Earth's atmosphere. Thus for instance, when in the text 'variations in the cosmic ray flux' are mentioned, it is less ambiguous than simply saying 'variations in the cosmic rays' e.g. which could imply changes in of some manner in the types of particles, the quantity of particles, or the energy level of the particles. [Benjamin Laken, Spain]	
7-1284	7	43	17			"Statistically significant, but weak, correlations…": What does this mean? What is the range of the "statistically significant" correlation coefficients? [Forrest Mims, United States of America]	Rejected. Weak=small but statistically-significant according to some statistical tests. There are many studies showing statistical correlations using different statistical techniques. We cannot easily provide a meaningful range. The quoted sentence is meant to be a concise assessment the literature.
7-1285	7	43	23	43	23	This is slightly inaccurate, because Kristjánsson et al. did not show global correlations, but rather correlations for the low latitudes of the Southern Hemisphere, and Laken & Calogovic studied selected geographical regions. Also, Kristjánsson et al. showed results for 22 Forbush events, not only the 5 strongest ones as Svensmark et al. did. [Jón Egill Kristjánsson, Norway]	Accepted. Text changed.
7-1286	7	43	27	43	27	An additional issue with cosmic-ray correlations is that the actual flux is influenced by atmospheric pressure (the atmosphere is a better shield when the pressure is high). Such day-to-day variations are larger than the 11-cycle. They introduce the possibility of aliasing in the known correlation of cloudiness with low-pressure systems. It depends on the way the correlations are performed. I don't know if you want to add this level of detail. [Daniel Murphy, United States of America]	Noted. But this is indeed not a level of detail the section should go into.
7-1287	7	43	36	43	38	Since it is a lab study, it should read "under middle and upper tropospheric conditions" rather than "in the middle and upper troposphere". [Bart Verheggen, Netherlands]	Accepted.
7-1288	7	43	37	43	37	As far as I can see CLOUD is the only facility or experiment and CERN is the only institution which is explicitly mentioned in the text of Chapter 7. Other institutions and major facilities or experiments could as well be mentioned, but it would be sufficient here to state the "Kirkby et al. (2012) have found that …". [Ottmar Möhler, Germany]	Accepted.
7-1289	7	43	38			.. but is very unlikely to give a significant contribution to nucleation taking place in the continental boundary layer (Kirkby et al., 2011). This statement is not correct: Kirkby et al. found that at temperatures typically found in the continental boundary layer, pure binary nucleation (of sulfuric acid and water) is not sufficient to explain the nucleation rates observed in the field. Adding ammonia enhances the nucleation rate but still yield values far belwo the one found in the field. This means that other species need to be added to reach the nucleation rates observed in the field, however, CLOUD has not yet published these data, and it s not yet known if with the addition of these additional species the same enhancement as for the binary nucleation occurs. [Urs Baltensperger, Switzerland]	Taken into account. Sentence has been rephrased.
7-1290	7	43	44	43	45	According to Merikanto (2009), 13 to 16% of cloud droplet number is due to nucleation, so stating that "big fraction of CCN in the global boundary layer is expected to originate from nucleation" is not consistent with the underlying article I find. Other studies have found similar fractions, see eg the (not peer reviewed) overview on RC: http://www.realclimate.org/index.php/archives/2009/04/aerosol-effects-and-climate-part-ii-the-role-of-nucleation-and-cosmic-rays/ where I refer to Merikanto (2009), Wang and Penner (2009), Spracklen (2008) and Pierce and Adams (2009), all arriving in the range of 5-20% of CCN or CDN deriving from aerosol nucleation, i.e. a relatively small fraction. Yu and Luo (2009) and other studies (also from Spracklen I believe) found a higher fraction however, so this is not a very well constrained quantity. The qualification "big fraction" is however not supported by the literature I think. [Bart Verheggen, Netherlands]	Taken into account. Sentence has been rephrased.
7-1291	7	43	45	43	45	replace "layer is" with "layer are" [Peter Irvine, Germany]	Editorial. Sentence has been changed.
7-1292	7	43	47	43	50	If the ion-aerosol clear air effect does not have a strong influence on the climate, it is not because GCRs cannot generate CCN or CDN, but because the climate-relevant CCN are not sensitive to a chance in the	This comment duplicates the next one. See below.

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						nucleation rate. If the nucleation rate were [Eimear Dunne, Finland]	
7-1293	7	43	50	43	53	If the ion-aerosol clear air effect does not have a strong influence on the climate, it is not because GCRs cannot generate CCN or CDN, but because climate-relevant CCN concentrations are not sensitive to a chance in the nucleation rate. If the nucleation rate were lower, or if the pre-existing aerosol concentration were lower, more ion-induced aerosol would survive to reach CCN sizes and affect the climate. I would suggest changing the sentence to read: "Although all model studies found a detectable connection between GCR variations and either CCN changes or column aerosol properties, the response appears to be too weak to cause a significant radiative effect because the aerosol system is insensitive to a small change in the nucleation rate in the presence of pre-existing aerosol (Pierce and Adams 2009, Kazil et al., 2012)." Pierce, J. R. and Adams, P. J. (2009), "Uncertainty in global CCN concentrations from uncertain aerosol nucleation and primary emission rates", Atmos. Chem. Phys. , Vol. 9, pp. 1339–1356. I am also not certain that it is necessary to state that all model studies found a connection between GCR changes and aerosol properties, since the models would contain explicit dependencies of the nucleation rate on GCRs, and so some response would be expected in any event. The question would be whether the response was larger than the natural variability of the system in the absence of a change in GCRs. [Eimear Dunne, Finland]	Accepted.
7-1294	7	43	50			.. Although all model studies found a detectable connection between GCR variations and either CCN changes or column aerosol properties: this statement is not fully correct: Pierce and Adams state: Globally for the MODGIL simulations, there is a 0.004% increase in CCN0.2% even though there is a factor of 4 increase in the new particle formation rate, and for the IONLIMIT simulations, there is a 0.08% increase in CCN0.2% from a 24% increase in the nucleation rate (Pierce, J. R., and P. J. Adams, 2009. Can cosmic rays affect cloud condensation nuclei by altering new particle formation rates?, Geophys. Res. Lett., 36, L09820, doi:10.1029/2009GL037946.I would not call 0.004% or 0.08% a detectable connection. [Urs Baltensperger, Switzerland]	Accepted. The sentence has been modified.
7-1295	7	43	52	43	53	I think this point should be made more exact: currently it is written 'the GCR are unable to effectively raise CCN and droplet concentrations', this is true, but may seem a bit counterintuitive to readers given the previous sentence which noted a detectable association between the GCR flux and CCN/aerosols. The point that should be made here, is that even though variations in the cosmic ray flux are able to influence aerosol nucleation and growth, it seems that they are not able to ultimately impact CCN concentrations to an extent that would significantly influence cloud properties. This is because ion-mediated nucleation and growth processes give a boost to the growth of small particles, however this boost does not translate to an increased number of larger (CCN-sized) particles, as pre-existing large particles effectively scavenge the newly generated particles before they can grow to sizes of relevance for clouds. I appreciate that attempting to include the specifics of this information would increase the word count to an unattractive extent, but perhaps a trimmed version of this point could be made, perhaps something to the effect of: 'Although all model studies found a detectable connection between GCR variations and enhanced aerosol nucleation and growth rates, the response was found to be too weak to cause a significant radiative effect, as newly generated particles were found to remain at sizes susceptible to loss by scavenging for relatively long periods, and consequently, are scavenged before they are able to grow and significantly alter CCN concentrations (Kazil et al. 2012).' [Benjamin Laken, Spain]	Accepted. The sentence has been modified.
7-1296	7	43	56	43	56	What physical quantity does "250 kV" refer to? [Jón Egill Kristjánsson, Norway]	Taken into account. This quantity is a frequently cited Ionospheric electrical potential difference, providing impetus to the fair weather atmospheric electric current, it is given as +250 Kilovolts (Harrison, 2004). This is maintained by electrified rain clouds and thunderstorms. We have simplified the sentence.
7-1297	7	44	1	44	5	The electric cloud microphysics progress in cloud boundary should be rewritten or revised. At the boundary of the cloud, due to the gradient of the concentration of the droplet and CCN particles, the charge will be redistribution according to the Gauss law which causes the gradient of conductivity and the droplet and particle will take net positive charge at top boundary and net negative charge at the bottom boundary which had been simulated by Zhou and Tinsley (2007, 2012) and measured by Beard (2004) and Harrisson and Nicoll (2011). And in this part it should be clear that the accumulated charge on the droplet or particle can	Rejected. The text already states that electrical effects "may influence droplet-droplet collisions ".

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						effect the collision process by electro-scavenging or anti-scavenging as suggested by Zhou and Tinsley (2009), Tinsley (2012). [Limin Zhou, China]	
7-1298	7	44	3	44	3	Replace "collision" by "collisions" [Jón Egill Kristjánsson, Norway]	Editorial. Corrected.
7-1299	7	44	9	44	9	This section is somewhat qualitative, probably necessarily so. But this final line on "climatic significance" leads to the question about how big a forcing needs to be to be significant. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Noted but no change is made. The reviewer is right, but this is a convenient way to conclude here given the lack of literature on the subject.
7-1300	7	44	9	44	9	"there is no evidence yet that associated cloud processes could be of climatic significant." If the author means the GEC effect mechanism impacting on cloud cover, it may be true. While the electro-scavenging and antiscavenging in cloud due to the GEC effect could impact on the winter cyclone system in north hemisphere, which could cause the block at the north Atlantic region and then change the circulation, which had been reviewed in detail by Tinsley (2012). This may be the climatic significant. [Limin Zhou, China]	Rejected. Our statement is not in disagreement with this comment. This may be of climatic significance but there is no evidence yet that it is.
7-1301	7	44	16	44	18	"The lack of trend in the cosmic ray intensity over the last 50 years…": This assertion is dated, for it is contradicted by the ongoing and historically significant reduction in solar activity. There was a significant (>5%) increase in the neutron count at Oulu during the extended solar minimum between cycles 23-24. The Oulu record began in 1964. Since the ongoing stabilization of global temperature has accompanied the reduced solar activity since about 2002, a new sentence is in order to express the fact that the significant reduction in solar activity now underway will likely increase the cosmic ray background. Please insert such language along with the reference in the following row: [Forrest Mims, United States of America]	Taken into account. This material is moved and discussed in the Solar Box in Chapter 10.
7-1302	7	44	16	44	18	Citation for proposed addition described in previous row: M. J. Owens, M. Lockwood, L. Barnard, and C. J. Davis. Solar cycle 24: Implications for energetic particles and long-term space climate change. GEOPHYSICAL RESEARCH LETTERS 38. L19106, doi:10.1029/2011GL049328, 2011. [Forrest Mims, United States of America]	Taken into account. This material is moved and discussed in the Solar Box in Chapter 10. This reference is not cited though as it partly relies on some extrapolation.
7-1303	7	44	16	44	18	The SOD wrongly claims that a persistent high level of temperature forcing cannot cause continued warming This argument had been used in FAQ 5.2 of the FOD as a grounds for dismissing a solar explanation for late 20th century warming. The Chapter 5 writing team seems to have responded to my criticism of this argument, which is no longer included in their FAQ. Unfortunately the writing team from Chapter 7 has now decided to include the argument that persistent high forcing cannot cause continued warming. In Chapter 7, it is the "7.4.5.3 synthesis" section that now makes the argument that temperature change is forced by the trend in the temperature forcing, not the level of the forcing (p. 7-44, lines 16-18): "The lack of trend in the cosmic ray intensity over the last 50 years (Agee et al., 2012; McCracken and Beer, 2007) provides another strong argument against the hypothesis of a major contribution of cosmic rays to ongoing climate change." Set aside for a moment the bogus insinuation that the planet is in the midst of an "ongoing climate change" in the warming direction, when the actual story of the 21st century has been a marked cessation of warming. Lines 16-18 are specifically in the context of hypothetical indirect solar forcings that might be much stronger than the slight variation in TSI (Total Solar Insolation). No matter the strength of the forcing, the authors are saying, a continued high level forcing should not cause continued warming. As I noted in my criticism of the FOD of Chapter 5 where the same claim was made, this conclusion obviously depends on unstated assumptions about ocean equilibration. If the oceans had equilibrated to the high level of post-1950s solar activity by say 1970 then yes, continued forcing at a similar level would only maintain that equilibrium and there would be no further solar-driven warming. On the other hand, if the oceans had NOT yet equilibrated to the high level of post-1950's solar activity by 1970 or 1980 then warming would continue. There is simply no grounds for assuming that ocean equilibration had to have been reached by any particular	Partly taken into account. It is indeed well known that the climate response lags behind the forcing, as is known from theory and climate models, including commitment experiments. It takes time for the climate system to equilibriate to a constant forcing. It is also well known that the climate system responds on different timescales, both short and long, as evidenced by observed climate response to volcanic forcings, model experiments to these volcanic forcings, climate response to abrupt CO2 forcing, climate response to ramp-up and down CO2 concentrations (eg Boucher et al., ERL, 2012) or SRM experiments (section 7.7). In particular most of the climate response is realised in a couple of decades. The argument laid out in the SOD is therefore valid, but for the clarity and consistency of the chapter, this discussion is moved to the Solar box in Chapter 10, where climate response to the solar forcing is discussed in more details.

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						20th century date. The upper ocean layer is known to warm quite rapidly after an increase in forcing, with a response time on the order of 4 to 7 years, but it will take much longer for this rapidly warmed-up upper ocean layer to transmit its warmth to the next deeper layer of ocean. As it does, the temperature differential between these layers will steadily decrease which in turn will decrease the rate of heat loss from the upper ocean layer to the next layer. That decrease in heat loss will cause the upper layer to warm on intermediate time scales, which will in turn cause continued warming of GMAST. The claim that continued high levels of forcing will not cause continued warming is simply WRONG, and hence cannot provide any grounds for dismissing a solar explanation for late 20th century warming. This argument must be removed from Chapter 7 the same way it was removed from Chapter 5. Getting back to the insinuation on lines 16-18 that 21st century temperatures have continued to warm, this should be replaced with a statement that recent temperature history is strongly at odds with the CO2 warming theory but fits quite well with the solar theory of late 20th century warming. As solar activity slowed down and then dropped off the cliff, temperatures stopped rising. The marked lack of warming since the 1998 El Nino makes the present claim about "ongoing climate change" in the warming direction particularly egregious. The SOD takes a piece of evidence that militates strongly in favor of the theory of enhanced solar forcing (the cessation of warming), and pretends that it supports the CO2-warming theory, when it actually undermines the CO2-warming theory. This is astounding dishonesty. Please take it out. [Alec Rawls, United States of America]	
7-1304	7	44	16	44	18	Claims that persistent high levels of forcing cannot cause continued warming must be judged highly suspect until backed up by GCM tests Chapter 7 of the SOD acknowledges strong evidence for a solar driver of temperature more powerful than can be accounted for by the slight variation in TSI (p. 7-43, lines 1-4): "Many empirical relationships have been reported between GCR or cosmogenic isotope archives and some aspects of the climate system (e.g., Bond et al., 2001; Dengel et al., 2009; Ram and Stolz, 1999). The forcing from changes in total solar irradiance alone does not seem to account for these observations, implying the existence of an amplifying mechanism such as the hypothesized GCR-cloud link." This raises the question of whether late 20th century warming might have been caused by the high level of solar activity between 1950 and 2000 rather than by human increases in atmospheric CO2. In each place where the SOD addresses this question it makes the highly unscientific claim that late 20th century warming cannot have been caused by the sun because solar activity was not rising over this period. For instance, in Chapter 10 on attribution, page 10-18, lines 3-5, directly states that solar-driven temperature change should be driven by the trend in solar activity, not the level of solar activity: "... several studies show that solar variations cannot explain warming over the past 25 years, since solar irradiance has declined over this period (Lockwood and Fröhlich, 2007, 2008; Lockwood, 2008(Lockwood, 2012))." Lockwood claims that the smoothed level of solar activity started turning down at about the end of solar cycle 21, in the mid 80s. By most measures solar cycle 22 was stronger than cycle 21, so I would put the turn down ten years later, but set that aside. The point here is that what Lockwood thinks, and what the SOD here repeats, is that temperature is driven by the trend in the forcing, not the level of the forcing. Chapter 7 says the same thing (p. 7-44, lines 16-18): "The lack of trend in the cosmic ray intensity over the last 50 years (Agee et al., 2012; McCracken and Beer, 2007) provides another strong argument against the hypothesis of a major contribution of cosmic rays to	This comment repeats a lot of the arguments made in #1303. See reply to comment #1303. Other arguments relate to issues which are not relevant to this chapter.

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						ongoing climate change." The same argument is also made less explicitly in FAQ 5.1 (p. 5-50, lines 32-33). Do the authors really think that you can't heat a pot of water by turning the flame to maximum and leaving it there? Solar activity was at high levels (strong solar cycles) over the entire period in question. According to Usoskin 2007 the 20th century experienced a "grand maximum" of solar activity that continued through about the end of century. Thus regardless of where the exact peak was, the entire second half of the century maintained average levels of solar activity that can be described as somewhere between high (Muscheler 2007) and very high (Usoskin). The claim that these persistent high levels of solar forcing cannot caused continued warming is highly counterintuitive and must rely on some unstated assumptions. The authors of these lines are probably assuming that the oceans had equilibrated to high post-50s warming by 1970 or so. Then yes, continued high solar forcing would be necessary just to maintain that equilibrium temperature. But any assumption that the oceans must have equilibrated by ANY 20th century date is highly speculative. That makes it a highly UNCERTAIN grounds for dismissing a solar explanation of late 20th century warming, not the "strong argument" that the SOD repeatedly asserts and implies. Also, such unstated assumptions obviously need to be made explicit and, most importantly, they need to be tested. Do the GCM test-runs In particular, the repeated claims that persistent high levels of solar forcing (beyond what can be accounted for by TSI) would not cause continued warming can and should be tested by GCM model runs. It seems clear that GCM tests of models with enhanced solar forcing effects have NOT yet been run. Otherwise these tests would be cited along with the claims that high post-50s solar activity could not have caused post-70s warming, but no such citations are listed at any of these points. Running the tests should be straightforward. The most likely avenue of enhanced solar forcing is some effect on cloud formation, whether through Svensmark's GCR-cloud mechanism or through the effects of UV-shift on atmospheric circulation, or through the earth's electrical circuit. Svensmark and Friis-Christensen (1997) suggest about a 2% variation in low clouds as solar activity varies, which would be a simple addition to existing GCMS. Climate sensitivity would have to be lowered as necessary to get the best fit to observed temperatures, and other parameters could be adjusted as well. Numerous parameters have been tweaked to bring the CO2-driven models (models where the only solar forcing is TSI) into their best fit with past climate data and the same would ideally be done for models with enhanced solar forcing, but conceptually this would be nothing new. The outcome is easy to predict. It is simple logic that a GCM test of continued high solar forcing WOULD show continued warming. You already know this from your commitment studies. You can also deduce it from the working of a simple 3-box model with an upper ocean layer, an intermediate layer and a deep layer. The upper ocean layer is known to warm quite rapidly after an increase in forcing, with a response time on the order of 4 to 7 years, but it will take much longer for this warmed-up upper ocean layer to warm up the intermediate layer of ocean below. As this slower warming takes place, he temperature differential between the upper and intermediate layers will steadily decrease which in turn will decrease the rate of heat loss from the upper ocean layer to the intermediate layer. That decrease in heat loss will cause the upper layer to continue to warm on an intermediate time scale, which will in turn cause continued warming of GMAST. The claim that continued high levels of post 50's solar forcing would not cause continued warming is clearly wrong. The best thing would be to simply remove it, but at the very least you should note that this is a speculative argument that at present is awaiting verification by GCM test runs. Of course it is embarrassing that these tests have not already been run. Along with internal variability, solar warming is THE alternative hypothesis to the favored CO2-warming theory. Given over $100b in public funding	

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						so far, "consensus" scientists really should have bothered by now to test the alternative hypothesis, but it is too late to rectify this failure in AR5. The only thing to do now is admit the omission and acknowledge that until the tests are run the proposed grounds for dismissing a solar explanation for late 20th century warming must be considered very uncertain. GCM test-runs are the ONLY evidence for the CO2-warming theory For some perspective on how big a role the GCM tests are playing, and how big a problem it is that the alternate hypothesis has not been GCM tested, note that the DIRECT evidence for the CO2-warming theory is virtually non-existent, while the paleo evidence for a powerful solar driver of climate is overwhelming. If traditional forms of evidence were used as a guide for which hypotheses received the attention of our new computational modeling tools then the primary object of study would be the enhanced-solar hypothesis. Instead, the only hypothesis that gets modeled is the one for which traditional forms of evidence are notably lacking. About that evidence, the paleo archives show a strong correlation between atmospheric CO2 and temperature, but with CO2 following temperature by an average of about 800 years (Caillon 2003), indicating that temperature drives atmospheric CO2 (as in theory it should, since warmer oceans hold less CO2). The reverse COULD also be taking place. CO2 could also be driving temperature, but thanks to the causality going in the other direction no such effect can be separated out from the paleo data. We do have evidence for a mechanism by which CO2 should cause some warming. A doubling of CO2 should have a modest temperature forcing effect, somewhere on the order of 1°C, but that forcing effect could either be amplified or dampened by feedback effects. The forcing does not in itself tell us whether CO2 explains much of recent warming, and there is no indication in the paleo records that CO2 is doing ANYTHING. In contrast, there is a veritable mountain of evidence in the paleo records for a powerful solar driver of climate, far more powerful than can begin to be accounted by the small variation in TSI. In my FOD comments I cited 2 dozen papers that have found something between a .4 and .7 degree of correlation between solar activity and various climate indices. That is, solar activity "explains" in the statistical sense something like half of all past temperature change. CO2 is invisible in the paleo data while solar activity screams out like a neon sign. So here is what it looks like is going on with the GCM testing. In the absence of direct evidence that CO2 is a powerful driver of climate the CO2-warming "consensus" has turned to this new and different kind of evidence: model-fitting, where the fact that CO2-driven GCMs are able to produce a not-so-bad fit to the last 150 years of observed climate data is presented as an affirmation of the CO2-warming theory. In actuality the evidence here is only negative. The models only demonstrate that CO2 can't be RULED OUT as the primary driver of recent temperature history: that there is a way to concoct a plausible scenario of how CO2 could be responsible. That is useful information, well worth collecting. What is perverse is to selectively apply this new kind of test only to the hypothesis that is NOT supported by traditional evidence. When it comes to the enhanced-solar hypothesis, this theory IS declared to be ruled out, and on the most unscientific grounds imaginable (continued high levels of forcing can't cause warming!), without ever being allowed access to the test that is used to say that the CO2 hypothesis can't be ruled out. If GCM data-fitting is a new kind of test, offering a new kind of evidence, then obviously it can't be applied selectively as a way of boost to one theory while hiding what it says about competing theories, yet this is exactly what is being done. The GCM tests are being used to provide what is presented as a strong new kind of evidence (supercomputers!) when it is actually weak evidence (negative evidence), and this inflated evidence is presented only for the favored CO2 hypothesis. We have this powerful new tool available and it is immediately subverted by this complete misuse. Instead of enhancing our ability to discern and follow evidence it is being used as a ruse to evade the evidence.	

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						It's time to start applying the GCM tool in the one place where traditional evidence says it most needs to be applied: to the solar-warming theory of late 20th century warming. It is time to start testing the alternate hypothesis, and AR5 should say so. Solar-warming GCM tests will blow CO2-warming GCM tests out of the water That's a guarantee. Both theories have trouble with mid-20th century dip in temperature. The cold dragon of the vasty deeps seems to have flicked its tail (part of the internal variability that could well turn out to be the biggest player of all). Both theories also do fine with post-WWII warming as a whole (solar activity and CO2 both attained historic highs over this period). But everywhere else the solar-theory fits better. It's a better fit for pre-WWII warming where the increase in CO2 forcing was still relatively minor but there was a substantial ramp up in solar activity from the turn of the century solar lull to the onset of the grand maximum in the 20s and 30s. Then there is the lack of warming over the last 15 years. The CO2 theory says that warming should have accelerated over this period. To be maintained it needs to again invoke a major episode of internal variability, while for the solar-warming theory the end to recent warming is just what one would expect now that the sun has dropped into a quiescent phase. If solar activity and CO2 have similarly sized forcing effects then warming should merely stop. If solar activity is the stronger effect then temperatures should next begin to fall. Allow a solar-driven GCM to be tweaked for fit the way the CO2 driven GCMs are and the solar theory would win in a walkover. Is this what is keeping "consensus" scientists from running GCM tests of the alternate hypothesis? Is it because they know that their CO2 theory will be routed? It is hard not be suspicious when "consensus" scientists are not only refusing to run enhanced-solar GCM tests but are at the same time making obviously wrong statements about what these tests would show if they WERE run. Continued high levels of enhanced solar forcing won't cause continued warming? Wanna bet? Maybe it has to be written into a bill. Maybe Senator Inhofe will have to make continued climate science funding contingent on the alternate hypothesis finally being included in the GCM modeling. But come on, some of you IPCC authors have to be real scientists. I'm sure you understand that it is wrong (bad science) to take this new kind of test and only apply it to what is not otherwise evidenced. So say it. Just note this curious omission—that the alternate hypothesis is still waiting to be GCM tested—and acknowledge that the proposed grounds for dismissing a solar explanation for late 20th century warming must be considered suspect until this oversight is rectified. [Alec Rawls, United States of America]	
7-1305	7	44	20			Section 7.5: Discuss in this section (or in Chapter 8) emission-based aerosol forcing versus concentration-based forcing. In the context of models driven by emissions, it is somewhat ambiguous how an emission-based forcing as used here should be defined. (For instance, how should NOx emission-induced perturbations to sulfate be accounted for?) Figure 8.17c and Table 8.SM.2 touch on this issue. [Larry Horowitz, United States of America]	Taken into account. Chapter 8 discusses this. We only consider concentration based metrics. We now state this in the introduction.
7-1306	7	44	22	44	43	My understanding in AR4 was that Rfaci only included the Twomey effect. Is that also true in AR5? It would be better to dispense with the RF concept if that is the case, since it has been known for a decade that it is unphysical to only include the Twomey effect when considering ACI. I'm not sure what the RF concept really adds to understanding for aerosol-cloud interactions. There are now so many metrics to choose from that I fear major confusion. The approach is inconsistent with the treatment of GHGs for which a forcing and a feedback are assessed in the more conventional manner rather than via the RF/AF approach. Also, why is there no calculation of Afari? [Robert Wood, United States of America]	Yes, Rfaci only includes the Twomey effect. We agree that RFaci is a construct that does not make sense and therefore obstain from providing an estimate for it. Note, that following the discussion at LA4, AF will be changed to ERF (effective radiative forcing). Yes, Rfaci only includes the Twomey effect. We agree that RFaci is a construct that does not make sense and therefore obstain from providing an estimate for it. Note, that following the discussion at LA4, AF will be changed to ERF (effective radiative forcing). Afari (now ERFari) is assessed but seperatey as an RFari plus a rapid adjustment. GHG forcings are assessed

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							with the same framework in 7.2
7-1307	7	44	23	44	23	RF includes stratospheric temperature adjustment. I think it is better that correct definition is used and state that for RF ari and aci the stratospheric temperature adjusment is of minor importance so these are quantified in terms of instantanous RF. [Gunnar Myhre, Norway]	agreed, text reworded
7-1308	7	44	24	44	25	According to Figure 7.2 RFaci was formally known as the cloud albedo effect [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	yes, that's correct, we added that.
7-1309	7	44	26	44	26	I know what you mean by "Hansen style experiments" but it is a bit vague and it is not the only framework for assessing the AF - isnt the Gregory method as widely used?? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	The Gregory method hasn't been used for RF/Afaci studies. However, we are now more explit and adopt a different term.
7-1310	7	44	26			The reference to "Hansen-style experiments" is subsequently explained as having fixed sea-surface temperatures and presumably refers to James Hansen - but this seems like an odd way of describing a technique. If there is a desire to reference Hansen - than an actual reference should be used. [Government of United States of America]	"Hansen style" has been removed
7-1311	7	44	28	44	29	The sentence "AF can be caused by afi, ari or the sum of afi and ari" is confusing. Surely AF is caused by the combination of afi and ari, both of which contribute but may not sum linearly [Robert Wood, United States of America]	good point, we need to make an assumption about linearity of ERFaci+ERFari = ERFari+aci but we point out the limitations of this assumption. The sentence has been deleted
7-1312	7	44	31	44	39	The second paragraph in this section starts with a reference to Chapter 2 in AR4. Later in this paragraph (line 35) there is a reference to chapter 7 that seems to be still referring to AR4 but it's not clear whether it is or not. Then at the beginning of the next paragraph there is a reference to chapter 8. It sounds as though the reference is to the fifth assessment but taken with these other references, it's not clear. This needs to be clarified. [Government of United States of America]	yes chapter 7 refers to AR4 and chapter 8 to AR5, we added this.
7-1313	7	44	35	44	35	Does "remote" relate to the location where the measurement is made, or the fact that it is a remotely-sensed measurement? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	remotely sensed is now adopted
7-1314	7	44	40	44	40	What is the reason for choosing 1750 as a reference period? Is this chosen as the 'pre-industrial' time? Do we have observations going back that far - if so, what type of observations are they and if not how are such values obtained? For some aerosols is this assumed to be the zero period or is there some agreed upon background level? There appears to be little consensus about what is truly 'pre-industrial' throughout the document with the observations chapters mostly only going back to the late 1800s due to observational constraints. [European Union]	1750 is the standard date used in chapter 8 and earlier forcing chapters as past reports. There was considerable forcing before the obs record. This is an important point, especially for BC that had anthropogenic effects before 1750, so details are now added
7-1315	7	44	42	44	43	Further description of how this conversion is done needs to be provided in this chapter. [Larry Horowitz, United States of America]	this conversion is not done within the chapter but in the published referenced literature. Text now changed to be clearer
7-1316	7	44	53	44	57	This may be a useful place to restate that even perfect measurements of aerosol radiation interactions determine the radiative effect, not the anthropogenic forcing. A crucial point that is obvious to those working in the field but not always obvious to outside readers. [Daniel Murphy, United States of America]	agreed, statement now added
7-1317	7	44	53	45	24	This text seems to be the kernel of the global estimate of aerosol direct radiative forcing. It adduces model estimates and observations to conclude that the forcing is 0.4 W m-2 with 5-95% uncertainty ± 0.3 W m-2. This is a smaller (magnitude) estimate and smaller uncertainty range than previous estimates. The estimate rests on models and satellite observations. From a model perspective, one might call atention to figure 9.29, which shows a spread of about 0.09 in AOD among the models. For a sensitivity of TOA forcing to AOD of roughly 30 W m-2 per tau (24 hr avg, equinox, low and mid latitudes) in cloud free conditions (McComiskey et al 08); derate by a factor of 2 for cloud cover to get 15 W m-2 per tau. So the error in tau of ~0.1 indicated in the figure corresponds to 1.5 W m-2. This is substantial in terms of forcing over the industrial period and much greater than the uncertainty in forcing given in this chapter. Surely some discussion is required to justify the much lower uncertainty given. Although the	WE agree that the uncertainty estimate is not justified enough in the current text. The uncertanity estimate is now expanded on. This leads to an expanded uncertanity range. The CCSP 2009 reference is not used as it is deemed grey literature and may not have been subject to full peer review.

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						multi-model mean forcing in figure 7.17 suggests a rather low forcing, the spread of the models about that multi model mean is fairly large, again raising question of the rather low 5-95% uncertainty attached to the aerosol direct forcing. With respect to satellite observations the following exerpt from Kahn, 2012, explicitly addresses measurement accuracy and provides important context (Note that Kahn refers to instantaneous cloud-free forcing; derate by factor of 4 for 24-hr average and 50% cloud cover): Calculations suggest that instantaneous, mid-visible AOD measurement accuracy of about 0.02 is typically required under cloud-free conditions to constrain DARF to approximately 1 Wm-2 (McComiskey et al. 2008; CCSP 2009), whereas the corresponding uncertainties in the current global AOD products from MISR and MODIS are 0.03 or larger over dark water, and 0.05 or larger over land (Kahn et al. 2010; Levy et al. 2010; Remer et al. 2005). Theoretical DARF sensitivity analysis identified particle single-scattering albedo (SSA) as the other leading factor in most situations, especially important for determining radiative forcing over land surfaces, and requiring an instantaneous constraint of about 0.02, though varying with other factors, particularly AOD and surface albedo (McComiskey et al. 2008). These considerations would suggest revisiting the asserted uncertainty in direct aerosol rad forcing.Taking the factor of 4, a measurement accuracy in AOD of 0.08 corresponds to 1 W m-2. If the accuracy of satellite measurement is 0.04, that is 0.5 W m-2 in DARF. Then there is the further issue of attribution to anthropogenic to get forcing. The draft cites Loeb and Su, but seems only to give lip service to that paper, which finds that the uncertainty associated with observationally determined DARF is 0.5 to 1 W m-2. CONTINUED ... [Stephen E Schwartz, United States of America]	
7-1318	7	44	53	45	24	CONTINUED ... The draft does not cite the thorough examination of uncertainty in DARF given in the recent U.S. Climate Change Science Program (CCSP) assessment of aerosols (Remer et al. 2009), the satellite-based cloud-free anthropogenic direct radiative forcing at the TOA is estimated to be -1.1 ± 0.4 W m-2 over the global ocean, where the uncertainty corresponds to one standard deviation, i.e., 0.7 W m-2 (1.64 sd corresponding to 5-95% of gaussian pdf). So it seems that these considerations regarding both model spread and satellite uncertainty need to be more explicitly addressed before confidence can be placed in the estimate of uncertainty in aerosol direct rad forcing given here. [Stephen E Schwartz, United States of America]	see above.
7-1319	7	44	53	45	24	References Kahn RA. (2012) Reducing the Uncertainties in Direct Aerosol Radiative Forcing. Surveys in Geophysics 33:3-4, 701-721. DOI 10.1007/s10712-011-9153-z Kahn RA, Gaitley BJ, Garay MJ, Diner DJ, Eck T, Smirnov A, Holben BN (2010) Multiangle Imaging SpectroRadiometer global aerosol product assessment by comparison with the Aerosol Robotic Network. J Geophys Res 115:D23209. doi:10.1029/2010JD014601 Levy RC, Remer LA, Kleidman RG, Mattoo S, Ichoku C, Kahn R, Eck TF (2010) Global evaluation of the Collection 5 MODIS dark-target aerosol products over land. Atmos Chem Phys 10:10399–10420. doi:10.5194/acp-10-10399-2010 Remer, L. A., and Coauthors, 2009: Executive summary. Atmospheric Aerosol Properties and Climate Impacts—A Report by the U.S. Climate Change Science Program and the Subcommittee on Global Change	These references are cited in the new uncertainty discussion, apart from the CCSP report which is deemed grey literature

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						Research, M. Chin, R. A. Kahn, and S. E. Schwartz, Eds., NASA, 1–8. Remer LA, Kaufman YJ, Tanre D, Mattoo S, Chu DA, Martins JV, Li R–R, Ichoku C, Levy RC, Kleidman RG, Eck TF, Vermote E, Holben BN (2005) The MODIS aerosol algorithm, products, and validation. J Atmos Sci 62:947–973 McComiskey, A., S. E. Schwartz, B. Schmid, H. Guan, E. R. Lewis, P. Ricchiazzi, and J. A. Ogren (2008), Direct aerosol forcing: Calculation from observables and sensitivities to inputs, J. Geophys. Res., 113, D09202, doi:10.1029/2007JD009170 [Stephen E Schwartz, United States of America]	
7-1320	7	44	56	44	56	Study of Zhao et al. (2008) over global ocean has been updated and extended to over global land in Zhao et al. (2011). Suggesting adding the new reference Zhao et al. (2011) here. New Reference: Zhao, T. X.-P., N. G. Loeb, I. Laszlo, and M. Zhou, Global Component Aerosol Direct Radiative Effect at the Top of Atmosphere, Int. J. Rem. Sens., 32:3, 633-655, 2011. [Xuepeng (Tom) Zhao, United States of America]	agreed. Reference added. It doesn't give a reasonable RF estimate, but it does add useful insights into DRE over land
7-1321	7	44				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, United States of America]	It does, via T changes. These are clearly in the arrows. What is the mechanism for non-T changes?
7-1322	7	44				Why is there only emphasis on global radiative forcings? What about regional forcings? This issue seems to have been completely neglected and yet the most important impacts of aerosols will likely be at the regional scale. [Robert Wood, United States of America]	Regional forcings do not necessarily imply regional climate effects. However, regional forcings are discussed in chapter 8. This is now alluded to in the introduction
7-1323	7	45	2			Regarding the best estimate of RFari of -0.3±0.2 W m-2. "There is medium confidence in this best estimate, but the uncertainty range is likely underestimated (Loeb and Su, 2010)." Loeb and Su did radiative transfer calculations in clear and cloudy skies and determined an uncertainty in direct radiative forcing by varying individual aerosol optical properties. The most important parameter was single scatter albedo, so the total error obtained by adding components (presumably in quadrature) is similar to the error in SSA. For the SSA uncertainty, Loeb and Su cite Dubovik et al, JGR, 2000 which is a careful accounting of the systematic errors of retrieval from the Aeronet network. From Loeb and Su, "the single scattering albedo is perturbed by ±0.06 over ocean and ±0.03 over land. The single-scattering albedo perturbations are based upon Table 4 in Dubovik et al. (2000), where the uncertainty for aerosol optical depths < 0.2 is between 0.05 and 0.07, and that for aerosol optical depths > 0.2 is 0.03. As SSA is an intensive property it is reasonable that an instrumental error would be greater at low optical depth. But is it reasonable that the SSA of aerosol at low optical depth to above average optical depth has a real range that is ±0.03 SSA units larger than that from polluted air. I would expect that the lowest SSA's (most absorbing aerosol) occur in high pollution regions. A change in SSA of 0.06 is substantial and potentially ruled out by other observations, a point that Loeb and Su refute. The authors might consider revising the text to reflect this science. [Government of United States of America]	Agreed. Uncertainty aspect now considerably expanded
7-1324	7	45	2			The statement hardly gives justice to the work of Loeb and Su. [Stephen E Schwartz, United States of America]	supplementray metrial now expands on these aspects to reflect this understanding. Section 7.3.4 also refered to and text expanded
7-1325	7	45	4	40	10	This paragraph includes mixed studies. It is not clear if the mean of Lohmann et al. is independent of Myhre et al.. Presumably this includes some of the same models? If so, is this accounted for in the analysis? [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Agreed. Lohmann sentence and estimate now dropped
7-1326	7	45	7	45	8	reference of figures out of order. Missing ref to Fig.7.18 [Andrea Flossmann, France]	references to Figs now corrected
7-1327	7	45	7	45	8	About the sentence:"Figure 7.17 shows the zonal mean total RFari for AeroCom phase II models and the 1750–2010 RFari from all models are shown in Figure 7.19." Please verify if this last one is Figure 7.19 or	references to Figs now corrected

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						Figure 7.18. [Rubén D Piacentini, Argentina]	
7-1328	7	45	7		10	Regarding agreement between models. Compared to Fig. 7.17, model predictions were more tightly grouped when predicting total AOD (Fig 9.29), which is potentially a harder problem. Excluding three outliers the range was 0.09 – 0.16. It helps to have a global constraint. [Government of United States of America]	CMIP5 models in chapter 9 are not really quality estimates of Rfari so are not used here. Text on uncertainty is expanded to address comment, specifically talking about aod
7-1329	7	45	8	45	8	figure 7.19 is referred to before figure 7.18 [Peter Irvine, Germany]	references to Figs now corrected
7-1330	7	45	8	45	8	Seems as this is Figure 7.18 [Gunnar Myhre, Norway]	references to Figs now corrected
7-1331	7	45	15			An RFari of -0.4 ± 0.3 W m-2 is arrived using the methodology of Loeb and Su (2010) Loeb and Su (2010) presented a broader uncertainty range that included positive numbers. The value and uncertainty of RFari are important results. From the written text, it appears as though the authors of this chapter combined results in the literature. As SSA was the prime driver of uncertainty in Loeb and Su's result, it would be helpful to know what value it was assigned in the current assessment. [Government of United States of America]	text now expands on these aspects to reflect this understanding. Only best estimate is given here. Uncertanity discussion expanded later in the text
7-1332	7	45	18	45	19	The Randles paper does not seem to have been submitted to Atmos. Chem. Physics (at least I can not find it on the discussion site). [Robert Pincus, United States of America]	Randles paper now published
7-1333	7	45	18	45	19	The Stier paper does not describe uncertainties in radiative transfer, which are quite small (doi:10.1029/2005JD006713) but rather differences in the formulation of the problem, including the treatment of absorption by gases. Perhaps this could be revised to "Uncertainties and parameterization errors in radiative transfer..." [Robert Pincus, United States of America]	text now edited to more fully reflect the Steir et al. results
7-1334	7	45	18	45	19	I was missing some more details on other host model effects in this section. The cited Randles paper does indeed look into radiative transfer, however, Stier et al. show that in particular the representation of clouds and surface albedos has a significant effect on the simulated forcing diversity, generally, but also in the used Myhre et al. results. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	details on Steir paper added, Randles paper also cited
7-1335	7	45	20	45	20	Samset et al. (2012) was rejected. [Steven Ghan, United States of America]	citation to Samset dropped
7-1336	7	45	20			The Samset et al. 2012 reference does not seem to be available and so it can not be determined the extent to which this study supports this statement. [Government of United States of America]	citation to Samset dropped
7-1337	7	45	27	45	29	It could be added that this figure is for 1850-2000. [Gunnar Myhre, Norway]	agreed, caption now changed
7-1338	7	45	31			AFari = semi direct effects? [Andrew Gettelman, United States of America]	yes, box 7.1 now refered to
7-1339	7	45	33	45	35	"There is high confidence that the local heating caused by absorbing aerosols can cause cloudiness to increase or decrease depending on their conditions." This is a weak statement. Do you mean to say that there is high confidence that local heating by absorbing aerosols has an influence on cloudiness (i.e., is not negligble), but the magnitude is depends upon (specify the factors)... [Norman Loeb, United States of America]	yes, text now reworded
7-1340	7	45	48			It is not clear how the estimate of -0.1 W/m^2 is obtained for the rapid adjustment.With so many presentations of representative values and associated uncertainties in this chapter, it is very important that the authors deal with these estimates in a consistent way. [Government of United States of America]	text expanded to explictiy state where it comes from - Ghan et al.
7-1341	7	45	51	46	47	Ocko et al. (2012) showed that the RFari of individual aerosol species do not add up to the total forcing due to the nonlinearity caused by internal mixing. So, I question the utility of breaking down RFair by species. At least, this issue should be acknowledged by citing the paper. The reference is Ocko, I. B., V. Ramaswamy, P. Ginoux, Y. Ming, and L. W. Horowitz (2012), Sensitivity of scattering and absorbing aerosol direct radiative forcing to physical climate factors, J. Geophys. Res., 117, D20203, doi:10.1029/2012JD018019. [Yi Ming, United States of America]	RF by species is still a useful concept. However, reference now added.
7-1342	7	45	51	47	32	Section 7.5.1.2 and 7.5.1.3 : I found these two sections difficult to read. Maybe because of the number of acronyms and numbers. I don't have any specific suggestion for improvement..except separating the text of	we have split section 7.5.1.3 into separate pararaphs and done some rewording

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						section 7.5.1.3 into several paragraphs instead of a single one. [Sandrine BONY, France]	
7-1343	7	45	51			Section 7.5.1.2: Explain what is meant here by "RF by species" in the context of internal aerosol mixtures. For an internally mixed aerosol, how is the net forcing partitioned among the component species? [Larry Horowitz, United States of America]	each group has a different method - citation and text added
7-1344	7	45	54	45	54	and biofuel' should be added after 'BC fossil fuel' [Gunnar Myhre, Norway]	changed
7-1345	7	45	55	45	55	OA is used elsewhere instead of OC [Gunnar Myhre, Norway]	OA now used
7-1346	7	45	56	46	30	see also Fig. 7.18. The forcing for SOA appears low. It should be mentioned that only few models calculated RFari for SOA in Myhre et l. (2012) [Urs Baltensperger, Switzerland]	figure corrected
7-1347	7	45				I recommend revising the droplet statement. There is fundamental physical understanding that black carbon absorption is enhanced by inclusion in water droplets (See Mark Jacobson's work). What is uncertain is the magnitude of the effect, not the existence of the effect. [David Fahey, United States of America]	we agree text now revised
7-1348	7	46	9	46	25	Recommend citing the recent work by Cappa et al (2012), which suggests that internally mixed BC does not exhibit a substantial absorption enhancement implying the radiative forcing impacts of BC may be overestimated (particularly from fossil fuel emissions). [William Landuyt, United States of America]	now cited
7-1349	7	46	9	46	25	While the section cites a paper suggesting the BC radiative forcing estimate of Ramanathan and Carmichael (2008) is likely high, I recommend citing the radiative forcing value suggested in this paper as well as the value cited in Sato et al (2003) and identify the potential mismatch between observational and model estimates of BC radiative forcing. The discussion in this section seems to give an impression that the RF for BC is much better understood than the totality of the literature would suggest. [William Landuyt, United States of America]	BC text now revised the Bond et al. estimates now incorporates this discussion
7-1350	7	46	9	46	25	Recommend citing the work by Ban-Weiss et al (2011) on the impact of the vertical distribution of black carbon on radiative forcing to the discussion and estimates of its value. [William Landuyt, United States of America]	other uncertainties are more important - text now heaviliy revised
7-1351	7	46	9	46	33	These two paragraphs are confusing. BC RF is from fossil fuel and biofuel sources so that is anthropogenic. What category contains POA and SOA from these sources? Open burning sources are attached to the BB aerosol which includes organics. Are the open burning sources, including crop management, land clearing, and trash burning solely anthropogenic? The Title heading for Section 7.5 would suggest they are. What category contains changes in biogenic emissions due to changes in land use? The aerosol community is pursuing the possibility that much of the biogenic aerosol is formed as a result of interactions between anthropogenic and biogenic compounds, which is part of Section 7.3.2.1. There does not appear to be a link between 7.3.2.1 and 7.5.1.2 that reflects the possibility that much of what is called natural organic aerosol might be dependent on anthropogenic emissions. Radiative forcing, called natural, could be anthropogenically controlled. [Government of United States of America]	agreed text was confusing. Paragraphs now reworded. WE are not able to make a forcing assessment of this natural aerosol effect so do not reference it here.
7-1352	7	46	9	46	33	I wonder if a paragraph is needed (eventually earlier in the chapter) on the problems to properly define BC forcing (definition of BC, carbonaceous aerosol, measurement and interpretation of absorption, co-emissions, pre-industrial background, separation from dust, internal mixing) [Michael Schulz, Norway]	addressed in section 7.3, paragraph now reworded
7-1353	7	46	9			This black carbon text is very unsatisfying for the reader. Repeat of earlier comment on TS-18 ln 27: When readers consult AR5 to learn what the forcing of black carbon is in the global atmosphere they will be disappointed and/or confused, starting with these two sentences that offer 2 equations and 3 unknowns. Further, black carbon from biomass burning is erased from the accounting here (Chapter 7) apparently because its RF is 'cancelled' by co-emissions of organic carbon. This is highly misleading. The revised Bond et al. 2012 now cites black carbon as the 2nd largest anthropogenic forcing term with approx equal contributions from the 3 sources cited above (I am a coauthor). This section should posit that black carbon	BC text completely revised. Bond et al. now refered to and Chapter 8 has these other emissions

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						has 3 principal source terms that need to be evaluated (along with pre-industrial emissions) to understand its role in the atmosphere and ultimately its forcing contribution, and then discuss what we know about these terms separately and combined. [David Fahey, United States of America]	
7-1354	7	46	9			I strongly recommend including more of the results of the Bond et al 2013 black carbon assessment (I am a coauthor). It is the most comprehensive and quantitative assessment of black carbon climate forcing terms. When a reader considers Bond et al Figure 9.1 and then Figure 8.17 (and Figure TS.5) and Table 7.1 and the accompanying text, there will be confusion. AR5 black carbon treatment appears inconsistent and incomplete, esp in comparison to Bond et al. which provides a stronger basis for its conclusions. I recommend that the AR5 authors more fully adopt the Bond et al results and reprint its Figure 9.1 or simpler version (I will make a customized graph for AR5 if requested). In addition to the direct climate forcing, these results allow AR5 to address black carbon indirect effects and the important role of co-emitted species. Of course, including the Bond et al results depends on the manuscript being accepted by JGR by the AR5 deadline. [David Fahey, United States of America]	text changed and Bond et al. more carefully refered to. We only condier the direct effect of fossil fuel plus biofuel here
7-1355	7	46	10	46	10	Line 10: Has “BB” been defined previously? [Kuo-Nan Liou, U.S.A.]	acronym no longer used
7-1356	7	46	13	46	13	Suggest: "… the inability of AERONET to measure aerosol properties at low optical depths…" because AERONET is about the best measurement we have of AOD itself in most situations. [Ralph Kahn, United States of America]	text now reworded, sentence dropped
7-1357	7	46	14	46	14	replace "using and" with "using an" [Peter Irvine, Germany]	text now reworded - sentence dropped
7-1358	7	46	14	46	25	The use of scaling to compensate for emission biases (or refractive indices?) concerns me, in particular because the used methodology is not available to the reviewer as it is in a submitted paper and contains work that has been kept embargoed for a long time. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	text now reworded - sentence dropped
7-1359	7	46	14			“... using and ...” should be “... using an ...” [James Coakley, United States of America]	text now reworded - sentence dropped
7-1360	7	46	15	46	18	"Scaling for"-sentence should be rewritten=> Scaling .. led to a 50% larger estimate in RF, whilst correcting for BC at too altitudes led to a smaller RF by 15%. [Michael Schulz, Norway]	text now reworded - sentence dropped
7-1361	7	46	17			(Noted by two reviewers) What is meant by "too high altitudes in models"? [Government of United States of America]	text now reworded - sentence dropped
7-1362	7	46	20			"RFs from Bond et al. (2012) were assessed to be +0.17, +0.12 and +0.15 W m–2 from 21 fossil fuel, biofuel and open burning sources, respectively." => the Bond et al revision now gives 0.29, 0.22, 0.20 as estimate. There is thus a considerable difference to the Myhre et al estimate. One consequence of a higher estimate would be to assume higher emissions and higher negative OA and possibly also SO4 forcing. I would think that the Myhre (with more recent model data) and Bond studies could used with equal weight to obtain a best estimate of 0.4 W m-2 with a larger range in uncertainty. [Michael Schulz, Norway]	text now reworded - a diffferent assessment is now made
7-1363	7	46	23	46	24	These numbers have changed significantly in a revision of Bond et al. (2012) [Steven Ghan, United States of America]	text now reworded - a diffferent assessment is now made
7-1364	7	46	23			Refering simply to the Bond et al. 2012 scaling might make the IPCC report not very transparent wrt to the pathway to arrive to the current best estimate. The Bond scaling is not specifically for anthropogenic FF and BF BC Rfari, but rather for an all source BC. It might be useful to highlight the role of aerosol size, OA, dust & NO2 absorption in sun photometer absorption data interpretation for retrieving BC from Aeronet. [Michael Schulz, Norway]	text now reworded - a diffferent assessment is now made that is hopefully more transparent
7-1365	7	46	27	46	33	The discussion doesn't correspond to fig 7.18. Is there a difference made between OA and SOA? Which? Seems in the text but not in the figure. What is POM? [Andrea Flossmann, France]	text and figure homogonised. Acronyms spelt out. OA used
7-1366	7	46	29	46	29	For BB the BC and OA is not shown separately as indicated in the text. It would however, be a good idea to show that. [Gunnar Myhre, Norway]	numbers now given.
7-1367	7	46	30	46	30	The acronym POM needs to be defined here. [Anthony Del Genio, United States of America]	changed to OA

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7-1368	7	46	30	46	30	OA is used elsewhere instead of POM [Gunnar Myhre, Norway]	changed to OA
7-1369	7	46	30	46	33	sentence is not clear [Peter Irvine, Germany]	sentence reworded
7-1370	7	46	33	46	33	I couldn't find Section 7.3.6 (Section 7.3 ends with 7.3.5). [Ralph Kahn, United States of America]	reference updated
7-1371	7	46	36	46	36	What does 'This' refer to? The correction or the estimate? [Gunnar Myhre, Norway]	text now clarified
7-1372	7	46	49	46	52	Figure 7.18: add lable of y-axis Rfari? ; expand in figure caption the BC FF, OC FF, BB, SOA and the difference between total and total modified [Andrea Flossmann, France]	figure improved
7-1373	7	46	50	46	50	The "ingrowing" whisker at the top of the SOA bar looks a bit funny - is it correct (if it is, I dont understand it)? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	figure corrected
7-1374	7	46	54	47	32	It is surprising to me that there is not much description about absorbing aerosols over snow from satellite remote sensing. Although I am not familiar with the subject, I remember Hori et al. (Remote Sens. Environ. Vol 111, pp. 291-336, 2007) showed snow impurity global maps from satellite. I guess there will be more papers since 2007. [Teruyuki Nakajima, Japan]	we agree. Sentence now added - reference to Warren 2013 made
7-1375	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, United States of America]	Accepted. Sections 7.4.3.2 and 7.4.3.3 now make it clear than increases in the aerosol result in suppression of rain from a microphysical perspective. In the general case, however, we do stand by the nuanced responses outlined in 7.4.3.1.
7-1376	7	47	5	47	8	The work onforing due BC deposited onHimalayan glaciers (snow darkening effect) by Nair et al., 2012 submitted to Tellus, is to be cited here as it gives a sort of quantification of radiative effects based on model and measurements over the western himalayas [K KRISHNA MOORTHY, INDIA]	Rejected. Not published in time to consider citing
7-1377	7	47	36	47	44	After having paid a considerable amount of effort of being precise on terminology, it is unfortunate to see in this paragraph the terms “cloud radiative forcing” and “cloud forcing”. Please use the term “cloud radiative effect” to be consistent with Fig. 7.6 and section 7.2.1.2. [Lazaros Oreopoulos, United States of America]	Accepted and corrected
7-1378	7	47	37	47	37	SW, LW, please show the abbreviation somewhere in this chapter or other chapters in front of this chapter. [Junying Sun, China]	SW and LW are now spelled out
7-1379	7	47	39	47	42	It is WRONG to estimate RFaci and AFaci from the change in cloud forcing, if no special care is taken to avoid “contamination” by direct aerosol absorption. [A somewhat analogous situation is that the change in cloud forcing cannot be used to evaluate cloud feedback (Soden et al., 2004, J. Climate, 17, 3661-3665).] Ghan et al. (2012) described how to do this with specially designed model simulations. I suspect that not all studies followed that good practice, and encourage the authors to look into this issue. The reference is Ghan, S. J., X. Liu, R. C. Easter, R. Zaveri, P. J. Rasch, J.-H. Yoon, B. Eaton, 2012: Toward a Minimal Representation of Aerosols in Climate Models: Comparative Decomposition of Aerosol Direct, Semidirect, and Indirect Radiative Forcing. J. Climate, 25, 6461–6476. [Yi Ming, United States of America]	yes, we agree and now say this explicitly.
7-1380	7	47	49	47	49	Although the mean Rfaci appears to have remained constant over time, the spread has increased. This needs to be discussed, because it does not get with the overall conclusion that the uncertainties have been reduced overall. [Robert Wood, United States of America]	Taken into account. We got rid of RFaci altogether.
7-1381	7	48	1	48	1	Replace 'larger' by 'stronger' [Gunnar Myhre, Norway]	Paragraph deleted because we do not discuss RFaci any longer
7-1382	7	48	2	48	5	Without defining what is meant by “cloud susceptibility”, the statements within this portion of the text become ambiguous. Later in lines 18-19, for example, the term is used as effective radius and cloud droplet number concentration “susceptibility”, and in lines 25-26 the term “cloud-aerosol susceptibility” (??) appears. To my knowledge, the most commonly used context for cloud susceptibility is actually cloud ALBEDO susceptibility, see e.g., Platnick and Oreopoulos (2008). [Lazaros Oreopoulos, United States of America]	Paragraph deleted because we do not discuss RFaci any longer
7-1383	7	48	3	48	4	I do not exactly understand what "empirical relationships used to bypass cloud activation" means. [Philip	Paragraph deleted because we do not discuss RFaci

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						Stier, United Kingdom of Great Britain & Northern Ireland]	any longer
7-1384	7	48	6	48	6	Do any models NOT use autoconversion that depends upon cloud droplet concentration? Such models should probably be excluded from this analysis [Robert Wood, United States of America]	No, all GCMs estimating ERFaci include autoconversion rates that depend on N.
7-1385	7	48	7	48	7	Models for which autoconversion depends inversely upon cloud droplet concentration do no "automatically" lead to increased liquid water as N is increased. Ackerman et al. (2004, Nature) and other show that this is the case with a very sophisticated model, and Wood (2007, JAS) can reproduce such behavior in a very simple model. Increased entrainment of dry air associated with reduced precipitation, cloud drop sedimentation, and faster evaporation, all can lead to smaller clouds with lower LWP. GCMs tend not to reproduce this behavior very well, but they do have the physics to do so in principle. [Robert Wood, United States of America]	Accepted. GCMs were meant not models per se. We changed that.
7-1386	7	48	7	48	10	It is stated that GCMs simulate an increase in cloud lifetime and liquid water while finer-scale models do not because “smaller droplets also evaporate more readily…a process that is not yet considered in GCMs”. However, in my opinion this greatly over-simplifies the problem in GCMs. In my view the reasons for these differences between GCMs and fine scale models are not yet clear; there could be a number of other dynamical or microphysical interactions that are not resolved or properly parameterized in GCMs that lead to this behavior. This is more or less stated later in the text on p. 7-49, lines 16-19. If we had a clear explanation for this behavior, it would be much more straightforward to address the problem! [Hugh Morrison, United States]	Accepted. we reworded that
7-1387	7	48	9	48	9	"less negative" - maybe I misunderstand, but I read this as implying AFaci is less negative than RFaci, but 7.19 gives little support to this [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Less negative than if the dispersion is not considered. Rewritten
7-1388	7	48	9	48	13	Afaci has been shown to be very sensitive to autonversion formulation (e.g. Rotstayn 2000, doi:10.1029/2000JD900129; Menon et al. 2002, doi:10.1175/1520-0469(2002)059<0692:GSOTAI>2.0.CO;2; Golaz et al. 2011, doi:10.1175/2010JCLI3945.1; Wang et al. 2012, doi:10.1029/2012GL052204) [Jean-Christophe Golaz, United States of America]	added and references cited
7-1389	7	48	10	48	10	"to the cloud droplet size distribution (dispersion)" was suggested to be changed to" spectral dispersion ". [Junying Sun, China]	we deleted "(dispersion)"
7-1390	7	48	15	48	27	I dont believe the satellite results quoted and summarized on Fig 7.19 represent the RFaci - most satellite studies do not estimate Rfaci at all and I am not comfortable with sat obs being designated as such in fig 7.19. Basically inferring relations between particle size and aerosol (or aerosol index) and inferrring some change in albedo from such relations is not RFaci at all - rarely do these satellite studies consider the water budget of the clouds which would have to be held fixed in estimating RFaci. In reality what is 'observed' is some form of AFaci. Rember teh particel szie inforamtion from satellites is convolved with poptoical depth and it is tehr latter that depends most strongly on LWP according to those studies that attempt to account for LWP changes. These studies (eg Chen et al study and Lebsock 2008 and a few others) tend to find a much smaller effect on albedo and this further reminds us of the important role of cloud water changes on the AFaci. The satellite averaging issue may bias results but I personally think this is a red herring and the real issue is observations are seeing a combination of processes and rarely the purely hypothetical Twomey effect. This whole issue of adjustmetns and the role of water path I think needs more emphasis. [Graeme Stephens, United States of America]	Thanks for pointing this out. We moved the purely satellite-based studies (Lebsock et al., 2008; Quaas et al., 2008) from RF to ERF
7-1391	7	48	21	48	22	Scale related issues are also inherent in other studies cited in this paragraph, as highlighted by Grandey and Stier, ACP, 2010. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Paragraph deleted because we do not discuss RFaci any longer
7-1392	7	48	21	48	22	The main problem McComiskey and Feingold (2012) identified that leads to small values from satellite studies is not really related to scale-related averaging, but is from a lack of constrain on liquid water path in most satellite studies. [Minghuai Wang, United States of America]	Paragraph deleted because we do not discuss RFaci any longer
7-1393	7	48	24	48	27	If the criticism by Penner et al. (2011) is valid, I do not understand why the Quaas et al. (2008) results are still used for assessing the overall uncertainty of aerosol forcing without any modification. [Yi Ming, United States of America]	Paragraph deleted because we do not discuss RFaci any longer
7-1394	7	48	27	48	27	Replace higher by 'stronger' [Gunnar Myhre, Norway]	Paragraph deleted because we do not discuss RFaci

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							any longer
7-1395	7	48	32	48	32	Ghan et al. (2012) a found a 0.5 Wm-2 longwave warming from homogeneous nucleation on sulphate aerosol, but it was partially compensated by a shortwave cooling by enhanced cirrus of undetermined magnitude. [Steven Ghan, United States of America]	Reference added
7-1396	7	48	40	48	45	I don't understand how the inverse approach can estimate Rfaci. Surely it must estimate Afaci because lifetime effects are included. Isn't this precisely the reason why the AF concept has been introduced? Estimating Rfaci from inverse methods is akin to having one's cake and eating it. [Robert Wood, United States of America]	Paragraph deleted because we focus on bottom-up studies alone
7-1397	7	48	43	48	44	Despite the claim by Knutti et al. (2002), it is impossible for any inverse study to constrain RFaci, which is instantaneous in nature. Any observation is bound to include some kind of response. [Yi Ming, United States of America]	Paragraph deleted because we focus on bottom-up studies alone
7-1398	7	48	43	48	44	I am skeptical as to whether RFaci can be distinguished from AFaci in an inverse method. See also discussion by Lohmann and Feichter (ACP, 2005, section 2.6). Incidentally, that paper mentions other papers as well besides Knutti et al. (2002) that attempt this inverse calculation (which again, I don't believe yields RFaci; these papers of course use the old ambiguous terminology of "indirect aerosol effect" and may be inferring RFaci+ AFaci). [Lazaros Oreopoulos, United States of America]	Paragraph deleted because we focus on bottom-up studies alone
7-1399	7	48	43	48	44	Don't inverse studies, such as Knutti et al, bracket the total of RFaci+AFaci+RFari instead of RFaci? [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Paragraph deleted because we focus on bottom-up studies alone
7-1400	7	48	45			Perhaps I missed something. Why is Murphy et al (2009?) study not mentioned here? How is this different than on Page 49? This highlights my confusion about terminology [Andrew Gettelman, United States of America]	Paragraph deleted because we focus on bottom-up studies alone
7-1401	7	48	47	48	48	Should cloud altitude also be mentioned here? Somewhere in section 7.5.2 a dicussion of how aerosols may influence cloud top could be discussed. [Gunnar Myhre, Norway]	Added at the beginning of 7.5 as this paragraph is now redundant
7-1402	7	48	53	49	48	It is arguably one of the most, if not the most, important sections in Chapter 7. I, however, find the overall methodology problematic, and some of the arguments poorly supported. As a result, the uncertainty ranges produced here may have been seriously underestimated. The specific comments are as follows. [Yi Ming, United States of America]	We acknowledge this and have revised our estimate of ERFari+aci accordingly.
7-1403	7	48	53	49	48	To put things into the greater context, the uncertainty range of AFaci is only slightly larger than that of CO2 (Fig. 8.17 in Chapter 8). How can one reconcile this with AFaci's "very low" level of scientific understanding (Fig. 8.16 in Chapter 8)? [Yi Ming, United States of America]	We acknowledge this and have revised our estimate of ERFari+aci accordingly.
7-1404	7	48	53			Section 7.5.3 needs a good summary graph of the various forcing. This could follow Figure 7.19 integrating the corresponding results. [European Union]	As we got rid of AFaci estimates, Fig 7.19. is now much simpler and such a summary graph is not needed
7-1405	7	48	55			avoid jargon, spell out: liquid phase stratiform cloud [Andrea Flossmann, France]	corrected
7-1406	7	48	56	48	56	How can ari play a different role when clouds interact with aerosols? How can one explain this physically? [Robert Wood, United States of America]	sentence deleted because AFaci is no longer discussed.
7-1407	7	48	57	49	1	Is this statement about "convective clouds" (and "+CNV" treatment in Figure 7.19) intended to refer only to deep convective (ice) clouds, or also to shallow cumulus? Some models (e.g., GFDL-CM3) include aci in shallow convective (liquid) clouds, but not in deep convection. [Larry Horowitz, United States of America]	it depends on the model.
7-1408	7	49	1	49	2	consider rephrasing this sentence [Peter Irvine, Germany]	sentence rewritten
7-1409	7	49	11	49	12	"… models systematically misrepresent…" This is a very strong statement ! Is it excessively negative ? References ? [Robert Kandel, France]	sentence rewritten
7-1410	7	49	11	49	43	The impression is given that the result of Wang et al. is considered superior based on the fact that it is based on a multi-scale modelling framework. Without more specific evidence, e.g. through demonstration of superior skill, it does not seem to be justified to give one study such a weight based on methodology only. [Philip Stier,	I think this is true and highlighting this study is justified. We however don't use this estimate to come up with our estimate of ERFari+aci but base that on all

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						United Kingdom of Great Britain & Northern Ireland]	GCM studies that include secondary processes (aerosol effects on mixed-phase and convective clouds) and on satellite studies.
7-1411	7	49	13	49	14	Section 7.5.3 : « In fact most GCMs neglect the radiative effect of convective clouds entirely» is a misleading statement, as the definition of « convective clouds » greatly varies from one model to another. The cloudiness associated with cumulus convection may still be considered as a separate cloud fraction in some models. However, in other models, it is derived from statistical cloud schemes coupled to subgrid-scale condensation processes that aim at representing the cloudiness (and associated radiative effects) associated with many different processes including shallow and deep convection (e.g. Bony and Emanuel 2001, Jam et al., 2012). In this case, model outputs will not necessarily include a « convective cloud fraction » per se, but the radiative effects of this cloud fraction will be taken into account. [Sandrine BONY, France]	sentence rewritten
7-1412	7	49	13	49	14	GFDL CM3 includes radiative clouds of convective clouds (Donner et al 2011, doi:10.1175/2011JCLI3955.1) [Jean-Christophe Golaz, United States of America]	The sentence has ben rewritten
7-1413	7	49	15	49	16	The GFDL AM3 model (doi:10.1175/2011JCLI3955.1) includes the radiative impact of convective clouds and the mesoscale anvils arising from them. [Robert Pincus, United States of America]	The sentence has ben rewritten
7-1414	7	49	16	49	16	Should cite Wang et al. (2011b) here. [Steven Ghan, United States of America]	Done
7-1415	7	49	19			The authors should consider removing the reference to lifetime effects that should be removed. [Government of United States of America]	rejected because it's necessary to discuss its uncertainties
7-1416	7	49	20	49	22	Regarding the statement - "One of the most reliable estimate in the +CNV category may be the estimate of –1.1 W m–2 to the farthest right because it is obtained from a multi-scale modelling framework approach (Wang et al., 2011b), which resolves convection." This MMF is using 2-D CRMs, and as such is still a parameterization, albeit different than standard GCMS. The fact that it gives different results for +CNV is certainly intriguing, but that those results are 'more reliable' may be misleading and, therefore, the authors should consider revising the text to clarify. [Government of United States of America]	Taken into account
7-1417	7	49	22	49	22	Add the following sentence here: "Wang et al. (2012) showed that the multi-scale modeling approach yields precipitation and liquid water path sensitivityto aerosol that is more consistent with satellite evidence than a typical GCM, and that this improvement is due to a much smaller role of autoconversion in precipitation from low clouds, most likely because precipitation is determined prognostically in the multi-scale modeling framework and diagnostically in most GCMs." Wang, M., Ghan, S., Liu, X., L'Ecuyer, T., Zhang, K., Morrison, H., Ovchinnikov, M., Easter, R., Marchand, R., Chand, D., Qian, Y., and Penner, J.E., 2012. Strong constraints on cloud lifetime effects of aerosol using satellite observations. Geophys. Res. Lett., 39, 15, doi:10.1029/2012GL052204. [Steven Ghan, United States of America]	reference added where prognostic precipitation is discussed (7.5.2). Note that the title is incorrect is provided in the comment.
7-1418	7	49	22			I disagree that the MMF approach (e.g., Wang et al. 2011b) "resolves convection". Typical horizontal grid spacing of the embedded CRM in the PNNL-MMF is ~ 2-4 km, and was 4 km in the Wang et al. 2011b study. This only marginally resolves deep convective scale motions, and does not resolve shallow convection. Thus, I suggest changing "resolves convection" to "permits explicit convection", or something like this. [Hugh Morrison, United States]	changed
7-1419	7	49	26		28	This inverse estimate, like any, depends on the assumed sensitivity. Murphy assumed (inferred from seasonal variation) a middle of the road sensitivity; but with the range of sensitivity that is constrained by satellite obs or otherwise, there is little constraint on total or aerosol forcing. Certainly the dependence of his assumed forcing on the sensitivity employed in the calculation should be noted. [Stephen E Schwartz, United States of America]	Taken into account. After discussions at LA4, we decided to change our estimate of ERFari+aci to just bottom-up. The Murphy study will be discussed in chapter 10 instead.
7-1420	7	49	30	49	32	Does this estimate include any of the CMIP5 models in which AFaci is identically zero (i.e., in which aerosol indirect effect is neglected)? If so, are they weighted equally with models representing aci? [Larry Horowitz, United States of America]	Taken into account. We don't use the CMIP5 models any longer in our estimate of ERFari+aci because we only consider bottom-up estimates. The CMIP5 models include a-posterio information. We mention that

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7-1421	7	49	30	49	32	When computing the uncertainty range of AFari+aci, the authors choose to consider only a subset of CMIP5 model (coupled atmosphere-ocean GCM) estimates, while excluding the free-running atmosphere-only GCM estimates (e.g., those denoted as +MPC and +CNV in the upper panel of Fig. 7.19). No justification is given. It is well known that aerosol forcing has to be in a certain range so that a coupled GCM can reproduce the historical warming (see Kiehl, J. T. (2007), Twentieth century climate model response and climate sensitivity, Geophys. Res. Lett., 34, L22710, doi:10.1029/2007GL031383). Thus, the atmosphere-only GCM estimates would provide a more faithful measure of the true uncertainty than the CMIP5 model results. If the former are properly taken into account, my sense is that the uncertainty range would be significantly larger. [Yi Ming, United States of America]	good point, we changed our estimate exactly as you have suggested
7-1422	7	49	30	49	32	As far as I know, all inverse estimates of aerosol forcing are in the form of a mean accompanied by a wide uncertainty range, reflecting the difficulty in constraining aerosol forcing and climate sensitivity simultaneously. It appears to me that the uncertainty range of AFari+aci presented here considers the mean values only, not the inherent uncertainties, and thus may have been underestimated. [Yi Ming, United States of America]	Taken into account. After discussions at LA4, we decided to change our estimate of ERFari+aci to just bottom-up. The inverse study will be discussed in chapter 10 instead.
7-1423	7	49	30	49	32	What is the rationale for grouping CMIP5 model and inverse estimates together? The two approaches are fundamentally different. What are the relative weights? [Yi Ming, United States of America]	Taken into account. We now estimate ERFari+aci based only on GCM studies that include secondary processes (aerosol effects on mixed-phase and convective clouds) and on satellite studies.
7-1424	7	49	30	49	36	The stated 5-95% uncertainty range for AFari+aci seems much narrower than the model and observational data would support. For instance, from the GFDL-AM3 model, the cited aerosol AF value of -1.44 W/m2 (from ACCMIP) can be compared with other estimates from the same model of -1.60 W/m2 (from the CMIP5 fixed-SST runs) and -1.80 W/m2 (cited by H. Levy et al, submitted to JGR) using fixed, but interannually varying, SSTs. It seems like the possibility of structural uncertainties leading to larger estimates of aerosol AF cannot be ruled out with such high certainty. [Larry Horowitz, United States of America]	Taken into account. These model estimates are now within the uncertainty range of -1.9 to -0.1 Wm-2. Moreover it should be noted that these are 90% uncertainty range so individual estimates can be outside this range, also because we do make an expert judgement to shift the range from model estimates.
7-1425	7	49	30	49	43	The choice of -0.9 for the best estimate of Rfaci+ari seems to be too strongly weighted toward the satellite measurements. As explained in the previous page (P48, Line 15-27), the satellite data cannot be used to estimate Rfaci+ari because they do not take into account the preindustrial cloud droplet concentration and are subject to large measurement errors for the aerosol optical properties. Again, it seems confusing to me how RF can be determined from observations when the cloud water and cloud cover changes are both impacted by aerosols in the real world. It is also well understood that clouds affect aerosols and so how can one infer a one-directional causality (aerosols impacting clouds) from observed correlations between aerosols and clouds? All of this should lead to (at best) a huge error bar on the satellite measurements and less weighting toward such measurements in the best estimate [Robert Wood, United States of America]	Taken into account. We now estimate ERFari+aci based only on GCM studies that include secondary processes (aerosol effects on mixed-phase and convective clouds) and on satellite studies. The satellite studies still have equal weight as the GCM studies, but the uncertainty range in the GCM studies is much larger so that the combined uncertainty range now is -0.1 and -1.9 W/m2. Note that under satellite studies also studies that combine GCM estimates with satellite data are considered. We made that clearer.
7-1426	7	49	30	49	43	A straight sum of the five different estimates in Fig. 7.19 for Afaci+ari gives something like -1.2 W/m2. Why is the best estimate much lower than this? [Robert Wood, United States of America]	Taken into account. The text now explains that we do not consider all GCM estimates here
7-1427	7	49	30		32	The approach, conistency between models and inverse estimates, runs the risk of circular reasoning: Rodhe, H., Charlson, R.J. and Anderson, T.L. (2000). Avoiding circular logic in climate modeling. Climatic Change 44, 419-422. [Stephen E Schwartz, United States of America]	agreed, we decided to change our estimate of ERFari+aci to just bottom-up. The inverse study will be discussed in chapter 10 instead
7-1428	7	49	32	49	34	It is acknowledged that the number of satellite-based estimates is rather limited. Why should they, as a whole, be given the same weight as the CMIP5 model and inverse estimates? [Yi Ming, United States of America]	Taken into account. We now estimate ERFari+aci based only on GCM studies that include secondary processes (aerosol effects on mixed-phase and convective clouds) and on satellite studies. The satellite studies still have equal weight as the GCM studies, but 6 of the 9 satellite studies are studies that combine GCM estimates with satellite data are considered. We made that clearer.
7-1429	7	49	35	49	35	This appears to be a typo. "RFari+aci" should be replaced by "AFari+aci" here. [Larry Horowitz, United States	Corrected

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						of America]	
7-1430	7	49	35	49	35	The best estimate and range of -0.9 (-1.5 to -0.3) W/m^2 given is actually for AFari+aci, not for RFari+aci as stated. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Corrected
7-1431	7	49	35	49	35	Shouldn't this be Afari+aci not Rfari+aci? [Robert Wood, United States of America]	Corrected
7-1432	7	49	35			The text gives a value for RFari+aci, but it seems like the authors may have meant for it to be AFari+aci [Government of United States of America]	Corrected
7-1433	7	49	36	49	36	"lower" - if I am understanding this properly, I think "lower" really means "less negative" (i.e. higher!) [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Corrected
7-1434	7	49	36	49	43	An attempt is made to justify the much reduced uncertainty range with four “new lines of evidence”, which are anecdotal at best. The claim that the estimate of aerosol forcing made with a super-parameterized GCM is more reliable than those with conventional models is based on an overgeneralization of the Wang et al. (2011b) study of different variants of the NCAR CAM, and thus is totally misleading. The statements at L20-22 and L37-38 of P49 should be removed. I see no merit in the third and fourth reasons, which effectively amount to circular reasoning as the satellite-based and CMIP5 model estimates provide the basis of the final uncertainty range. [Yi Ming, United States of America]	Partly taken into account. We disagree with the comment on the MMF approach because we consider it as superior because it permits deep convection. We agree that the second and third reason are now redundant and removed them.
7-1435	7	49	37	49	37	I struggled here a bit, because of the switching between RF and AF. As I understand, the -1.1 from Wang et al is closer to the AR4 RF and yet it seems to be used here to justify the the AR5 value. Could you clarify? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Partly taken into account. It's actually exactly in between the AR4 estimate of -1.2 W/m2 and AR5 of -1 W/m2. However if you look at Figure 7.19. there are many more estimates that are more negative than -1 W/m2 and which are less physical. The whole section has been clarified hopefully.
7-1436	7	49	37	49	37	The MMF doesn't do a very good job of reproducing the low clouds that are thought to be the primary conduit for Aci. I wouldn't hang my hat on it's ability to estimate Afaci [Robert Wood, United States of America]	Partly taken into account. We now mention one limitation of the MMF approach
7-1437	7	49	38			References for these studies? [Andrew Gettelman, United States of America]	references are given above (e.g. Wang et al., 2011 or Posselt and Lohmann, 2009)
7-1438	7	49	45			I still find this terminology very confusing, and think it should be more tightly coupled to the traditional terms used in the literature. I keep getting lost. It does not make sense that RF is part of AF. ari= direct + semi direct, aci= indirect (where RF Twomey effect is,part of that?). Still confusing. What about mentioning direct (AFari) and total flux perturbation AFari+AFaci? [Andrew Gettelman, United States of America]	Noted. Terminology is introduced in Section 7.1 and figure 7.3. It is used consistently throughout the chapter. Note that AF was changed to ERF.
7-1439	7	49	47			estimate of ' what? RFaci or Afaci [Andrew Gettelman, United States of America]	paragraph rewritten.
7-1440	7	49	50	50	9	Figure 7.19 summarizes probably the most important result of this chapter an should be elaborated to be more informative. [Government of Poland]	Taken into account. Figure 7.19 shows better the thinking behind the new estimate for ERFari+aci.
7-1441	7	49	51	49	51	This is an important figure but I had a couple of comments on it. The colours of the upper 2 panels do not seem to be decoded for the reader, and so it is hard to know what the labels ari and aci refer to in these figures - is it just the colour in which they are associated (so for example, in top left, does the aci refer to just the orange, or to the blue and red too?) [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account. Colors are now explained
7-1442	7	49	51	50	9	figure caption of Figure 7.19: "published prior to AR4.." is this really what you want to say? Or is it that these values were integrated into AR4,… Otherwise, it seems bizarre that you would put random values up there, without references the studies of AR4,..., right? you should also spell: +MPC (Mixed Phase Clouds) and +CNV (CoNVecitve clouds), withe capital letters to explain the acronym, as it took me a while to figure it out. Also, would be nice to explain the color coding. Att: the caption for lower panel is not integrated into the caption section, thus it does not appear under the figure, but as main body text. Here also you should spell out liquid phase stratiform clouds. [Andrea Flossmann, France]	Taken into account. Colors are now explained and acronyms are spelled out. The reviewer is correct about the caption to the lower panel, which TSU had forgotten to copy paste.
7-1443	7	49				Figure 7.19: The explanations for the lower panel are missing. [Government of Germany]	Noted. There are on the next page. This is a mistake made by the TSU when formatting the final SOD.

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7-1444	7	50	1			The authors might consider adding another table here, listing the AR5 best estimates of RF and AF values for aerosol. This would make a nice summary of this section. [Government of United States of America]	Taken into account. We added a table with the values that are used for our best estimate
7-1445	7	50	2	50	9	This text is missing from the actual figure caption beneath the figure on p. 7-130. [Anthony Del Genio, United States of America]	Noted. This is a mistake made by the TSU when formatting the final SOD.
7-1446	7	50	15	50	56	Section 7.6.4 (Aerosol-Clouds Interactions) has the same title as the entire Section 7.4. The authors should consider renaming Section 7.6.4 as Aerosol-Precipitation Interactions. Discussion should include aerosols effects on different rain and cloud types, e.g. light vs. heavy precipitation, warm rain vs. ice-phase rain processes (e.g., Li et al 2011, Rosenfeld et al. 2008) in the context of climate. Also included should be effects of precipitation on removal of aerosols on the distribution of aerosol loading. (See partial list of references) A lot of recent work on possible impacts of aerosols on regional precipitation and hydrologic cycles have been left out. These include many recent studies on aerosol effects on precipitation in monsoon regions of Asia, Australia, West Africa , and South America including the Amazons . There authors should consider including a separate paragraph to cover this important topic, with cross-references to Chapter 14, where regional phenomena, including aerosol effects on precipitation, and water cycle are discussed (See partial list of references below). References Changing rainfall characteristics (partial list) 1. Allan, R. P., and B. J. Soden (2008), Atmospheric warming and the amplification of precipitation extremes, Science, 321, 1481–1484, doi:10.1126/ science.1160787. 2. Shiu, C-J, S-C Liu, C. Fu, A. Dai, Y. Sun , 2012: How much do precipitation extremes change in a warming climate?, Geophysical Research Letters, 2012, 39, 17 3. Lau K.-M., and H. T. Wu, 2003: Warm Rain Processes Over Tropical Oceans and Climate Implications. Geophys. Res. Lett., vol. 30, No. 24, 2290, doi:10.1029/2003GL018567. 4. Lau, K. M., and H. T. Wu, 2006: Trends in tropical rainfall characteristic, 1979-2003. Int.. J. Climatology, 27, 979-988, doi:10.1002/joc.1454. 5. Lau, K.-M., and H.-T. Wu (2011), Climatology and changes in tropical oceanic rainfall characteristics inferred from Tropical Rainfall Measuring Mission (TRMM) data (1998–2009), J. Geophys. Res., 116, D17111, doi:10.1029/2011JD015827. 6. Lau, K.-M., Y. P. Zhou, and H.-T. Wu (2008), Have tropical cyclones been feeding more extreme rainfall?, J. Geophys. Res., 113, D23113, doi:10.1029/2008JD009963. 7. Lintner, B. R., M. Biasutti, N. S. Diffenbaugh, J.-E. Lee, M. J. Niznik, and K. L. Findell (2012), Amplification of wet and dry month occurrence over tropical land regions in response to global warming, J. Geophys. Res., 117, D11106, doi:10.1029/2012JD017499. 8. Trenberth, K. E., A. Dai, R. M. Rasmussen, and D. B. Parsons (2003), The changing character of precipitation, Bull. Am. Meteorol. Soc., 84, 1205–1217, doi:10.1175/BAMS-84-9-1205. 9. Qian Y., D. Gong, R. Leung, 2010: Light rain events change over North America, Europe, and Asia for 1973–2009, Atmospheric Science Letters, 11, 4. Aerosol effects on precipitation/cloud types (partial list) 1. Fan J, D Rosenfeld, Y Ding, LYR Leung, and Z Li. 2012. "Potential Aerosol Indirect Effects on Atmospheric Circulation and Radiative Forcing through Deep Convection." Geophys.l Res. Letters 39: Article No. L09806. doi:10.1029/2012GL051851 2. Li, Z., F. Niu, J. Fan, Y. Liu, D. Rosenfeld, and Y. Ding, 2011: Long-term impacts of aerosols on development of clouds and precipitation. Nature Geoscience, doi:10.1038/NGEO1313. 3. Ramanathan V., P J. Crutzen, J. T. Kiehl, D. Rosenfeld, 2001: Aerosols, Climate and the Hydrological Cycle, Science, Vol. 294 no. 5549 pp. 2119-2124 DOI: 10.1126/science.1064034 4. Rosenfeld, D., U. Lohmann, G. B. Raga, C. D. O'Dowd, M. Kulmala, S. Fuzzi, A. Reissell, M. O. Andreae, 2008: Flood and drought: how do aerosols affect precipitation. Science, 321, 1309,	Taken into account. The title of section 7.6.4 was changed. We have not changed the outline though. Aerosol-cloud-precipitation for weakly precipitating clouds are discussed in Section 7.4. Changes in precipitation induced by circulation changes are discussed in Chapter 14. We have added appropriate cross-references.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						doi:10.1126/science.1160606. 5. Rotstayn, L.D., U. Lohmann, 2002: Tropical Rainfall Trends and the Indirect Aerosol Effect. J. Climate, 15, 2103–2116. doi: http://dx.doi.org/10.1175/1520-0442(2002)015<2103:TRTATI>2.0.CO;2 Aerosol effects on regional precipitation and water cycle (partial list) 1. Fan J, LYR Leung, Z Li, H Morrison, H Chen, Y Zhou, Y Qian, and Y Wang. 2012. "Aerosol Impacts on Clouds and Precipitation in Eastern China: Results from Bin and Bulk Microphysics." Journal of Geophysical Research. D. (Atmospheres) 117: Article No. D00K36. doi:10.1029/2011JD016537 2. Gautam, R., C. Hsu, K. M. Lau and M. Kafatos, 2009: Aerosol and rainfall variability over the Indian monsoon region : Distributions, Trends and Coupling. Geophys, Annales, 27, 3691-3703, www.ann-geophys.net/27/3691/2009/ 3. Huang, Y., W. L. Chameides, and R. E. Dickenson, 2007: Direct and indirect effects of anthropogenic aerosols on regional precipitation over East Asia. J. Geophys. Res., 112 (D03211), doi: 10.1029/2006/2006JD007114. 4. Lau, K . M., and K.-M. Kim, 2010: Fingerprinting the impacts of aerosols on long-term trends of the Indian summer monsoon regional rainfall, Geophys. Res. Lett., 37, L16705, doi:10.1029/2010GL043255. 5. Lau, K. M., K. M. Kim, C. Hsu and B. Holben, 2009: Possible influences of air pollution, dust and sandstorms on the Indian monsoon rainfall. WMO Bulletin, 58 (1), 22-30 6. Lau, K. M., and K. M. Kim, 2006: Observational relationships between aerosol and Asian monsoon rainfall, and circulation, Geophys. Res. Lett. 33, L21810, doi:10.1029/2006GL027546. 7. Lau, K. M., M. K. Kim, and K. M. Lau, 2006: Aerosol induced anomalies in the Asian summer monsoon: The role of the Tibetan Plateau. Climate Dynamics, 26 (7-8), 855-864, doi:10.1007/s00382-006-0114-z. 8. Lau, K. M., Kim, K. M., Sud, Y. C., and Walker, G. K., 2009: A GCM study of the response of the atmospheric water cycle of West Africa and the Atlantic to Saharan dust radiative forcing, Ann. Geophys., 27, 4023-4037, doi:10.51941 Angeo-27-4023-2009. 9. Martins, J. A. , Dilva Dias, M. A. F., and Goncalves, F. L. T, 2009: Impact of biomass burning aerosols on precipitation in the Amazon: A modeling case study. J. Geophys. Res., 114, D02207, doi:10.1029/2007JD009587. 10. Qian Y, D Gong, J Fan, LR Leung, R Bennartz, D Chen, and W Wang. 2009. "Heavy pollution suppresses light rain in China: observations and modeling." Journal of Geophysical Research. D. (Atmospheres) 114: article number D00K02. doi:10.1029/2008JD011575 11. Rotstayn, L. D., W. Cai, M. R. Dix, G. D. Farquhar, Y. Feng, P. Ginoux, M. Herzog, A. Ito, J. E. Penner, M. L. Roderick, and M. Wang (2007): Have Australian rainfall and cloudiness increased due to the remote effects of Asian anthropogenic aerosols? J. Geophys. Res., 112, D09202, doi:10.1029/2006JD007712. 12. Wang, C., Kim, D., Ekman, A. M. L., Barth, MC., and Rasch, P. J., 2009: Impact of anthropogenic aerosols on Indian summer monsoon. Geophys. Res. Lett, 36, L21704, doi:10.1029/2009GL040114. [Government of United States of America]	

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7-1447	7	50	18			Section 7.6. As I commented on page 5 lines 30-42, I would not expect precipitation changes to be covered in this chapter; although precipitation is related to clouds, it's a different subject. Changes in precipitation are dealt with in 12.4.5, and I think it would be logical to move this material to that section. [Jonathan Gregory, United Kingdom]	The authors from Chapters 7, 10, 11 and 14 met to discuss the treatment of precipitation throughout the report. It was decided that the main processes that drive robust changes in precipitaiton would be presented in Chatper 7 because many of them (changes in water vapor, or aerosol heating) are already discussed in this process chapter. The subsequent chapters agreed to then build on the process level discussion in Chapter 7 in their discussion of projected changes. They also indicated a willingness to remove redundant material in their chapters so as to provide a more integrated report as a whole. To reflect these changes the introductory section (7.6.1) has been rewritten to better guide the reader as to the treatment of precipitation throughout the report, and this introduction is also being provided to Chapter 1 in the hope that it will be incoporated in their presentation of the report as a whole.
7-1448	7	50	22	50	24	I had a little trouble understanding what was meant by 'differential heating of the surface relative to the atmosphere'. Is the point being made that it is a function of the net radiation at the surface, or is it something else - ie a differential heating which results in a different air/sea temperature difference? Either way, I think it would be useful to acknowledge the perspective outlined in many papers (see above) that the precipitation is constrained mainly by the radiative cooling of the atmosphere. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Yes, both perspectives are useful, but the authors agree with the reviewer that approaching the constraint from the point of view of the rate of radiative cooling within the troposphere is more elegant. The introductory section has thus been rephrased to emphasize this point.
7-1449	7	50	23	50	23	What does the atmospheric heating rate have to do with the supply of energy for evaporation at the surface? Would it not be more accurate to say "by the generally positive net radiation at the surface"? [J. Graham Cogley, Canada]	In reponse to suggestions by other reviewers, this discussion has been reformulated to emphasize the tropospheric cooling rate, and thereby avoids the inelegance pointed out by this reviewer.
7-1450	7	50	24			“by making water available for” seems clumsy to me - “allows”? “leads to”? (with “and” in next line before “warms” [William Ingram, United Kingdom]	This sentence has been rephrased and thereby avoiding the clumsy phrasing noted by the reviewer.
7-1451	7	50	32			Figure 7.20: Caption needs information on different colours used in the graphic (blue, red etc.) [Thomas Stocker/ WGI TSU, Switzerland]	Figure has been deleted.
7-1452	7	50	33	50	40	Figure 7.20 needs to discussed somewhere. The colors and a reference to preceeding chapters or litterature. This diagram is so condensed that I am not sure to have correctly understood the message. [Andrea Flossmann, France]	Figure has been deleted as the point it was making was complicated and possibly unclear.
7-1453	7	50	34	50	34	"… for evaporation, which is later released by condensation." Condensation is a source of energy. [J. Graham Cogley, Canada]	Yes, but it must balance the rate of energy extraction by radiative processes. This hopefully is clearer in the revised document which focuses on the energy balance of the troposphere.
7-1454	7	50	36	50	37	I would delete both instances of "referred to as". [J. Graham Cogley, Canada]	The sentences were rephrased following the reviewers suggestion.
7-1455	7	50	39			The phrase "increase, respectively decrease" is not clear. Please clarify. [Government of United States of America]	This has been rephrased
7-1456	7	50	43	50	45	What robust features? The statement “in almost every case, the Clausius-Clapeyron relation,……., underlies these robust features” is not true. There are many robust features in precipitation response that are more related to the changes in the large scale circulation, amplified by water cycle feedback processes giving rise to the “rich-getting-richer and poor-getting-poorer” scenario (see discussed in Section 7.6.2), not constrained by the CC relation. The authors should consider revising the text to reflect this reality. [Government of United	This has been rephrased, it is now stated that the robust changes and their explanation are described in the text that follows this statement.

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						States of America]	
7-1457	7	50	45	50	45	Use "vapor pressure" in place of Clausius-Clapeyron to reduce jargon. [Daniel Murphy, United States of America]	The reference to Clausius-Clapeyron has been removed and now it is indicated simply that changes can be related to well understood processes as discussed in the subsequent text.
7-1458	7	50	47	51	27	Section 7.6.2 : It could be mentionned that the pattern of precipitation change induced by the effect of warming is substantially correlated to the pattern of present-day circulation, suggesting that biases in the simulation of the present-day climate translates into at least one component of precipitation projections (Bony et al., in revision). [Sandrine BONY, France]	This point is a useful one and has been added to the text of this section, it will also be raised for consideration by Chapter 9 where its implications are more directly relevant.
7-1459	7	50	47	51	27	This section is not well written, especially on the "wet-get-wetter" and "dry-get-driver" part. It is hard to follow sometimes. [Minghuai Wang, United States of America]	The need to cover a great deal of material in a condensed form posed challenges for the presentation. However, by addressing a number of constructive suggestions, including those by this reviewer (e.g., comment #1500), we believe that the presentation is substantially improved.
7-1460	7	50	47	51	27	Section 7.6.2 should cross-reference to Ch02 and please ensure the messages are consistent. [Thomas Stocker/ WGI TSU, Switzerland]	This has been checked, and a more specific reference to Chapter 2 has been added.
7-1461	7	50	49	50	53	This paragraph could be strengthened by expanding on the concepts involved a bit. For example, regarding the relation between the increase in specific humidity and precipitation: the cited work is Stephens and Hu (2010); however, there are additional citations on this issue. Furthermore, there are energetic limitations because of the latent heat needed to maintain an additional 7.5% of water in a 1 K hotter atmosphere. The statement about the spectroscopic properties of water vapor is also true and applies to any greenhouse gas. But how are these statements linked? Are energetics a root cause for keeping the rate of precipitation increase below that of water vapor? Please expand on the links among specific humidity, LW absorption, and precipitation in this paragraph (see sections 11.3.2.3, 12.4.5.1 and 12.4.5.2). [Government of United States of America]	This part of the text has been deleted, as the point it made was not essential and the original phrasing was confusing. Now we simply state that the change in the tropospheric cooling rate is less than the change in the saturation vapor pressure with temperature, and it is the former that limits precipitation changes not the latter.
7-1462	7	50	49	51	27	Section 7.6.2 The authors should consider includcing a discussion on the changing rainfall characteristics (types, frequency and intensity) in a warmer climate in this section. Changing rainfall characteristics is a reflection of possible changes in rain structure, cloud population, extreme weather systems, and dynamical feedback (Lau and Wu, 2006, 2011, Lau et al. 2008, 2010). They provide signals of possible impacts of global warming, as well as feedback from local and regional forcing involving aerosols and clouds on precipitation (Li et al. 2011; Rosenfeld et al 2008). Is it covered elsewhere in the report? If so, cross-referencing would be important. [Government of United States of America]	The regionally specific character of precipitation changes is discussed in the later chapters, and this is now indicated in a newly written introduction to section 7.6
7-1463	7	50	49	51	27	The discussion in Section 7.6.2 is mostly on effects of global warming on total precipitation trends. The authors should consider including a discussion on long-term changes in rainfall characteristics (types, duration, and intensity) and possible attributions. [Government of United States of America]	These points are discussed in the projections chapter (12), the interested reader is now referred to this chapter at the beginning of section 7.6 whose introductory sentences have been reformulated.
7-1464	7	50	49			the 7.5% are not consistent with section 7.2.4.1, as stated [Andrea Flossmann, France]	Yes, this is noted and the numbers (6-10% for the surface WVMR) are now incorporated in this section.
7-1465	7	50	51	50	51	I suggest expanding "is energetically limited", perhaps in the form "does not continue to increase in proportion to increases in net radiation". [J. Graham Cogley, Canada]	This section has been rephrased, and the offending statements no longer appear.
7-1466	7	50	51	50	52	"… the spectral absorption bands of water vapour are 'saturated'; the fractional increase in their ability to absorb …". [J. Graham Cogley, Canada]	This section has been rephrased, and the offending statements no longer appear.
7-1467	7	50	51			Is this just in the current climate? (...spectroscopic properties of water in the current climate demand ...) [Andrew Gettelman, United States of America]	These sentences have been reformulated and simplified, and reference has been removed as it was

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							not essential to the argument and only seemed to generate confusion.
7-1468	7	50	55			I think that the reason why Held and Soden (2006) and Richter and Xie (2008) find increases in precipitation with temperature which are on average weaker than quoted by Andrews at al (2009) and other earlier papers (e.g Allen and Ingram,2002, Lambert and Webb, 2008) is that their values include the suppression effect increasing CO2 levels throughout the simulations, as well as the effect of absorbing aerosols, effects which are excluded from the estimates of increasing precipitation with temperature in the other papers. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	This may be, but the discussion is not focussing on the relative difference among these studies, but rather on the point that they collectively are all less than the increase in water vapor. The section has been slightly rephrased which hopefully makes this point somewhat more clear.
7-1469	7	50	57	50	58	I'm not clear what exactly is meant here, but is the implication that the wet get wetter/dry get drier mechanism is caused by a change in the circulation? I don't think that Held and Soden say this. According to my reading of their paper, they argue that if the lower-tropospheric relative humidity is unchanged and the flow is unchanged, the evaporation minus precipitation will increase proportionally with lower-tropospheric vapour as the climate warms. Allen and Ingram 2002 simply argue that this might be expected from the increased specific humidities leading to increased atmospheric fluxes of humidity from its sources to its sinks, citing. Manabe and Wetherald, 1980. Ref: Manabe, S. & Wetherald, R. T. On the distribution of climate change resulting from an increase in CO2 content of the atmosphere. J. Atmos. Sci. 37, 99–118 (1980). Perhaps this could be rephrased to avoid any confusion. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Yes, the original phrasing was awkward and lended itself to misinterpretation. It has been rephrased to be more clear, and is now more obviously in accord with the reviewers understanding.
7-1470	7	50	57	51	27	All this discussion of the "wet-get-wetter" completely omits the recent Scheff & Frierson (2012a,b) which show that in both CMIP3 & CMIP5 changes in P-E – the quantity the argument physically applies to - are very consistently dominated by a "wet-get-wetter" signal, while changes in P are very consistently dominated by a poleward shift of the storm tracks (& so the subtropical dry zones where they abut the storm tracks). These direct & robust results are much more relevant & useful than the current discussion here, which should therefore be cut & replaced. [William Ingram, United Kingdom]	The Scheff and Frierson paper is now cited in the discussion of precipitation shifts.
7-1471	7	50	57			The range of 1.4-3.4%/K used here is from Lambert and Webb, 2008, and is quoted by Andrews et al 2009. Ref: Lambert, F. H. and M. J. Webb (2008), Dependency of global mean precipitation on surface temperature, Geophys. Res. Lett., 35, L16706, doi:10.1029/2008GL034838. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	The references has been corrected as suggested.
7-1472	7	50		53		Section 7.6 : The whole section is very nicely written and organized. However, the title of the section is confusing : what does « Links » mean ? An alternative could be : « Processes underlying precipitation changes ». [Sandrine BONY, France]	The reviewers suggestion was adopted and the introductory section was rewritten to better reflect this purpose.
7-1473	7	50				Table 7.5: Values for MPI and MPI/M are missing. [Government of Germany]	Taken into account. MPI has been removed from the table as no value is available.
7-1474	7	51	7	51	7	No comma after "oceans". [J. Graham Cogley, Canada]	The comma has been deleted.
7-1475	7	51	7	51	27	Regarding the dry gets drier response, I always wonder: during holocene (8000BP) the climate was warmer and the Sahara was green. So there, the dry did get wetter and not drier. What was the difference with respect to today? [Andrea Flossmann, France]	The argument is particularly weak near precipitation margines, i.e., where P-E changes sign. This caveat is now added explicitly to the other caveats.
7-1476	7	51	9			Clearer with "relative" or "fractional" before "changes" & dropping "in a relative sense". (The reference could be dropped too, as it's not from the period AR5 is supposed to cover, & the point must have been made long before) [William Ingram, United Kingdom]	Agreed. The reviewer's suggestion has been adopted.
7-1477	7	51	11			Drop 2nd "in" [William Ingram, United Kingdom]	Done
7-1478	7	51	16	51	18	The placement of the parentheses here suggests that Figures 7.2 and section 7.2 explain the dependence of the weakening circulation on the static stability, but I don't think that is what was intended. Perhaps the closing bracket should come after 'with waming'. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	T has been rephrased to more accurately reference the Figure.
7-1479	7	51	16	51	18	Could the reference to Section 7.2 be relaced with something more specific? Section 7.2 covers several pages. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Yes, this has been done (the reference now points to Section 7.2.5.3)

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7-1480	7	51	18			“This” - WHICH? Reads like “anticipated reduction” but makes more sense if “wet-get-wetter” [William Ingram, United Kingdom]	Yes, the wet-get-wetter is what was meant and this ('wet-get-wetter') is now stated explicitly.
7-1481	7	51	19	51	19	"divergence due to the circulation". [J. Graham Cogley, Canada]	Corrected, following the reviewers suggested wording.
7-1482	7	51	22			What on earth does “differently” mean? (It can't take “than” anyway.) [William Ingram, United Kingdom]	I don't know what I was thinking here. "Differently has been removed"
7-1483	7	51	24	51	24	"complicates interpretation of local responses". [J. Graham Cogley, Canada]	Agreed, and changed following the reviewers suggestion.
7-1484	7	51	25	51	25	"at the scale." [J. Graham Cogley, Canada]	Changed following the reviewers suggestion.
7-1485	7	51	25	51	27	Section 7.6.2: Regarding the phrase, "remains highly uncertain" - Is this a statement of confidence w/in the IPCC framework? Or are the authors saying they didn't account for it? [Government of United States of America]	This has been rephrased to avoid the awkward phrase. It is now stated that on the catchment scale the response of precipitation to warming is not well understood.
7-1486	7	51	29	51	57	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 depends on location, season and spatiotemporal scale of the analysis, its impact on monsoon parameters such as onset, break and active spells via its Intra Seasonal Oscillations (ISOs) needs to be known. Studies are available in the literature, which revealed that absorbing aerosols facilitate transition of monsoon breaks to active spells, and thus help in extended-range prediction. [Panuganti, C.S. Devara, India]	These are good points but more appropriately taken up by Chapter 14. The introduction to section 7.6 now contains a pointer to the discussions of these and related effects in the later chapters, and specifically mentions the Monsoon.
7-1487	7	51	31	51	32	Should earlier papers which discuss the suppression of global precipitation by CO2 not also be cited here? E.g. Mitchell et al 1987, Allen and Ingram 2002, Lambert and Webb 2008? (See above comments for references). [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	These papers have now been cited elsewhere in the section, and so adding them to the list here would only make the text longer and more difficult to describe (see the reviewers subsequent point) as the suite of experiments becomes more inhomgeneous.
7-1488	7	51	31	51	32	It would be helpful to clarify the conditions under which half the change in precipitation can be related to CO2. This will be different for different forcing scenarios, different climate sensitivities etc. I imagine the upper limit will correspond to a CO2 doubling scenario in a model with low climate sensitivity run to equilibrium. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Yes, this is now stated that a quadrupling of CO2 is being referred too, although I suspect the change is not strongly dependent on the state of the atmosphere.
7-1489	7	51	31			I did not understand this. GHG increases reduce precipitation so the sign of the change is opposite to the increase in precipitation due to GHG-induced warming. Perhaps this refers to the circulation response? A little more clarification woluld help. [Richard Allan, United Kingdom]	Yes, this appears to have been an orphaned paragraph; the 'half effect' really was meant to referred to the direct effect of CO2 on circulation. This paragraph has been removed, and the desired point is now stated as a subsidary point as it should The main 'direct effect' of CO2 is to reduce precipitaiton.
7-1490	7	51	31			Is the statement that as much as half of the change is directly related to the radiative effects of CO2 come from Figure 7-21? If so, this statement only seems applicable over the ocean. For ocean, it does indeed appear that ~4% out of the ~8% reduction in overturning occurs rapidly, but this is not the case over land or in the global mean. [Government of United States of America]	No, this was an orphaned paragraph. The 'half' effect meant to refer to the effect of CO2 on the circulation strength, this has now been clarified.
7-1491	7	51	32	51	32	It is not clear to me what is meant by the link between hydrological and cloud adjustments mentioned here, and it is not clear to me either that Wyant et al 2012 say anything about this. I would not wish the reader to take away the impression that there is evidence that hydrological cycle adjustments are caused primarily by cloud adjustments, or vice versa, although it is possible that they do affect each other to some small degree. But Lambert and Webb (2008) show that the hydrological cycle adjustment to CO2 is mostly due to clear-sky effects, and Colman and McAvaney (1997) and Wyant et al (2012) attribute cloud adjustments in the models they examine mechanisms which do not as far as I can tell relate to hydrological cycle adjustments. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	This pararaph has been removed, as discussed in the response to the previous comment this "half" effect was meant to rever to the effect of CO2 on circulation. With the revisions this has been clarified.
7-1492	7	51	36	7	42	This text seems to be misplaced. It would make more sense to put it after line 28, i.e., after the paragraph in	The reviewers suggestion has been followed.

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						which the reduction in the overturning rate is discussed. [Anthony Del Genio, United States of America]	
7-1493	7	51	36	7	42	This figure needs a more descriptive caption. It is very hard to understand what is plotted in this figure and how one uses it to draw certain conclusions. First, what is the x-axis supposed to be? I gather that it means different things for different runs, i.e., changing T vs. time as models with an abrupt CO2 quadrupling approach equilibrium, vs. instantaneous T at time of quadrupling for transient scenarios, vs. equilibrium T for runs with fixed SST. Correct or incorrect? How do I conclude from these plots that half the overturning change is felt before any warming takes place? It does not look that way to me at all. Are the points on this figure from a single model or a CMIP5 multi-model mean? [Anthony Del Genio, United States of America]	The caption has been considerably revised in light of this reviewers and other reviewers coments.
7-1494	7	51	37	51	42	Figure 7.21: explain in caption: DTs? Delta: between what and what? Ts is? [Andrea Flossmann, France]	This has been clarified in a revised caption
7-1495	7	51	37			Far too obscure – just what is this fractional measure? “Overturning” means both the going up & coming down, so how can it be defined for part of the circulation, e.g. land only or sea only? [William Ingram, United Kingdom]	The caption has been rewritten and the terms have been defined.
7-1496	7	51	38		39	This sentence is conspicuously untrue. I can't work out what was meant but it needs to be removed or replaced by something that makes sense & is true. [William Ingram, United Kingdom]	am not exactly sure what aspect of the sentence the reviewer found objectionable, but as pointed out by other reviewers the caption was confusing and has been rephrased, hopefully in a way that avoids the objectional interpretation of this reviewer.
7-1497	7	51	41			Not, not “illustrated” here”! If this is shown in the reference, indicate so correctly [William Ingram, United Kingdom]	Again the reviewers comment is a little obscure; I hope the considerably revised caption addresses the original concern.
7-1498	7	51	44	51	44	Delete "well-mixed" for generality. [J. Graham Cogley, Canada]	The word "atmosphere" was exchanged for "troposphere" but the word well mixed is retained, as for GHGs that are not well mixed the effect is less clear.
7-1499	7	51	44	51	51	The reason why an increase in greenhouse gas leads to reduced surface precipitaiton in the absense of a compensating temperature change is not well articualted in that paragph, and the logic flow there is hard to follow. [Minghuai Wang, United States of America]	This is hopefully addressed through the restructuring (removal of first, confusing paragraph) and also the introduction of section 7.6 which places more emphasis on the tropospheric cooling rate.
7-1500	7	51	45	51	45	…of the LOWER atmosphere. Increases in GHGs can increase the radiative cooling of the stratosphere. [Daniel Murphy, United States of America]	Atmosphere has been replaced by "troposphere"
7-1501	7	51	45			Omit “rate” as adding nothing [William Ingram, United Kingdom]	Done
7-1502	7	51	46	51	45	Should this not be the net radiative cooling rate of the troposphere ? [Robert Kandel, France]	Yes, it was noted by several other reviewers and has since been changed.
7-1503	7	51	49	50	53	I do not think that there is currently a consensus in the literature pointing to a dominant role for water vapour in controlling the dependence of the atmospheric radiative cooling and hence global precipitation on increasing surface temperature. I think it would be appropriate to also cite here other studies that show that increases in atmospheric radiative cooling with surface temperature are primarily due to the radiative effects of increasing atmospheric temperature itself (Mitchell et al 1987, Previdi 2010, O'Gorman et al 2012), although water vapour does make an important contribution, offsetting the temperature effect by up to 39%. Refs: Mitchell, J. F. B., Wilson, C. A. and Cunnington, W. M., 1987: On CO2 climate sensitivity and model dependence of results. Quarterly Journal of the Royal Meteorological Society, 113, 293-322, ;Previdi, M. (2010) Radiative feedbacks on global precipitation, Environ. Res. Lett., 5, 025211 ; O'Gorman, P. A., Allan, R. P., Byrne, M. P. & Previdi, M. 2012 Energetic constraints on precipitation under climate change, Surveys in Geophysics 33, 585-608 [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Partly taken into account. This has been reworded to avoid the impression that greater water vapour is what causes the increased cooling.
7-1504	7	51	53	51	54	In the sentence starting with “ The response of the hydrologic cycle….” The words “by aerosols” should be added right before “partly explains”. [Government of United States of America]	Aerosol effects were not the topic of this paragraph, they are dealt with in the subsequent paragraph.

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7-1505	7	51	54	51	57	"Increased solar forcing does not strongly affect the net atmospheric cooling….". This statement is only true in arid or semi-arid, convectively stable regions if the atmospheric heating by aerosols do not excite atmospheric water cycle feedback, involving latent heating. In moist, convectively unstable regions such as the Indian monsoon, atmospheric heating by aerosols can lead to rapid feedback in the atmospheric water cycle, with increase in moist static energy in the lower troposphere leading to increased precipitation (Wang et al. 2009). The authors should consider revising the text accordingly. [Government of United States of America]	This paragraph has been revised ot make its original point, relating to gaseous absorption, more clear. Aerosol effects are considered in the subsequent paragraph.
7-1506	7	51	57	51	57	Maybe I misread/misunderstand Takahashi, but I thought he is saying that the shortwave forcing offsets the longwave divergence by between 10 and 30%, which is "work" that the hydrological cycle doesnt need to do in rebalancing the energy budget - I agree it is uncertain, but I am not sure it is necessarily small. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Yes, 10% seemed small when I first wrote this. The statement has been modified, but the magnitude of the effect is difficult to assess from the Takahashi reference, which shows considerable scatter among models.
7-1507	7	51	57	52	2	This sentence about solar radiation management schemes appears to relate to geoengineering. It would be good to actually use the term 'geoengineering' here so that readers searching for anything related to that important topic can easily find it. It may be worth making the point here that SRM schemes, while reducing the surface temperature, would not result in an identical hydrological climate to that with lower GHG. [European Union]	Yes, but only geo-engineering methods based on solar radiation management. To make the connection clear, without loosing the precision of the original statement,the phrase "geo-engineering" has been added as a parenthetical.
7-1508	7	51		52		I recommend revising Section 7.6.3 to reflect sections on the hydrological cycle in the rest of the report – particularly, Chapter 12, Section 12.4.1 or Chapter 11. The work of Previdi, 2010, ERL, Volume 5, confirms that changes in clear sky atmospheric cooling at the top of atmosphere are not dominated by changes in water vapour as stated by Stephens and Hu, but by temperature, as found by earlier studies. [Francis Hugo Lambert, United Kingdom]	Yes, these points have been addressed right at the outset of section 7.6, and more coordination among the writers of the different sections is taking place.
7-1509	7	52	4	52	6	Citations of Lambert and Webb, 2008 and Previdi 2012 would be appropriate here for the effect of clouds on the changing hydrological cycle with the warming climate. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Taken into account. A reference to Lambert and Webb (2008) has been added. The second reference is not added as it does not add further insight.
7-1510	7	52	4	52	13	As noted in the comment above, Stephens and Ellis (2008) describe how high cloud radiative feedbacks affect changes in global average precipitation and how these feedbacks are induced by reductions in mid and low clouds. This is one of the first studies to call this out although the general notion of cloud radiation feedbacks on precipitation is outlined in my 2005 cloud feedback paper. [Graeme Stephens, United States of America]	considered but reference is not cited as this point is not explored in more depth, and the additional literature does not add further insights which we assess.
7-1511	7	52	4	52	15	Reading this paragraph, one isn't sure what the conclusion is regarding the role of absorbing aerosols in changing precipitation. A recent paper suggested that the effect of absorbing aerosols on the shortwave heating of the atmosphere, at least in terms of the CMIP3 models, provided a better explanation of the variations in precipitation among the models than did the changes in the longwave heating produced by increasing CO2 (Pendergrass and Hartmann, GRL, 39, L07817, doi:10.1029/2005GL025478, 2012). Again, I'm not sure that the results in this paper contradict the intended meaning of the paragraph. The paragraph needs to better clarify the relative roles of absorbing aerosols and LW atmospheric heating on precipitation. [James Coakley, United States of America]	Accepted. This study has been added to the text.
7-1512	7	52	8	52	8	Absorbing aerosols act more like CO2..., The mechanism should be different, the effects should be different on tempral and spatial scales, please do not only emphasis that to mislead the understanding. [Junying Sun, China]	This has been rephrased inlight of the reviewers comment
7-1513	7	52	8	52	9	The statement regarding "absorbing aerosols stabilizing the troposphere and reducing precipitation" is not necessarily true. Absorbing aerosol can increase precipitation. In dynamically active and moisture-rich regions such as the monsoon, shortwave heating by absorbing aerosols during the pre-monsoon period can induce latent-heating and circulation feedbacks, which overcome the semi-direct effect, leading to enhancement, and re-distribution of monsoon rainfall (Lau et al. 2006, 2010, Lau and Kim, 2006, Gautam et al. 2009). The authors should consider reivisng the text accordingly. [Government of United States of America]	In this section we focus primarily on the large-scale effects. However the role of aerosol particles on local circulation systems is now explicitly stated, and the interested reader is referred to Chapter 14 where these effects are discussed in further detail.
7-1514	7	52	8	52	9	"Absorbing aerosols act more like CO2…" is not most adequate because the strong cooling effect of this type of aerosols to the surface. I assume the discussed reduction of precipitation due to the stabilization of the	Yes this was an awkward and unecessary analogy that has been removed through the course of the

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						troposphere is referring to the local effect of aerosols? There are many discussions in recent literature including Wang (2007; GRL, 34, L05709) and Ming et al., (2010; GRL, 37, L13701) that have suggested different effects mostly occurring remote to the aerosol-laden regions and associated with impacts on large-scale circulation. [Chien Wang, United States of America]	revisions. The Ming et al. paper is cited and the interested reader is referred to Chapter 14 for a further discussion of these effects.
7-1515	7	52	9	52	9	The hydrological response to absorbing aerosols was explained fully based on the atmospheric energy balance (Ming et al., 2010). A vague attribution of the reduced precipitation to stabilization does not reflect the current understanding. The “hydrological forcing” concept discussed in Ming et al. (2010) is also worth mentioning. [Yi Ming, United States of America]	Ming et al., 2010 is referenced and the text has been rewritting to make the connections and explanations more clear.
7-1516	7	52	9			The authors should consider removing "...sensitively" from this sentence. Or the sentence could be changed to the effect that the impact of those drivers is sensitive to their height and distribution. [Government of United States of America]	The reviewers suggestion has been followed.
7-1517	7	52	11	52	11	Wang (2009, Annals Geophysicae, 27, 3705-3711) also discussed in detail that the precipitation changes attributed to black carbon emissions from different continents. [Chien Wang, United States of America]	A regional breakdown of effects is beyond the scope of this seciton and more appropriate for Chatper 14.
7-1518	7	52	15	52	39	Interesting issues that demonstrate the contribution of cloud condensation nuclei (CCN) in the sub-cloud (cloud-free) layer to the cloud cells aloft in the boundary layer, thereby influence the distribution of regional precipitation, have been reported in the literature. Such results need better understanding of physics and dynamics of low-altitude clouds. [Panuganti, C.S. Devara, India]	These and related ideas are introduced and discussed in section 7.4.
7-1519	7	52	15	52	56	Worthwhile to mention that the current generation of clean technology reduces the emission of sulphur and fine particulate matter, but leads to an unanticipated increase in the direct emission of ultrafine particles (1-10 nm median diameter) which are highly effective precursors of cloud condensation nuclei (CCN). These additional ultrafine particles might probably modify cloud microphysics, as well as precipitation intensity and distribution on a regional scale downwind of emission sources. See Junkermann et al, 2011, The climate penalty of clean fossil fuel combustion. Atm Chem Phys 11, 12917-12924 [European Union]	These points go beyond the scope of the section and have not been incorporated. They may be useful for discussions of mitgation measures.
7-1520	7	52	15	52	56	The following papers on aerosol impacts on different large-scale circulation regimes are highly relevant to the discussion here, and should be cited: Ming, Y., and V. Ramaswamy, 2011: A Model Investigation of Aerosol-induced Changes in Tropical Circulation. Journal of Climate, doi:10.1175/2011JCLI4108.1. Ming, Y., V. Ramaswamy, and G. Chen, 2011: A Model Investigation of Aerosol-induced Changes in Boreal Winter Extratropical Circulation. Journal of Climate, doi:10.1175/2011JCLI4111.1. Bollasina, M.A., Y. Ming, and V. Ramaswamy, 2011: Anthropogenic Aerosols and the Weakening of the South Asian Monsoon. Science, doi:10.1126/science.1204994. [Yi Ming, United States of America]	Ming et al 2010 is cited, the other studies are more relevant to the discussion in Chapter 14, which looks at regional circulation systems, and in particular the monsoons.
7-1521	7	52	17	52	17	The statement that aerosol-cloud interactions do not directly affect radiation is in conflict with the concept of "aci" that you define in Fig. 7.2. Surely the Twomey effect of aerosols on cloud particle size and thus albedo should be considered to be a way in whioch aerosol-cloud interactions directly affect radiation. And if I get more SW reflection as a result, I will get less OLR in equilibrium, implying less atmospheric radiative cooling, which must be balanced by less latent heating and thus less precipitation. And none of that has anything directly to do with how effects of aerosols on cloud microphysics influence the tendency of aerosols to either suppress or invigorate precip in individual cloud systems. [Anthony Del Genio, United States of America]	This confusing statement has been reformulated to make the intent more clear.
7-1522	7	52	17	52	18	Section 7.6.4:Regarding the phrase, "radiant streams of energy" it's not clear what this means...the word streams has too many possible meanings, in radiative transfer and otherwise, and should be avoided [Government of United States of America]	The introduction to the section has been reformulated and the offending phrase has been removed.
7-1523	7	52	17	52	18	(Noted by two reviewers) How is it possible that aerosol-cloud interactions do not directly influence radiation? Is the key word "directly"? Please clarify. [Government of United States of America]	This confusing statement has been reformulated to make the intent more clear.
7-1524	7	52	17	52	19	This statement is false. Aerosol-cloud interactions affect the energy balance substantially and hence can influence the global precipitation.The same previous statement about solar management applies to aerosol-cloud interactions. [Steven Ghan, United States of America]	Actually it is not necessarily false. The net cloud radiative forcing on the atmosphere is about zero. Changing TOA fluxes is not enough to change precipitation on the global average, rather TOA fluxes have to change in a way that is not compensated for by changes in surface fluxes, and this is less trivial.

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							Nonetheless the introductory material to this subsection has been considerably revised in light of the confusion it previously caused.
7-1525	7	52	17	52	19	"In contrast to aerosol-radiation interactions, aerosol-cloud interactions do not directly influence the radiant streams of energy that drive the climate system, and thus are not expected to have any influence on the global mean precipitation rate." The fact that the radiation effects of aerosol-cloud interactions may not be direct is irrelevant. Indirect radiation effects of cloud-aerosol will ultimately affect global precipitation. I therefore suggest that the above statement be revised. [Lazaros Oreopoulos, United States of America]	See the comments to the other reviewers on this point
7-1526	7	52	17	52	19	The statement "aerosol cloud interactions do not directly influence the radiant streams of energy that drive the climate system". This statement obviously contradicts the cloud albedo effect (Twomey effect). [Daniel Rosenfeld, Israel]	No, see the comments to earlier reviewers. Even so, the offencing phrase has been stricken.
7-1527	7	52	17	52	19	I am not an expert on aerosol-radiation interactions, but it seems to me that the Twomey effect would definitely be expected to change the albedo of clouds, and hence would be expected to affect the radiant streams of energy that drive the climate system. Surely the reason why the Twomey effect would not be expected to affect global precipitation is because it primarily affects atmospheric scattering and the fluxes at the top of atmosphere and surface, but by a similar amount, thus having only a small impact on the radiative heating of the atmosphere. This is a similar argument to the one underlying the explanation for absorbing aerosols having more of an effect on global precipitation than scattering aerosols, and to the argument for solar forcing causing a weaker precipitation adjustment than CO2 forcing. I think that the results of Grabowski 2006 support this. Ref: Grabowski, W. W (2006) : Indirect Impact of Atmospheric Aerosols in Idealized Simulations of Convective–Radiative Quasi Equilibrium, Journal of Climate 19, 4664-4682. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	This reviewer is more of an expert than he might imagine as he was one of the few to understand why it is difficult for ACI to impact precipitation globally. But as indicated to the other reviewers this description has been reworked to avoid confusion on the part of other reviewers.
7-1528	7	52	17	52	20	This comment is related to how "aerosol-cloud interactions" are defined here, and how this differs from RFaci and AFaci. I understand what the authors mean – that global mean precipitation rate can only be affected by energetic constraints, and can't be changed directly by microphysics itself. But the way this paragraph is worded is confusing, since it is stated that aerosol-cloud interactions "do not directly influence" radiant energy. But of course aerosol-cloud interactions affect radiation through RFaci and AFaci, which in turn could affect global mean precipitation at least in principle. This is especially confusing because in other places in the chapter "aerosol-cloud interactions" also refers to the subsequent effects on radiation (e.g., Figure 7.2). Thus, I think "aerosol-cloud interactions" needs to be more clearly defined here to strictly mean microphysical changes resulting from aerosols, excluding any subsequent impacts on radiation. I would suggest using terminology other than "aerosol-cloud interactions" to describe this, to avoid confusion with other parts of the text. [Hugh Morrison, United States]	The reviewers comments are well taken, and based on these and other comments the introductory statements for this subsection have been rephrased.
7-1529	7	52	17	52	56	The whole notion that invigoration can induce a positive radiative forcing is absent from this section and from Chapter 7 in general. There are both observational and simulation evidence that this effect can be substantial: Koren I, Remer LA, Altaratz O, et al., 2010b: Aerosol-induced changes of convective cloud anvils produce strong climate warming. Atmos Chem Phys 10: 5001–5010 Fan, J., D. Rosenfeld, Y. Ding, L. R. Leung, and Z. Li, 2012: Potential aerosol indirect effects on atmospheric circulation and radiative forcing through deep convection. Geophys. Res. Lett., doi:10.1029/2012GL051851. [Daniel Rosenfeld, Israel]	The relevant forcing for precipitation is not at the TOA, but rather through the net forcing of the atmosphere. A TOA forcing effects precipititaiton through warming, or cooling, but these effects are discussed elsewhere.
7-1530	7	52	17		19	This sentence is obvious nonsense. I can't work out what was meant but it needs to be removed or replaced by something that makes sense & is true. [William Ingram, United Kingdom]	See related comments to other reviewers.
7-1531	7	52	18	52	18	aci do impact on surface temperature and so do influence the global mean precipitation rate. I didnt understand this sentence [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	On the global average it is the net atmospheric heating rate which is critical.
7-1532	7	52	20			Please clarify how precipitation is distributed. in space? time? something else? [Government of United States of America]	Both were intended, the word 'spatio-temporal' has been added before the first use of the word distribution.

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7-1533	7	52	22	52	28	The whole concept of invigoration is misrepresented. First, the highly cited basic references that explain the concept in Science Magazine is absent: Andreae, M. O., D. Rosenfeld, P. Artaxo, A. A. Costa, G. P. Frank, K. M. Longo, and M. A. F. Silva-Dias, 2004: Smoking rain clouds over the Amazon. Science, 303, 1337-1342. Rosenfeld D., U. Lohmann, G.B. Raga, C.D. O'Dowd, M. Kulmala, S. Fuzzi, A. Reissell, M.O. Andreae, 2008: Flood or Drought: How Do Aerosols Affect Precipitation? Science, 321, 1309-1313.). [Daniel Rosenfeld, Israel]	Both of these seminar references have been added so as to give a clearer picture of the ongoing invigoraiton discussion within the literature.
7-1534	7	52	24	52	24	after "poorly understood" add "As a result, the climatic significance of aerosol-cloud interactions has been poorly known and quantified. To overcome this, Li et al. (2011b) analyzed 10 years of extensive and high-quality data from the US Dept of Energy's Atmospheric Radiation Measurement (ARM). They found strong but conditional relationships between aerosol number concentration, cloud top height and precipitation frequency for deep convective clouds, but much weaker relationships for warm clouds. The long-term ground-based finding is consistent with the analysis of global satellite measurements (Niu and Li, 2012). Such findings support the theory of aerosol invigoration effect (Andrea et al. 2004, Rosenfeld et al. 2008) with signficant implicatins for climate studies." [Zhanqing Li, United States of America]	A reference to the Li study has been added, but the study itself is not developed in more detail than the studies which are equally comprehensive, but show little to now effect, e.g., the Seifert study based on a complex microphysical scheme and nearly 4000 simulated days constrained by observations in an NWP setting.
7-1535	7	52	28	52	31	Key observations that are not subjected to the difficulties that are eluded to here are omitted. These evidence include a 10-year climatology of measurements at the SGP site showing that invigoration occurs for warm cloud base when surface measured aerosols are high: 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 precipitation. Nature Geoscience, 2011, doi:10.1038/ngeo1313. Additional key reference shows that there is invigoration over large tropical ocean areas when affected by volcanic emissions: Yuan T., L. A. Remer, K. E. Pickering, and H. Yu, 2011: Observational evidence of aerosol enhancement of lightning activity and convective invigoration. GRL, 38, L04701, doi:10.1029/2010GL046052. Yet another key reference shows a weekly cycle of invigoration of convection in the southeast US during summer, along with the weekly cycle of anthropogenic aerosols: Bell, T. L., D. Rosenfeld, K.-M. Kim, et al. (2008), Midweek increase in U.S. summer rain and storm heights suggests air pollution invigorates rainstorms. J Geophys Res 113, D02209, doi:10.1029/2007JD008623. [Daniel Rosenfeld, Israel]	It is our assessment that these studies share many of the difficulties alluded to, for instance Boucher and Quass point out the important role of lower mid-tropospheric water vapor, which is not adequately controled for in these other studies. In the end we try to strike a balance by presenting some of the relevant studies, but not going into depth in what is a contradictory literature for which no consensus has developed and understanding remains low.
7-1536	7	52	29	52	29	Should cite Li et al., 201: Li, Z., F. Niu, J. Fan, Y. Liu, D. Rosenfeld, and Y. Ding (2011), The long-term impacts of aerosols on the vertical development of clouds and precipitation, Nat. Geosci., 4, 888–894, doi:10.1038/ ngeo1313. [Steven Ghan, United States of America]	This study has now been added.
7-1537	7	52	33	52	39	It is stated in lines 33-34 that "the degree of invigoration depends sensitively on a number of factors, including the representation of cloud and aerosol microphysics". Then, the text states that simulations at climate scale find little to negligible invigoration. But the climate models have a very poor representation of cloud and aerosol microphysics. Despite this fact, the text is written in a way that provides the models more credibility than the observations, which were omitted from here at the first place (see my comment with respect to lines 28-31 of this page). The whole section should be rewritten with a more balanced view. [Daniel Rosenfeld, Israel]	Here the original statement referred to cloud resolving models run in a climate context, i.e., in radiative convective equilibrium. The text has however been modified to make this explicit and avoid possible confusion on the part of other readers.
7-1538	7	52	38	52	39	The text states: "…convective intensity is also limited by energetic constraints". What does this statement mean? It needs not to be reminded that everything must obey the laws of physics. It seems to me that the statement implies that the concept of invigoration violates the energy conservation. But Rosenfeld et al. (Science, 2008) have clearly showed how aerosols can induce invigoration within the same energy constraints. The claim that invigoration would be buffered under new convective radiative equilibrium that includes it does not allow for transient effects of aerosols, which are at the scale of the lifecycle of the aerosols, that is shorter than the time required for reaching the new radiative convective equilibrium. Therefore, discarding the importance of invigoration is unwarranted. [Daniel Rosenfeld, Israel]	This has been rephrased to make the limitations of gross moist stability more explicit.
7-1539	7	52	39	52	39	At the end of this paragraph, add "In light of the conditional dependence of the invigoration effect, however, it can become significant for certain climate or cloud regimes such as deep clouds of low cloud base and high cloud top as revealed in Li et al. (2011b) and Niu and Li (2012). [Zhanqing Li, United States of America]	This is what is meant by conditional.

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7-1540	7	52	41	52	48	The literature reports a noval study related to cloud dissipation followed by drizzle based on increased local atmospheric stability resulting in delay in the production of fresh CCN (Rechaging of the Atmosphere) and associated cloud formation after wet removal. The results also reveal that the turbulent mixing induced by the wind-shear over the experimental site associated with complex terrain, plays important role in the formation and evolution of boundary layer clouds. [Panuganti, C.S. Devara, India]	Because this is an assessment our review of the literature is necessarily selective around those studies contributing to the main ideas being assessed, which in this case is assessment. The reviewers comments might fit better in Section 7.4, but even here it is not clear that the cited study is crucial to the assessment.
7-1541	7	52	46	52	47	The text states: "Even in cases where effects are reported, the interpretation of the data remains controversial". But the results of Jirak and Cotton (2006) were not challenged. Also other prominent studies that were not challenged are omitted from here, such as: Rosenfeld, D., J. Dai, X. Yu, Z. Yao, X. Xu, X. Yang, C. Du, 2007: Inverse relations between amounts of air pollution and orographic precipitation. Science, 315, 9 March 2007, 1396-1398; Rosenfeld D., Givati, A, 2006. Evidence of orographic precipitation suppression by air pollution induced aerosols in the western U.S. J. Applied Meteorology and Climatology, 45, 893-911 Yang X., M. Ferrat, Z. Li, 2012: New evidence of orographic precipitation suppression by aerosols in central China. Meteorol Atmos Phys, DOI 10.1007/s00703-012-0221-9. This section should be rewritten with a more balanced view. [Daniel Rosenfeld, Israel]	To addresse the reviewers point we have more fully presented the literature and ideas related to aerosol invigoration in our revised assessment, in an attempt to address the issue of balance raised by this reviewer.
7-1542	7	52	50	52	56	This summary paragraph to the section of "cloud-aerosol interaction" is extremely biased towards being a contrarian to the existence of significant cloud aerosol interactions. This is evident by quoting all the studies that question this, while omitting the studies that provide the strongest observational evidence, both here and in the whole section. This highly biased attitude is culminated by the reference of Yuter et al. which is still in a submitted state, and unlikely to be accepted without a major revision, while omitting the reference to the already published and highly cited paper that Yuter et al. are commenting on. Again, this whole section requires a major revision and rewrite by a person who has no prejudice to the subject of this section. [Daniel Rosenfeld, Israel]	We have revised our assessment to give a more balanced view and to indicate that the low level of confidence in the robustness of a systematic invigoration/intensificaiton effect.
7-1543	7	52	53	52	53	The way this sentence is written is sounds as if the specific phenomenon is exclusive to Germany. [Lazaros Oreopoulos, United States of America]	This has been revised to address the reviewers comment and present the more general nature of the result.
7-1544	7	52	53	52	54	Concerning "no systematic effect of aerosol-cloud interactions on precipitation is evident (Seifert et al., 2012)." I suggest to cite the another idea in the paper, "Long-term impacts of aerosols on the vertical development of clouds and precipitation", pubished in Nature-geoscience Nov.13, 2011 by Li et al. The analysis focused on a long-term impact of aerosols on the vertical development of clouds and rainfall frequencies, using a 10-year dataset of aerosol, cloud and meteorological variables collected in the Southern Great Plains in the United States. The results show that cloud-top height and thickness increase with aerosol concentration measured near the ground in mixed-phase clouds. The authors attribute the effect to an aerosol-induced invigoration of upward winds. [Junying Sun, China]	The Li study is now cited, along with several additional papers supporting or developing the idea of an invigoration effect.
7-1545	7	52	53	52	54	Concerning " no systematic effect of aerosol-cloud interactions on precipitation is evident (Seifert et al., 2012)." I suggest to cite the another idea in the paper, "Long-term impacts of aerosols on the vertical development of clouds and precipitation", pubished in Nature-geoscience Nov.13, 2011 by Li et al. The analysis focused on a long-term impact of aerosols on the vertical development of clouds and rainfall frequencies, using a 10-year dataset of aerosol, cloud and meteorological variables collected in the Southern Great Plains in the United States. The results show that cloud-top height and thickness increase with aerosol concentration measured near the ground in mixed-phase clouds. The authors attribute the effect to an aerosol-induced invigoration of upward winds. [Bin Zhu, China]	The Li study is now cited, along with several additional papers supporting or developing the idea of an invigoration effect.
7-1546	7	52	54	52	55	The weekly variation of some aerosol properties is apparent. The studies have been concentrated on PM or aerosol optical properties, not CCN. This means that their applicability to determine weekend effects related to indirect cloud effects is quite limited. As CCN sized particles (Aitken/accumulation modes, sizes smaller than about 500 nm) have typically longer lifetimes as PM, and the weekly variation is quite close to these lifetimes, this generalization might not hold everywhere. [Ari Asmi, Finland]	The reviewers point is well taken. In the revised text we have tried to establish that the weekend effect is controlversial, even if one takes the data at face values. The reviewer gives another reason to question such an effect, but to develop this idea would require us to got deeper into the subject, something

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							which the evidence and our low confidence in the ideas does not warrant.
7-1547	7	52	54	52	56	This isn't my area, so I'm just wondering here, but e.g., papers by Bell and Rosenfeld (GRL 2008) and Bell et al. (JGR 2008) seem to argue for weekly cycles of aerosol-mediated precipitation, and several papers, e.g., Koren et al. (ACP 2010), seem to support the invigoration hypothesis -- maybe not exactly "storm intensity" -- I'm not sure. So should these effects be presented as controversial, or just dismissed? [Ralph Kahn, United States of America]	These studies were referred to in the original draft, but only indirectly. We have revised the text to make the reference to the weakly cycle more explicit.
7-1548	7	52	55	52	55	Concerning "a robust effect on precipitation or storm intensity has proven difficult to detect",this reference was suggested as, "Intensification of Pacific storm track linked to Asian pollution",published in PNAS 2007 by Renyi Zhang et al. the paper shows a trend of increasing deep convective clouds over the Pacific Ocean in winter from long-term satellite cloud measurements (1984–2005). The intensified Pacific storm track represents possibly detected climate signal of the aerosol–cloud interaction associated with anthropogenic pollution. [Junying Sun, China]	The reviewers point is well taken,and a number of additional studies have been cited to present a fuller picture of the possible effect of the aerosol on storm intensity. But the reviewer is reminded that we are writing an assessment, not a review.
7-1549	7	52	55	52	55	Concerning " a robust effect on precipitation or storm intensity has proven difficult to detect", I suggest cite the paper, "Intensification of Pacific storm track linked to Asian pollution",published in PNAS 2007 by Renyi Zhang et al. the paper shows a trend of increasing deep convective clouds over the Pacific Ocean in winter from long-term satellite cloud measurements (1984–2005). The intensified Pacific storm track represents possibly detected climate signal of the aerosol–cloud interaction associated with anthropogenic pollution. [Bin Zhu, China]	The reviewers point is well taken,and a number of additional studies have been cited to present a fuller picture of the possible effect of the aerosol on storm intensity. But the reviewer is reminded that we are writing an assessment, not a review.
7-1550	7	52	56			A place to put a discussion of rainfall in India and China??? [Daniel Murphy, United States of America]	This is covered in the discussion of the Monsoon (Chpt 14) and the reader is now referred to this seciton.
7-1551	7	53	1	53	33	The following paper discussed how aerosols may affect extreme rainfall, and should be cited: Chen, G., Y. Ming, N. Singer, and J. Lu, 2010: Aerosol-induced Changes in Mean and Extreme Precipitation. Geophysical Research Letter, 38, doi:10.1029/2010GL046435. [Yi Ming, United States of America]	To address the spirit of this comment a number of additional studies have been cited to present a fuller picture of the possible effect of the aerosol on storm intensity. But the reviewer is reminded that we are writing an assessment, not a review.
7-1552	7	53	1	53	33	All of section of 7.6.5: The Physical Basis for Changes in Precipitation Extremes: The whole major subject of potential aerosols impact on invigorating hailstorms and tornados and weakening tropical cyclones is completely absent from this section and from the whole Section 7. Observations references for aerosols enhancing hail and/or tornadoes are: Andreae, M. O., D. Rosenfeld, P. Artaxo, A. A. Costa, G. P. Frank, K. M. Longo, and M. A. F. Silva-Dias (2004), Smoking rain clouds over the Amazon,Science, 303, 1337–1342, doi:10.1126/science.1092779. Wang, J., S. C. van den Heever, and J. S. Reid (2009), A conceptual model for the link between Central American biomass burning aerosols and severe weather over the south central United States,Environ. Res. Lett., 015003, doi:10.1088/1748-9326/4/1/015003. Rosenfeld, D., and T. L. Bell (2011), Why do tornados and hailstorms rest on weekends?, J. Geophys. Res., 116, D20211, doi:10.1029/2011JD016214. Simulation references for aerosols enhancing hail and tornadoes are: Lerach, D. G., B. J. Gaudet, and W. R. Cotton (2008), Idealized simulations of aerosol influences on tornadogenesis,Geophys. Res. Lett.,35, L23806, doi:10.1029/2008GL035617 Storer, R. L., S. C. van den Heever, and G. L. Stephens (2010), Modeling aerosol impacts on convective storms in different environments,J. Atmos. Sci., 67, 3904–3915, doi:10.1175/2010JAS3363.1. Khain, A., D. Rosenfeld, A. Pokrovsky, U. Blahak, and A. Ryzhkov (2011), The role of CCN in precipitation and	This previous section discusses the effect of aerosol changes on precipitation intensity, and hence extreemes. This is clearly stated in the scoping of the first paragraph. Some of the seminal studies mentioned by the reviewer are cited in this section. The effect of aerosols on tropical cyclones is discussed in chapter 14.

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						hail in a mid-latitude storm as seen in simulations using a spectral (bin) microphysics model in a 2D dynamic frame,Atmos. Res., 99, 129–146, doi:10.1016/j.atmosres. 2010.09.015. Observational references for aerosols weakening tropical cyclones are: Dunion, J. P., and C. S. Velden, 2004: The impact of the Saharan air layer on Atlantic tropical cyclone activ-ity. Bull. Amer. Meteor. Soc., 85,353–365. Rosenfeld D., M. Clavner and R. Nirel, 2011: Pollution and dust aerosols modulating tropical cyclones intensities. Atmospheric Research, 102, 66-76. Jenkins, G. S., A. S. Pratt, and A. Heymsfield, 2008: Possible linkages between Saharan dust and tropical cyclone rain band invigoration in the eastern Atlantic during NAMMA-06. Geophys. Res. Lett., 35,L08815, doi:10.1029/2008GL034072. Rosenfeld D., W.L. Woodley, A. Khain, W.R. Cotton, G. Carrió, I. Ginis, J.H. Golden, 2012: Aerosol effects on Microstructure and Intensity of Tropical Cyclones. Bul. Amer. Meteor. Soc., 2012, 987-1001. Simulation references for aerosols weakening tropical cyclones are: Khain A., and B. Lynn, 2011: Simulation of tropical cy-clones using a mesoscale model with spectral bin microphysics. Recent Hurricane Research—Climate, Dynamics, and Societal Impacts, A. R. Lupo, Eds., Intech, 197–227. Khain A., B. Lynn, and J. Dudhia, 2010: Aerosol effects on intensity of landfalling hurricanes as seen from simulations with the WRF model with spectral bin microphysics. J. Atmos. Sci., 67,365–384. Rosenfeld D., A. Khain, B. Lynn, and W. L. Woodley, 2007: Simulation of hurricane response to suppression of warm rain by sub-micron aerosols. Atmos. Chem. Phys., 7,3411–3424. Carrió, G. G., and W. R. Cotton, 2011: Investigations of aerosol impacts on hurricanes: Virtual seeding flights. Atmos. Chem. Phys., 11,2557–2567. Cotton, W.R., H. Zhang, G. M. McFarquhar, and S. M. Saleeby, 2007: Should we consider polluting hurricanes to reduce their intensity? J. Wea. Modif., 39,70–73. [Daniel Rosenfeld, Israel]	
7-1553	7	53	3	53	4	The previous section (Section 7.6.4) does not appear to discuss the physical basis for the aerosol effect on precipitation extremes. It merely describes certain ways aerosol may affect individual deep convection. It is more appropriate to say that a warming climate favors precipitation extremes due to dynamical feedback, and that regional climate forcing such as aerosol-water cycle interaction could provide strong local and regional dynamical feedback to amplify or mitigate the extreme. [Government of United States of America]	It is not clear what specifically the reviwer has in mind. Section 7.6.4 has been modified to more explicitly describe one of the promposed mechanisms by which aerosol particles may lead to an invigoration of convective storms, and numerous pointers have been added to the discusion in Chapter 14 of regional precipitation systems.
7-1554	7	53	7	53	17	The arguments advanced in this paragraph hold for oceanic storms and presumably storms in coastal regions. There is, however, some observational evidence (section 2.5.5) that surface humidity at low levels over land may not be increasing as much with temperature as constant relative humidity arguments would indicate. Moreover, the Executive Summary of Chapter 12 states that a "projected differential warming of land and ocean promotes changes in atmospheric circulation and resulting moisture transport that lead to decreases in near surface relative humidity over most land areas". [Adrian Simmons, United Kingdom]	Yes,was noted in our cross chapter discussions, and this point is now made and the reader is referred to chatper 2 and 12 for further consideration of these points.
7-1555	7	53	7			Not true as written – replace “individual” with e.g. “the most vigorous” [William Ingram, United Kingdom]	The reviewers suggestion has been adopted.
7-1556	7	53	10			“, where the air is warmer,” clearer as “on warming” [William Ingram, United Kingdom]	Changed to 'with warming' this might be a britsh/american english distinction.
7-1557	7	53	12			individual storms are expected to become more intense: would be good to support this with some references. [Andrea Flossmann, France]	Taken into account. This sentence was supported by several references on page 53, lines 7-8 of the SOD, which are retained in the final draft. As this sentence on line 11-12 duplicates an earlier sentence, it has been removed from the final draft.

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7-1558	7	53	27	53	38	Please add possible side effects as for (distribution of) precipitation (see e.g. results of Hauke Schmidt/ MPI in the FP7 project IMPLICC). [Government of Germany]	This is a geomip study which looks at the effect of solar radiation management on precipitation. It is not clear how ti relates to the arguments being developed in this section.
7-1559	7	53	28			Regarding the phrase, "precipitation extremes will increase": does this mean intensity (in terms of rain rates) or frequency of occurrence? [Government of United States of America]	This has been rephrased to make the link to intensity more explicit.
7-1560	7	53	29			And location & season [William Ingram, United Kingdom]	The reviewers suggestion has been adopted.
7-1561	7	53	31	53	31	Insert a comma after AR4. [Steven Ghan, United States of America]	Editorial, accepted.
7-1562	7	53	35	58	17	The section on SRM presents a number of studies suggesting that there is a broad knowledge on GE in the scientific community. This is, however, not the case, there are large uncertainties related to the individual techniques and their impacts and risks. Please add a para on uncertainty and mention the general lack of knowledge and need of research. [Government of Germany]	Accepted. The SOD text did include information on uncertainties, impacts and risks. More text has been added. IPCC does not make statements on the need for more research though.
7-1563	7	53	35	58	17	The section has improved considerably, and it appears that most of my comments from the first order draft review were taken into account adequately. [Mark Lawrence, Germany]	Noted
7-1564	7	53	35			In the Section 7.7 there is no information about space-based reflectors which theoretically possess unlimited forcing potential. One can say that such reflectors have no connection to Chapter 7 “Clouds and Aerosols”. But 7.7.2.3 “Surface Albedo Changes” has also no connection to Chapter 7 (and this method has negligible forcing potential). [Government of Russian Federation]	Accepted. The introductory paragraph of 7.7.2 now discusses space based methods. See also 7.8.3.1
7-1565	7	53	39	53	39	The Glossary does not refer the "large-scale" interventions. Please harmonize by changing glossary. [Government of Germany]	Accepted. We have harmonized the text describing geoengineering between the section, the glossary, and the FAQ
7-1566	7	53	39	53	39	The wording "Geo-engineering is the deliberate..." suggests that GE is an existing engineering technique. This is not the case and the sentence should be modified, e.g. "The term GE describes the idea of deliberate large-scale..." [Government of Germany]	Taken into account. We have harmonized the text describng geoengineering between the section, the glossary, and the FAQ
7-1567	7	53	39	53	39	Only the term "geoengineering" is used, without any reference to the fact that "climate engineering" is the preferred term by much of the community, including in some of the major assessments (e.g., from the German Ministry for Education and Research). "Climate engineering" is noted in the FAQ, but would be important to also mention here at the beginning of sectino 7.7.1. Furthermore, there is still an inconstency with Chapter 6, which still introduces the term "climate intervention" along with geoengineering (but also does not refer to "climate engineering"); coordination between these chapters, and also presumably with the other WGs, is really needed. [Mark Lawrence, Germany]	Taken into account. We have harmonized the text describng geoengineering between the section, the glossary, and the FAQ, and do mention the alternative term "climate engineering".
7-1568	7	53	39	53	39	The definition of geoengineering provided states "…to counter undesirable impacts of climate change…” yet this is different to both the citations provided and the content of the chapter. The most heavily cited definition in the geoengineering literature is by the Royal Society (2009) which states "...the deliberate large-scale intervention in the Earth’s climate system, in order to moderate global warming” whilst a similarly well cited Keith (2000) states "…to reduce undesired anthropogenic climate change”. In the content of the chapter there is an implicit focus, accurately reflecting the literature that the purpose of SRM is to lower global mean temperature (exceptions include Caldeira & Wood 2008 doi: 10.1098/rsta.2008.0132 and Robock et al 2008 doi: 10.1029/2008JD010050 which a focus on Arctic temperatures). Lowering global mean temperatures is not the same as countering undesirable impacts of climate change. For example, as shown in modelling work by Irvine et al 2012 (doi: 10.1038/nclimate1351), greater levels of SRM interventions are required to counter sea level rise than to counteract temperature increases. [Naomi Vaughan, United Kingdom]	Taken into account. We have harmonized the text describng geoengineering between the section, the glossary, and the FAQ. The definition now mention the "alleviation of impacts of climate change".
7-1569	7	53	39	53	52	As in the summary at the beginning of chapter 7 I’m missing clear hints on the uncertainties, risks and caveats of climate engineering methods like SRM. To my mind it is not sufficient here to just shortly refer to chapters were other aspects of climate engineering are discussed. I think a majority of aerosol, cloud and climate researchers is still very skeptic about climate engineering which should be reflected somehow in this chapter. [Ottmar Möhler, Germany]	Accepted. The introductory paragraph has been revised to note the uncertainties and risks more more clearly.

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7-1570	7	53	40	53	40	Is it OK to refer to a report ? [Jan Fuglestvedt, Norway]	Noted. We have consulted with the TSU. It is OK. Moreover the report was peer-reviewed.
7-1571	7	53	40			Add the German governmental report Rickels, W., G. Klepper, J. Dovern, G. Betz, N. Brachatzek, S. Cacean, K. Güssow, J. Heintzenberg, S. Hiller, C. Hoose, T. Leisner, A. Oschlies, U. Platt, A. Proelß, O. Renn, S. Schäfer and Z.r. M., Large-Scale Intentional Interventions into the Climate System? Assessing the Climate Engineering Debate. Scoping report conducted on behalf of the German Federal Ministry of Education and Research (BMBF). Kiel Earth Institute, Kiel (2011), 170 pp. [Jost Heintzenberg, Germany]	Rejected. The reviewer has not identified a rationale for its inclusion. We are not aiming to a comprehensive review, but rather an assessment.
7-1572	7	53	43	53	44	This is a rather unknown technique of SRM which is also not discussed in the following paragraphs. Please check the sentence. [Government of Germany]	Taken into account. The sentence has been slightly revised, but space precludes a discussion of the concept in the introductory paragraph, It is discussed in more detail in section 7.7.2.4
7-1573	7	53	43	53	44	"by altering the cloudiness" seems to allude to the method of aerosol injection to the stratosphe. Describing this method with "cloudiness" as a quasi-natural effect is not adequate. Please delete or change "by large scale injection of aerosols into the stratosphere" [Government of Germany]	Noted. See response to 7-1572.
7-1574	7	53	44	53	45	We propose to replace "carbon dioxide reduction" by "carbon dioxide removal". [Government of Germany]	Accepted.
7-1575	7	53	44	53	45	The most common term is Carbon Dioxide Removal not "Reduction". [Government of Germany]	Accepted
7-1576	7	53	44	53	45	It is better to say “Carbon Dioxide Removal” instead of “Carbon Dioxide Reduction”. In this case we think about just removal of greenhouse gases FROM THE ATMOSPHERE. [Government of Russian Federation]	Accepted
7-1577	7	53	44	53	45	In Chapter 6 Section 6.5.1 CDR stands for Carbon Dioxide Removal, in this line it is called Carbon Dioxide Reduction. Consistent terminology between chapters would be preferable. [Naomi Vaughan, United Kingdom]	Accepted
7-1578	7	53	44			“Altering the high-level cloudiness” is only one of several possibilities. Later on “Altering the low-level cloudiness” is discussed in 7.7.2.2. [Government of Russian Federation]	Noted. The chapter discusses both altering low level cloudiness and high level cloudiness to change the planetary energy budget. We have tried to clarify this..
7-1579	7	53	47	53	49	“Here we assess…” Presumably the SRM methods discussed herein should involve clouds and aerosols which are the subject of this chapter. It is therefore surprising that section 7.7.2.3 that discusses surface albedo changes with no cloud or aerosol link is included in this chapter. This subsection (7.7.2.3) should be moved. [Lazaros Oreopoulos, United States of America]	Rejected. All SRM methods are assessed in one location.
7-1580	7	53	49	53	51	The sentence "It should be noted that no technology for SRM has been fully developed and can be considered ready for large-scale deployment" , Should be included in the executive summery. [Government of NORWAY]	Accepted. A similar sentence as been added stating this.
7-1581	7	53	50	53	50	Especially in the introduction to the SRM-section it should be emphasized not only that there is no SRM-technology which has been fully developed, but also that the basic understanding of the related processes and their potential effects and risks is very limited, and that all statements about SRM are associated with extremely low confidence. [Government of Germany]	Noted. We have added some text noting the limitations of current understanding of Geoengineering. More information is provided in the synthesis section.
7-1582	7	53	50	53	50	The expression "not technology has been fully developed" is not appropriate, given the fact, that there is no GE-technique that would be suitable or even possible to use given the associated risks. The current text suggests that GE would be an option in the future which has not yet been assessed. [Government of Germany]	Rejected. The text does not suggest that SRM techiques are suitable or possible. SRM is an option that is being considered. Its assessment is the point of this section.
7-1583	7	53		57		In FAQ 7.3 the differential effects of Solar Radiation Management (SRM) on day and night time temperatures are mentioned and it is stated that SRM does not help to reduce the night time temperatures. This is not discussed at all in the section on SRM and should be for completeness. [European Union]	Accepted. A sentence has been added.
7-1584	7	54	1		38	(particularly 10-25) The important point needs to be made that the effect of liquid aerosols does not scale well because the more of the liquid, the larger the drops & so proportionally the less the surface area to scatter (& eventually the fallspeed becomes significant). I appreciate this is not new work since AR4, but neither is the 1st sentence on the page: both are necessary background. As far as I know, we don't really know how much	Rejected. There is already text on this on page 54 lines 10-25 (SOD version).

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						clumping (at least in the delivery process) might pose related problems for solid aerosol, which should perhaps be acknowledged. [William Ingram, United Kingdom]	
7-1585	7	54	2	54	4	"Some proposed SRM techniques increase stratospheric aerosol in a similiar way to explosive volcanic eruptions" suggests that there is conclusive proof of the effectiveness of SRM techniques. This is not clear yet. Please reformulate. [Government of Germany]	accepted. Sentence revised.
7-1586	7	54	2			Here or at some other place Hegerl, G.C. and S. Solomon, Risks of Climate Engineering, Science 235(2009), pp. 955–956 should be cited who reported that the effects of the Pinatubo eruption on the global water cycle demonstrate the negative side effects that can be expected from this type of SRM. [Jost Heintzenberg, Germany]	Rejected. That study cited the Trenberth and Dai paper that is already cited.
7-1587	7	54	4	54	5	This needs re-phrasing as it could be interpreted as saying that research to date has [been] "injecting sulphur-containing gases into the stratosphere" which isn't the intention. [Government of United Kingdom of Great Britain & Northern Ireland]	accepted. We now indicate that the studies are simulations of consequence to the injection of sulfur containing species.
7-1588	7	54	4	54	5	This sentence requires a full stop rather than a comma at the end. Also, references for this statement are absent and would be appropriate. For example Rasch et al (2008) An overview of geoengineering of climate using stratospheric sulphate aerosols. Phil Trans Roy Soc A 366:4007 doi: 10.1098/rsta.2008.0131 [Naomi Vaughan, United Kingdom]	Accepted.
7-1589	7	54	5	54	5	"...stratosphere, Other…" => "...stratosphere. Other…" [Mark Lawrence, Germany]	Accepted.
7-1590	7	54	10	54	10	"The RF from stratospheric aerosols will depend on the injection strategy..." suggests the next step to be implementation rather than research. Please reformulate. [Government of Germany]	Accepted. Changed "will" to "would"
7-1591	7	54	10	54	25	You might mention here that the *lifetime* of particles in the stratosphere is on order of a year, so whatever injection strategy is adopted, it would have to be continuously or periodically renewed, which would have both financial and environmental implications. [Ralph Kahn, United States of America]	Accepted. Renewal/replenishment is now mentioned, and there is increasing attention to consequences throughout the section. Cost is not assessed in this section.
7-1592	7	54	11	54	11	Typo. "The injection strategy affects…" (not "effects") [Ralph Kahn, United States of America]	Accepted.
7-1593	7	54	11	54	11	Replace "effects" by "affects" [Jón Egill Kristjánsson, Norway]	Accepted.
7-1594	7	54	11			"effects" → "affects" [William Ingram, United Kingdom]	Accepted.
7-1595	7	54	14	54	17	I think this may be a bit cryptic for some readers. Perhaps you need to explictly contrast the atmospheric lifetimes of CO2 and the aerosols to explain [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Rejected. This comment may refer to page 58. It is not so much about the differences in atmospheric lifetimes, which is mentioned earlier, but what matters here is the characteristic response time of the climate system. The text has been modified.
7-1596	7	54	15	54	15	deposition of vapour on pre-existing particles, change to condensation, just like others. [Junying Sun, China]	Rejected. In the stratospheric aerosol community the process is generally refered to as "vapor deposition"
7-1597	7	54	16	54	16	"…and sedimentation that changes as…" => "…and sedimentation, all of which change as…" [Mark Lawrence, Germany]	Accepted. Sentence has been rephrased.
7-1598	7	54	22	54	22	The inclusion of the delivery mechanism suggested in Pierce et al (2010) "from an aircraft" seems unnecessary given the lack of any other discussion relating to delivery mechanisms in this section and the statement on page 53 lines 46 to 51 that implementation, cost, economics and technological feasibility are not under discussion in this section. [Naomi Vaughan, United Kingdom]	Noted The injection method makes a qualitative and quantitative difference in the efficacy of the strategy, and there were requests for quantitative estimates of the amount of precursor needed. The sentences have been substantially revised.
7-1599	7	54	23	54	23	Maybe put the 10 MtS/yr in context? How much did Pinatubo emit? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted.
7-1600	7	54	27	54	27	Please add that some potential side-effects and risks are likely to be unknown at present time. [Government of Germany]	Accepted. We have inserted more text reminding the reader of unknown side effects througout the section

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7-1601	7	54	27	54	27	It seems very strange here to be discussing side effects before summarizing the potential for SRM to offset varyig amounts og climate change and various factors, and to be discussing the impacts out of the context of the effects of increasing GHGs on climate. For example, on the ozone depletion, how would it compare to the depletion due to CFCs and to the changes due to climate change. It just seems to me that an entire paragraph is missing here giving an indication of the importance of the counter-balancing effect and the various types of changes and impacts that would not be occurring--so the foregone costs. The analysis decision-maikers would want to make would presumably be a relative risk analysis, and there is just not the information here for that to be done even roughly--all that are mentioned are the potential additional adverse consequences. Fine to include, but also need to indicate the averted impacts as well. [Actually, reading a bit further, it would seem the question is why this paragraph is here rather than in section 7.7.3 where the results of model studies are presented, and these results would generally fit. [Michael MacCracken, United States of America]	Noted. We have made some attempt to put the side effects into context by adding a phrase to the introductory sentence indicating that the side effects must be weighed against the benefits of SRM, and noting that ozone depletion will occur through this century regardless of geoengineering.
7-1602	7	54	27	54	38	The paragraph on the risks of SRM based on stratospheric aerosols injection is underdeveloped: the ozone layer would be at risk at global scale during the decades of still elevated levels of ozone depleting substances in the stratosphere. Risks associated to elevated UV radiation due to ozone decrease should be mentioned more specifically. In particular, the sentence "The change in ozone could increase UV radiation reaching the surface, although attenuation by the aerosols may partially compensate" seems very mild to me. Decrease of precipitations in the tropical regions should be mentioned specifically. Recommendations on the need for further research on SRM using more sophisticated models (e.g. including atmospheric chemistry) should be included. [Sophie Godin-Beekmann, France]	Taken into account. We have modified the paragraph somewhat and now refer the reader to a broader discussion of positive and negative consequences discussed in the following section, which is where precipitation responses is dealt with. IPCC makes no recommendations for further research.
7-1603	7	54	27	54	38	For Stratospheric Aerosols, potential side effects are detailed. However, there is a variable treatment of potential side effects for the other SRM methods. There is no mention of potential side effects for Cloud Brightening, such as the possible but currently impact on marine ecosystems (Vaughan & Lenton, 2011 doi: 10.1007/s10584-011-0027-7), or the possible side effects of large local RF of 30 to 100 Wm-2. For Surface Albedo Changes potential effects are mentioned for modification of cropland albedo (page 55 lines 43-45), but not for deserts and an extensive list of possible side effects is given for ocean surfaces (page 55 lines 52-55). For Cirrus Thinning there is no mention of possible side effects. A consistent treatment, detailing the potential and possible side effects and where appropriate highlighting the limited literature on a number of these issues would provide a more thorough assessment. [Naomi Vaughan, United Kingdom]	Taken into account. We discussed side effects for stratospheric aerosols that were specific to that strategy and had been explicitly studied. We are not aware of studies that document those kinds of side effects and risks for other SRM methods. The Vaughan and Lenton paper focusses on ecosystem risks from ocean fertilization methods following Wingentiner et al. We now mention that study, but indicate that is not going to be assessed because of the follow up studies (Vogt et al, Woodhouse et al)
7-1604	7	54	27			In my opinion there is a “grammatical” contradiction. The report states: “A variety of potential side effects HAVE BEEN IDENTIFIED”. Then the effects are listed in conjunctive mood: “would lead”, “could increase”, “could impact”. Finally it is said that “These impacts poorly quantified”. Under such condition it would be better to say in the beginning: “A variety of potential side effects IS ASSUMED”. [Government of Russian Federation]	Noted. I think you mean "subjunctive" not "conjuntive". We have slightly revised the paragraph to make the statements more consistent. We state that the side effects are "potential". Each of the side effects is a consequence of the ozone chemistry of the stratospheric aerosols or their physical properties affecting UV and light scattering. There may however be other processes (not yet identified) that counteract those side-effects, and since they are quite small effects, we try to acknowledge the uncertainty by using the subjunctive to indicate the "possibility"
7-1605	7	54	31			Regarding the phrase, "from radiative heating associated with the ozone changes" - does this refer to radiative heating in the troposphere? Is this 1km change in tropopause altitude a global average, or just for the Arctic? The sulphate injection in the stratosphere could counteract some of this tropospheric warming... [Government of United States of America]	Taken into account. The radiative heating occurs in both the troposphere and stratosphere. It is most important in the tropics. It is a consequence of radiative heating from both aerosol and ozone changes. We have revised the sentence.
7-1606	7	54	35	54	36	How does the statement that acid deposition from geoengineering is too small to matter jibe with the (at least implicit) concern expressed on p. 7-58, lines 13-14, that ocean acidification from SRM is something to worry about? [Anthony Del Genio, United States of America]	Taken into account. The ocean acidification discussed on page 7-58 is from CO2. The acidification from the sulfate is very small (see Kravitz et al 2009) We have revised the sentence
7-1607	7	54	35	54	38	What is assumed about the spatial distribution of stratospheric sulfate as it settles into the troposphere and to	No change made. This has been accounted for in the

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						the surface? Could this be non-uniform, concentrating sulfate precipitation in some locations? [Ralph Kahn, United States of America]	cited study of acid deposition (see Kravitz et al, 2009)
7-1608	7	54	35			impact solar technologies => decrease efficiency of solar power plants …. Technology seems to me not the write word. [Michael Schulz, Norway]	Partly taken into account. We think the language is correct. The point is that for solar methods that require direct sunlight, SRM has an effect. But there are also solar methods that work off diffuse sunlight, and SRM would have less of a detrimental effect on these technologies
7-1609	7	54	37			"rain acidifcation that impact most ecosystems" => can this be quantified more? Or can this not be quantified? I think this should not fall into the category of "poorly quantified". [Michael Schulz, Norway]	Noted. We have slightly increased the precision of these statements.
7-1610	7	54	40	55	25	Please mention possible side effects and risks of cloud brightening, e.g. effects on the atmosphere circulation and local weather patterns as well as effects on precipitation frequency, intensity and spatial distribution. [Government of Germany]	Taken into account. Some additional detail appears in section 7.7.3.
7-1611	7	54	42	55	25	Section 7.7.2.2: Some statement about the lifetime of tropospheric aerosols that would be used for cloud brightening (~ days to weeks) would be very helpful. The lifetime of stratospheric aerosols should be anticipated to be longer (~months to a few years)...or is this considered a 'technological issue'? [Government of United States of America]	Taken into account. We believe there is not yet sufficient understanding of the subtleties of marine cloud brightening to merit much discussion of lifetime of tropospheric aerosols.
7-1612	7	54	44			The authors should consider revising the text to read: "ships" replace with 'ships (Durkee et al., 2000)' [Government of United States of America]	Rejected. For brevity, all discussion of shiptracks are now discussed elsewhere in the section
7-1613	7	54	44			Shouldn't this be weakened? My understanding was that it has been argued that shop tracks may largely be classical convection, with the slight warmth injected into a fairly homogenous cloud causing a line of ascent but a broad surrounding descent which means the cloud field as a whole may well darken. [William Ingram, United Kingdom]	Rejected. I can find no literature suggesting this mechanism for shiptrack formation. Refer to Section 7.4 and 7.5.
7-1614	7	54	51	54	56	Somewhere in this paragraph it should be mentioned that we don't yet even understand fundamentally how aerosols affect stratocumulus. For example, a major subject of interest these days is pockets of open cells in stratocumulus decks. People have suggested that these may be related in some ways to cloud-aerosol-precipitation interactions, but no real theory of this has yet to emerge. If we can't yet explain whether and how aerosols are involved in making or suppressing POCs, how can we anticipate what sprinking aerosols onto stratocumulus will do? [Anthony Del Genio, United States of America]	Noted. These issues have now been relegated to section 7.4 for brevity, although the text makes the very strong point that there are many uncertainties in understanding of cloud brightening.
7-1615	7	54	54			"values of droplet concentration" → "droplet concentrations" [William Ingram, United Kingdom]	Taken into account. Text drastically revised.
7-1616	7	54	55	54	55	"these clouds occupy a relatively small fraction of the planet" is not correct! According to the recent review paper by Wood (2012 in Mon.Wea.Rev.), stratocumulus clouds "cover approximately one-fifth of the Earth's surface area in the annual mean (23% of the ocean surface, 12% of the land surface)" [Jón Egill Kristjánsson, Norway]	Accepted. Paragraph revised
7-1617	7	54	55	54	55	comma required after "However" [Mark Lawrence, Germany]	Taken into account text substantially revised.
7-1618	7	54	55	54	55	Stratocumulus cover 25% of the global oceans. This is not really a small fraction of the planet. Several model studies show that one can achieve cancellation of CO2 radiative forcing by seeding an area much smaller than this, as is mentioned on P55, L4. [Robert Wood, United States of America]	Accepted, text substantially revised.
7-1619	7	54	55	54	56	I realize that most of the disucssion of climate change here considers single, global average values of forcing terms, but wouldn't the specific spatial distribution of the presumptive modifications exert considerable influence on which climate change effects are actually mitigated, and to what degree? And wouldn't gradients produced by any actual implementation also have impacts on the climate system? [Ralph Kahn, United States of America]	Taken into account. The space allocation is too short to discuss this issue in detail. We do mention that regional issues have been studied, and that most studies indicate that SRM does reduce climate change regionally over most parts of the globe. .
7-1620	7	54		55		Subsection 7.7.2.2: One very relevant concept that is missing from the presentation in this section is cloud albedo susceptibility. It is the marine clouds with high albedo susceptibility that should be the targets of artificial brightening methods. While pristine clouds (low CDNCs) are indeed generally more susceptible, the	Accepted. We agree that it would help to discuss this concept and text has been revised.

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						concept of cloud albedo susceptibility is more general and includes also the optical thickness of the cloud (think: not much gain from trying to brighten already bright clouds). Maps of liquid cloud albedo susceptibility have been published by Oreopoulos and Platnick (2008). [Lazaros Oreopoulos, United States of America]	
7-1621	7	55	3			This is completely & seriously untrue as written! Those papers show nothing of the sort. They show that IF it were possible to achieve large droplet concentration increases – which I & certainly some of those authors don't believe for a minute – this would generate such AF. [William Ingram, United Kingdom]	Taken into account. The sentence was accurate in reporting the study results, but the wording left a very misleading impression. It has been revised to indicate that it is a model result, and probably too simple to provide an accurate characterization of the forcing.
7-1622	7	55	5			“it is not clear whether” → e.g. “there is no evidence that” for accuracy [William Ingram, United Kingdom]	Accepted, Paragraph revised.
7-1623	7	55	6	55	10	Please also highlight the potential role of anthropogneic aersols, which can be transported from land over the ocean or directly emitted by ships in this pristine envionment and act as cloud condesation nuclei, thus potentially decrease the potential of artifial sea-salt emission. Also the emission of Dimethylsulfide (DMS) form the ocean may play a similar role. [Andrew Ferrone, Germany]	Rejected. These issues are dealt with in other sections of the chapter and are not specific to this section.
7-1624	7	55	6			The phrase "...it is not clear whether such increases are achievable." warrants clarification. [Government of United States of America]	Accepted. Paragraph revised
7-1625	7	55	8	55	10	For diversity, it would be highly appropriate to also mention Alterskjær et al. (2012 in ACP, 12, 2795-2807), who obtained -4.8 W m-2 using NorESM. They only injected sea salt between 30 degrees north and south, based on an investigation of cloud susceptibility, for which their model results were supported by MODIS retrievals. [Jón Egill Kristjánsson, Norway]	Accepted. New study is now cited.
7-1626	7	55	17	55	20	I find the para somehwat unlcear and suggest more explanation. [Jan Fuglestvedt, Norway]	Accepted. The paragraph has been deleted, and some of the points made earlier in the subsection.
7-1627	7	55	17	55	20	It appears that one is trying to give the reader the impression that the model estimates of the effectivenes of marine cloud brightening are probably overestimated. That may (or may not) be true, but we can not conclude that from such a simple argument. For instance, the same models that obtain a cooling effect from marine cloud brightening of -3 to -5 W m-2 have AF estimates of around -1 W m-2. So, why should we trust them in the latter case and not the former? In both Partanen et al. and Alterskjær et al. the same aerosol and aerosol-cloud schemes were used in both cases. [Jón Egill Kristjánsson, Norway]	Accepted. We agree this paragraph was confusing. It has been deleted, and some of the points we were attempting to make were strengthened earlier in the section.
7-1628	7	55	17	55	25	I dont see the use or meaning of these two paragraphs here. [Michael Schulz, Norway]	Accepted. See comments 7-1626 and 7-1727. Second paragraph is kept though as it brings useful information not covered elsewhere.
7-1629	7	55	17			The authors should consider replacing "clouds" with "stratocumulus" and replace the reference with the following, more appropriate reference:George, R. C. and Wood, R.: Subseasonal variability of low cloud radiative properties over the southeast Pacific Ocean, Atmos. Chem. Phys., 10, 4047–4063, doi:10.5194/acp-10-4047-2010, 2010. [Government of United States of America]	Accepted. See comments 7-1626 and 7-1727.
7-1630	7	55	20	55	20	should this read "avhievable with cloud brightening SRM"? [Peter Irvine, Germany]	Taken into account. Paragraph has been deleted.
7-1631	7	55	20			“climate models assume” is again quite untrue. Replace with something true, like “has been prescribed in climate models” or “was prescribed in the climate model studies above” [William Ingram, United Kingdom]	Taken into account. Paragraph has been deleted.
7-1632	7	55	27	55	55	Please mention possible side effects and risks of surface albedo changes. [Government of Germany]	Accepted. A parargraph has been added.
7-1633	7	55	27			I think that in the Section 7.7.2.3 too much attention is paid to “Surface Albedo Changes”. These methods have small forcing potential and very regional-dependent effects on temperature and precipitation. Maybe the idea that these technics are not very effective should be noted in this part of report. [Government of Russian Federation]	Partly taken into account. The "surface albedo changes" section is relatively short in comparison to other subsections and the limitations are discussed.
7-1634	7	55	32	55	37	This paragraph is poorly worded and difficult to follow. Please amend the language. [Government of Australia]	Taken into account. Paragraph is substantially reworded.

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7-1635	7	55	35	55	35	"overstimation of the surface area of the built environment that would be available for whitening" - is there any solid, citable evidence for this? There is no citation. I have seen some email discussions on this topic that make the statement plausible, but without some solid, citable evidence to fall back on, it is not appropriate for the IPCC as a speculative statement. Even if Lenton and Vaughan (2009) and Oleson et al. (2010) discuss this, it is not appropriate to use them as evidence of an "overestimation", since they were published before Akbari et al. (2012) and thus cannot directly counter its arguments. [Mark Lawrence, Germany]	Taken into account. "overestimation" is changed to "uncertainty in". The citation to Lenton and Vaughan is warranted because it discusses the same RT calculations that are presented in Abkari et al (2012) and in an earlier reference.
7-1636	7	55	36	55	36	Insert "cooling effects" after "suggest that these" [Jón Egill Kristjánsson, Norway]	Accepted. Reworded.
7-1637	7	55	39	55	47	You might mention here that in addition to altering the surface abledo and possibly the water cycle, the entire *biological* component of the ecosystem could be affected in unknown ways by introducing non-native or bioengineered species. At least in my view, the fact that the impacts are not at all understood would be a reason to highlight the uncertainty here. [Ralph Kahn, United States of America]	Accepted. A sentence has been added.
7-1638	7	55	39			Again, this is totally unjustified. Hamwey's “Exploratory Study” modelled the effect of a 25% increase in the albedo of grasslands, but gave no explanation for this value or any suggestion that there was evidence that any usable increase in grassland albedo was possible. [William Ingram, United Kingdom]	Accepted. "hypothetical" has been added.
7-1639	7	55	43	55	44	Changing albedo by replacing native species with other natural or bioengineered species is a crazy idea and needs to be strongly substantiated and evaluated in detail. Sure albedo may change but also ecosystem C and N cycling, soil C and N stocks and thus exchange rates of GHG between ecosystems and the atmosphere. This effect can overwrite any albedo benefits but is not mentioned at all [European Union]	Accepted. A sentence has been added to discuss side effects and risks.
7-1640	7	55	43	55	44	What are the processes/mechanisms causing these potential effects from surface albedo changes in low latitudes of reduced soil moisture, cloud cover and precipitation? [European Union]	Rejected. There is no space here to go into much further details.
7-1641	7	55	43			Add “side-” [William Ingram, United Kingdom]	Sentence is deleted.
7-1642	7	55	44			Why should there be a reduction in soil moisture and cloud cover with higher albedo? Can this be explained? [Michael Schulz, Norway]	Sentence is deleted.
7-1643	7	55	45	55	47	A short sentence should be added with regards to the Irvine et al. (2011) study, paralleling the note made in lines 43-44: "However, this cooling was accompanied by substantial impacts on the distribution of tropical precipitation in the simulations." [Mark Lawrence, Germany]	Taken into account. Discussion has been re-ordered.
7-1644	7	55	46			What has desert albedo to do with crop albedo change? A little confusing. [Michael Schulz, Norway]	Taken into account. Discussion has been re-ordered.
7-1645	7	55	49	55	55	For oceans feedback of oams on the biosphere are mentioned, why is this not done for terrestrial ecosystems (see above) [European Union]	Taken into account. Side effects of changing terrestrial ecosystems is being discussed.
7-1646	7	55	49	55	55	Seitz is not proposing to create foam, but to increase the concentration of microbubbles in the near surface layer of the ocean, so the equivalent of creating a cloud in the water (as a ship wake does with larger bubbles). This should be corrected. [Michael MacCracken, United States of America]	Accepted. Microbubbles may be a better term, but Evans does mention foam when talking about bubbles.
7-1647	7	55	52	55	55	As best I can tell, the overall impact of putting artificial foam on the world's oceans has not been assessed qualitatively either. I suggest removing the word "quantitatively" and "in the peer-reviewed literature" here, as they leave the impression that the impacts are understood qualitatively, and possibly also quantitatively but just not in the published literature. [Ralph Kahn, United States of America]	Accepted.
7-1648	7	56	1	56	13	Please mention possible side effects and risks of cirrus thinning. [Government of Germany]	Accepted. A sentence has been added noting that no studies have been done of the side effects.
7-1649	7	56	8	56	8	"higher temperatures" seems misleading here. Better: "lower supersaturations" [Jón Egill Kristjánsson, Norway]	Taken into account. Paragraph revised..
7-1650	7	56	10	56	10	Insert "ice" before "particle" [Jón Egill Kristjánsson, Norway]	Taken into account. Paragraph revised
7-1651	7	56	11	56	13	Cirrus clouds also tend to stabilize the upper troposphere (LW heating), thereby inhibiting or delaying deep convective developments in the Tropics. Modifying cirrus properties would presumably also have an impact on deep convective cloud properties. [Government of Australia]	Noted. There are no studies examining this, and for brevity these details willno longer be discussed

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7-1652	7	56	11			Even “poorly” seems optimistic to me! “barely”? “that are not”? [William Ingram, United Kingdom]	Rejected. The message seems strong enough.
7-1653	7	56	12	56	12	Delete the word "responses" [Jón Egill Kristjánsson, Norway]	Taken into account. Text drastically revised.
7-1654	7	56	12	56	12	"for water vapour responses could produce responses" - remove the first "responses" [Mark Lawrence, Germany]	Taken into account. Text drastically revised.
7-1655	7	56	15	57	50	Please mention that SRM would not prevent ocean acidification and other related significant changes in the marine bio-geo-chemical cycles. [Government of Germany]	Taken into account. The reader needs to be reminded of this point, but we prefer to do that in the synthesis. This section is to assess the response to SRM, not the response to CO2 increases. .
7-1656	7	56	17	56	18	I worry about the tone here. *Every* model evaluation is idealized, not just the runs that test multiple ways in which SRM might be implemented. The first six sections of this chapter demonstrate in fair detail the strengths as well as the limitations of current climate modeling -- and in particular, how incomplete is the representation of even the physical processes associated with just aerosols and clouds, and how equivocal are the results, even for this one part of the Earth system. There are major gaps in our ability to simulate, let alone predict, environmental changes for even the anthropogenically unperturbed system, or the impacts of the system perturbed only by GHGs. So perhaps this section could provide better context for the substantially larger uncertainties associated with model runs that attempt to make predictions involving more subtle and complex perturbations. Otherwise, readers might gain a false sense of confidence in the viability of some of these geoengineering options. [Ralph Kahn, United States of America]	Taken into account. We have inserted some additional cautionary language in the introductory paragraphs of this section,
7-1657	7	56	17	56	41	Have coupled climate CN models been used here? Unclear. I would guess that the biosphere (terrestrial and marine ecosystems) strongly respond to SRM. But this is not outlined at all [European Union]	Taken into account This is discussed in Section 6.5.4 and we have now included a cross-reference to Chapter 6.
7-1658	7	56	17	56	42	Given the uncertainty in the way that models deal with clouds, precipitation and aerosols is there some larger statement of uncertainty needed here in this paragraph? [European Union]	Accepted. Please see the new introductory paragraph to section 7.7.3
7-1659	7	56	17	56	42	Baughman et al. (2012, J Climate) discusses potential impacts of marine cloud brightening on the tropical circulation and precipitation patterns [Robert Wood, United States of America]	accepted. But there is no room for much discussion.
7-1660	7	56	17			The sentence "The many way that SRM can be implemented" suggests that SRM can really be implemented: This is misleading. Please change wording. [Government of Germany]	Accepted.
7-1661	7	56	24	55	27	I'd argue that the longwave forcing from greenhouse gases does vary substantially both seasonally and latitudinally, however there is a much larger latitudinal and seasonal difference with the shortwave forcing. The shortwave forcing falls off more rapidly with increasing latitude than for the longwave greenhouse forcing. The shortwave forcing tracks the solar seasons whereas the longwave forcing tracks the temperature which follows the solar seasons but the cycle is both delayed and diminished in magnitude, i.e. peak to trough difference. This sentence requires some rephrasing. [Peter Irvine, Germany]	Noted. This is not inconsistent with our text, but there is not much room for this kind of detail. We have revised our words somewhat using "more uniform" and "less uniform" heating rates to make the pont
7-1662	7	56	26	56	26	Insert "anthropogenic" before "greenhouse gases" [Jón Egill Kristjánsson, Norway]	Noted. Paragraph revised substantially
7-1663	7	56	26	56	27	I suggest skipping parentheses around "associated with .." and "from SRM". [Jón Egill Kristjánsson, Norway]	accepted.
7-1664	7	56	26	56	27	"shortwave forcing (from SRM)." => "shortwave forcing (from SRM), which is greater in the tropics than the high latitudes due to the lower incident angle of solar radiation and the lower average surface albedo." [Mark Lawrence, Germany]	Taken into account. We found another way to phrase the sentence indicating that the LW forcing is not uniform.
7-1665	7	56	29	56	29	The paper by Kravitz et al. (2011) only describes the GeoMIP experiments, it does NOT show any results. i.e., the graphics in Figure 7.22 and 7.23 do not come directly from it. I made this comment on the FOD and since it is only technical, it is unclear why it was not fixed. The first published results are described in Schmidt et al. (2012b), which is at least now cited later. The figure is new in this draft. Its origin should be clearly indicated in the figure caption; perhaps it is from Kravitz et al. (2012a), but it is not possible for me to be sure since that is only submitted and I do not have a copy. If it is a new figure made specifically for the IPCC report, then this should also be noted clearly with sufficient background information to know how it was developed (i.e., how many models were involved, etc.) [Mark Lawrence, Germany]	Taken into account. The Kravitz (2011) citation is to the paper describing the GEOMIP experimental design, not to the paper discussing these results. We now cite the Schmidt et al paper for the model result reference

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7-1666	7	56	33	56	34	Need to insert "near-surface" before "atmospheric stability", or else rephrase. [Jón Egill Kristjánsson, Norway]	Taken into account.
7-1667	7	56	33			“temperature increases” made this line very hard for me to read! “Balancing the global-mean surface temperature with a combination of these RF increases”? [William Ingram, United Kingdom]	Taken into account. The sentence has been modified to reduce confusion.
7-1668	7	56	36	56	36	add full stop or period after section 7.6 [Peter Irvine, Germany]	Noted. This text has been deleted for brevity
7-1669	7	56	36	56	36	"Section 7.6 Other…" - period needed before "Other" [Mark Lawrence, Germany]	Noted. This text has been deleted for brevity
7-1670	7	56	36	56	36	A full stop is required after Section 7.6 [Naomi Vaughan, United Kingdom]	Noted. This text has been deleted for brevity
7-1671	7	56	36	56	37	add citation for stomatal response to co2 [Peter Irvine, Germany]	Accepted.
7-1672	7	56	40	65	41	Specifically the precipitation changes due to SRM may have a large effect on carbon and nitrogen cycling and biosphere-atmosphere exchange for terrestrial systems. Need to be mentioned [European Union]	Accepted.
7-1673	7	56	40			Needs a reference [William Ingram, United Kingdom]	Accepted.
7-1674	7	56	55	56	55	Irvine et al. 2010 does not investigate regional application of SRM rather the regional effects of global SRM therefore should be removed from this list [Peter Irvine, Germany]	Accepted.
7-1675	7	56	56	56	57	This sentence needs to be etirely rewritten. First Bala et al. 2010 considered ocean-only SRM which is critical and the direction of the RF gradient must be made clear, and why this should give rise to the increase in continental precipitation. I'd suggest the following: "Bala et al. 2010 found that for cloud albedo increases limited to the oceans there was a large reduction in global precipitation but an increase in precipitation and runoff over land caused by changes in circulation driven by the relative temperature difference between the land and the ocean." [Peter Irvine, Germany]	Taken into account. This paragraph has been largely rewritten and is much less specific now.
7-1676	7	56	57	57	2	I'd suggest revising this sentence to be more in line with the above suggestion: "In contrast, Irvine et al. 2011 found a lesser reduction in global precipitation for desert albedo geoengineering but a greater reduction in continental precipitation and additionally found substantial reductions in rainfall over the Indian and Sahel regions". At least I'd suggest revising "reduction in rainfall over ..." to "substantial reduction in rainfall over..." as the reductions are of the order of 20-30% [Peter Irvine, Germany]	Taken into account. This paragraph has been largely rewritten and is much less specific now.
7-1677	7	57	2	57	2	Irvine et al. 2011 dealt with desert albedo geoengineering not Irvine et al. 2010 [Peter Irvine, Germany]	Taken into account. Only Irvine et al (2011) is cited now.
7-1678	7	57	11	57	24	The paragraph raises 3 issues: the issue that SRM may be implemented in different ways (but only considers different strengths) to achieve different goals, the issue of the dependence of the resultant SRM scenarios on CO2 concentrations (i.e. mitigation and CDR alters the situation), and the potential for a termination shock. The order of the paragraph seems quite muddled. I'd suggest restructuring the paragraph to raise the issues I listed in the order I raised them above. [Peter Irvine, Germany]	Taken into account. The paragraph has been largely rewritten.
7-1679	7	57	11	57	24	My paper: Irvine et al. 2012, which appeared in nature climate change, deals with many of the issues raised in this paragraph, i.e. different goals for SRM (temp and SLR, and their rate of change), different appraoches for implementing SRM (strength and rate of phase-in) and the potential for a termination shock or for a more gradual phase-out and how these would impact the rate of temperature change. [Peter Irvine, Germany]	Taken into account. Irvine et al (2012) is now cited for the rapid rate of temperature change after termination.
7-1680	7	57	11	57	24	This is an important paragraph that captures a key feature of SRM as used to counteract global warming, that it is necessary to maintain the intervention for a very long time. It may be appropriate for it to have a separate subheading (e.g. Termination Effect) and occur after the two following paragraphs about further modelling studies. Importantly in the comparable part of Chapter 6 on CDR it contains a subheading 'The Rebound Effect' this may easily be confused with the 'Termination Effect' (Royal Society, 2009) that is described by this paragraph and stated in line 22. [Naomi Vaughan, United Kingdom]	taken into account. The paragraph has been streamlined, but it cannot go into much more details (especially regarding a comparison with the rebound effect) given space limitation.
7-1681	7	57	11		12	Caveat in brackets actually applies to whole sentence: omit “for a long time (“ & “)” [William Ingram, United Kingdom]	Accepted.
7-1682	7	57	12	57	15	We think the content of this sentence should be conveyed in the executive summary. "If SRM were used to counter positive forcing, it would be needed as long as the CO2 concentrations were high. If greenhouse gas	Accepted. This issue has been brought to the ES, although in a slightly different way.

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						concentrations increase, then the scale of SRM to offset the resulting warming would need to increase" [Government of NORWAY]	
7-1683	7	57	12	57	20	A seminal paper by Matthews & Calderia (2007) (doi:10.1073/pnas.0700419104) should be included in the references here. [Naomi Vaughan, United Kingdom]	Accepted.
7-1684	7	57	15	57	17	Here is another example of the issue with the tone of this section, e.g., considering the uncertainties in even the basic "climate sensitivity" parameter (I've *added* the words in asterisks): Figure 7.24 shows *simulations* of what the globally averaged warming and precipiation changes *might be* under a 1% yr-1 CO2 increase (GeoMIP experiment G2) with and without SRM... [Ralph Kahn, United States of America]	Taken into account. We appreciate the distinction and will try to remind the reader that these are only simulations.
7-1685	7	57	15	57	18	Maybe add a comment on the important precipitation reduction in GeoMIP runs and their possible implications? [Andrea Flossmann, France]	Taken into account. Something is said on precipitation. Note however that the precipitation reduction in the global mean is not "important".
7-1686	7	57	16			Section 7.7.3: Is there no study to cite for the GeoMIP experiment G2? Robock or Kravitz? [Government of United States of America]	Accepted.
7-1687	7	57	17			“the” - WHICH? With CO2 & no geoengineering, or CO2 plus geoengineering? Oh, neither! [William Ingram, United Kingdom]	rejected. This comment is unclear.
7-1688	7	57	19	57	19	"significant impacts" (should be plural) [Mark Lawrence, Germany]	Accepted.
7-1689	7	57	19			impactS [William Ingram, United Kingdom]	Accepted.
7-1690	7	57	20	57	22	The authors should consider including an example of these 'other strategies'. [Government of United States of America]	Accepted.
7-1691	7	57	20	57	22	This sentence requires rephrasing and the issue of the range of "scenarios" for SRM and mitigation requires more in-depth treatment. [Peter Irvine, Germany]	Noted. Not much opportunity for elaboration because of length restrictions.
7-1692	7	57	20	57	22	This sentence is not as concise as it could be and is somewhat circular, "…use SRM with mitigation……in combination with greenhouse gas emissions reductions". Mitigation means greenhouse gas emissions reductions. [Naomi Vaughan, United Kingdom]	Accepted. Text has been reformulated.
7-1693	7	57	20	57	22	In addition to significant mitigation, i.e. emissions at or near zero, these scenarios would require CDR to enable the SRM intervention to be applied for a ‘shorter’ period of time than multiple centuries (Matthews 2010 doi: 10.4155/cmt.10.14 and Boucher et al 2012 doi: 10.1088/1748-9326/7/2/024013). In Smith & Rasch (2012) SRM was used for less than a century, due to significant emissions reduction and significant CDR capacity (See Figure 1a in Smith & Rasch 2012). [Naomi Vaughan, United Kingdom]	taken into account. The suggestion has been merged with comments 7-1691 to 7-1692
7-1694	7	57	20	57	24	Using mitigation in addition to SRM would still invoke a ‘termination effect’; it would just be of a smaller magnitude. The magnitude of the termination effect would be determined by the difference between the global temperature due to the atmospheric CO2 concentration (CO2 emissions must be at or near zero to lower the atmospheric CO2 concentration Matthews, 2010; Matthews & Caldeira, 2008 doi: 10.1029/2007GL032388) and the global temperature SRM has generated. [Naomi Vaughan, United Kingdom]	taken into account. I am not convinced. If SRM and mitigation were invoked simultaneously today, and then SRM terminated in 10-50 years, I do not think there would be termination effect..
7-1695	7	57	22	57	22	Replace "emissions" by "emission" [Jón Egill Kristjánsson, Norway]	Accepted.
7-1696	7	57	22	57	24	A reference to WGIII could be given if this is discussed there. [Jan Fuglestvedt, Norway]	Rejected. Unfortunately the SOD of WGII does not seem to discuss this effect.
7-1697	7	57	22	57	24	I disagree that the scenarios would not be subject to the termination effect, they would still experience such a shock if a failure occurred or if SRM were phased out, instead they reduce the magnitude of the risk and the period in which it could occur. [Peter Irvine, Germany]	taken into account. The suggestion has been merged with comments 7-1691 to 7-1693
7-1698	7	57	24	57	24	The inclusion of the Wigley (2006) reference implies that in Wigley (2006) SRM was used for a less than a century. Wigley (2006) uses SRM in conjunction with a mitigation strategy for 300 years (and further, the results figure stops at year 2400) (See Figure 1. Left Panel in doi: 10.1126/science.1131728). [Naomi Vaughan, United Kingdom]	Accepted. Citation deleted

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7-1699	7	57	27			left & right → above & below [William Ingram, United Kingdom]	Accepted. Panels are labelled a) and b).
7-1700	7	57	32	57	32	Insert "have" before "examined" [Jón Egill Kristjánsson, Norway]	Rejected. Use of have would imply the examination are continuing. They are done
7-1701	7	57	34	57	34	Insert "of" after "doubling" [Jón Egill Kristjánsson, Norway]	Accepted.
7-1702	7	57	34			Omit “a” [William Ingram, United Kingdom]	taken into account. The sentence was revised another way
7-1703	7	57	39			“larger” makes no sense – omit or say what was meant [William Ingram, United Kingdom]	Accepted.
7-1704	7	57	44			What does “global” mean? The sentence clearly seemed to imply “everywhere on the globe” rather than “in the global mean” but then this is contradicted by the next sentence [William Ingram, United Kingdom]	taken into account. The sentence has been modified to reduce confusion.
7-1705	7	57	46			What does “signatures” mean? [William Ingram, United Kingdom]	taken into account. The sentence has been modified to reduce confusion.
7-1706	7	57	46			Omit “that” or restore lost text that belongs with it [William Ingram, United Kingdom]	taken into account. The sentence has been modified to reduce confusion.
7-1707	7	57	53	57	55	The first sentence gives the impression that SRM may be a realistic option. This is not clear yet. Please add: "Nevertheless a lot of open questions as for effectiveness, side-effects and risks still exist. Level of uncertainy is still very high" [Government of Germany]	taken into account. We have added cautiounary text to address this concern.
7-1708	7	57	54	57	54	Be clear here that (some/all?) Solar Radiation Management strategies are only able to counter a portion of the global warming at the surface (not the entire atmospheric column) and that because tropospheric temperatures will continue to be higher (get higher?) (due to high levels of GHG) there will be other knock-on effects on the hydrological cycle such as decreased global precipitation but with regional differences. [European Union]	taken into account. The temperature effect applies at altitude as well as at the surface. We have introduced additional text in the synthesis to address the other concerns
7-1709	7	57	54	57	54	Not clear what this means: "may be able to counter a portion of the global warming", is it restricted to temperature or all of the effects of climate change? [Peter Irvine, Germany]	taken into account. We have changed the word counter to counteract (as in "to mitigate" and clarified that we are refering to temperature, precipitation, and sea ice extent.
7-1710	7	57	55	57	55	Has a complete assessment on the costs (including external effects, not only costs of implementing and running the method), risks and impacts been done to justify tthe expression "scalable"? If yes, please provide more information, why SRM by stratospheric aerosol would be an option and what is meant with "scalable". If not, please use a more appropriate expression or delete the statement. [Government of Germany]	taken into account. As stated in the introduction, we do not consider costs in our assessment. We used the term "scalable" to mean that multiple lines of evidence indicate that it appears possible to increase the SRM radiative forcing to an amplitude sufficent to counteract the equivalent CO2 radiative forcing without obvious negative consequences that would preclude further study.
7-1711	7	57	55	57	55	Especially in the context of the sentence that follows, I think this understates the uncertainties associated with both the paramterization within the models themselves, and the impacts on unmodeled aspects of the Earth system. [Ralph Kahn, United States of America]	Taken into account. We now remind the reader much more forcefully of the risks and uncertainties of SRM.
7-1712	7	57	56	57	57	Section 7.7.4: Are there citations for the statement: "models disagree on the amount of material that would need to be injected to achieve this." If so, the authors should consider including them. [Government of United States of America]	Rejected. This is a synthesis section that draws on material discussed earlier in the section.
7-1713	7	57	56			“twofold increase in” → standard “doubling of” for clarity [William Ingram, United Kingdom]	Taken into account. Sentences revised.
7-1714	7	57	57	58	4	The current text reads as if it's unfortunate chance that some SRM has been less studied & so we can't say much about it – the reality is that climate scientists have concentrated on the ones that seem most plausible to them, so the absence of information is in fact evidence of implausibility (not necessarily well-founded). While I appreciate IPCC's job is to review the published literature rather than the gaps, the current text is seriously	Taken into account.This is a difficult issue to address in this chapter. There are many issue that influence the methods that have been studied, ranging from cost, to readiness, to level of scientific understanding.

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						misleading. Surely some review already cited (ideally the Royal Society one) makes this point? If not, it should be made as strongly as possible given that constraint. Phrases like "best-established" & "less well established" might be good. [William Ingram, United Kingdom]	Cost, and readiness cannot be discussed within this assessment making it difficult to fold these criteria into the text
7-1715	7	57				Section 7.7.4 It would seem appropriate here to link to other chapters where geoengineering is discussed such as CDR in Ch 6 and model projections in Ch 11. [European Union]	Accepted. Geoengineering is not discussed in Chapter 11.
7-1716	7	58	1	58	3	This statement ignores desert albedo geoengineering, but perhaps rightly so. Perhaps this should be made clear. [Peter Irvine, Germany]	Accepted. This statements holds true with desert included.
7-1717	7	58	3	58	4	This sentence is focused on other approaches for SRM, but it might well be appropriate here to have a sentence instead that at least mentions that there is a category of ideas focused on applying the range of existing techniques to other than moderating global average temperature.It is indeed the case that they have not been studied to any great extent, but it would seem to me that the possibility should at least be mentioned so it is covered. On the global scale, for example, the focus or optimization could be on global average precipitation or on the hydrologic cycle in general or on latitudinal variations in temperature (MacMartin et al. 2012, MacCracken et al 2012.). In addition, there are papers suggesting applications of the various technologies or alternative ones to specific impacts, ranging from hurricanes (the patent application of wave powered barges; a paper by Salter) and for other potential impacts (MacCracken, 2009). Leaving out mention of all of this or including it only implicitly seems to me an omission that needs to be remedied, if only indicating that there has been very limited investigation. [Michael MacCracken, United States of America]	Taken into account. This is now mentioned in the chapau of Section 7.7.
7-1718	7	58	4	58	4	Add "risks" [Government of Germany]	Accepted. Risks and side effects are now mentioned in the sentence.
7-1719	7	58	4	58	4	This implies that the efficacy, scalability, viability and consequences have been addressed in this chapter. Viability and consequences have been mentioned for some but not all the SMR methods presented (see comment 29). [Naomi Vaughan, United Kingdom]	Accepted. We have now included statements about these topics more thoroughly in the revisions. We removed "viability" from the list.
7-1720	7	58	7	58	7	"imprecise"? [Government of Germany]	Rejected. We think the word delivers the appropriate message
7-1721	7	58	8	58	10	Please add - as stated on p. 62, line 36-38 - that a geoengineered climate might nevertheless be different from the current or past ones. [Government of Germany]	Accepted
7-1722	7	58	8	58	13	Again about tone: "Model consistency" does not guarantee fidelity with the real Earth. And it is likely that there are important side effects that have not been identified as yet. I think this critical uncertainty in the current state of understanding should be made clear. [Ralph Kahn, United States of America]	Accepted. Text has been substantially revised. Side effects are discussed more frequently in the text, and the text now highlights more strongly that the conclusions are based on models.
7-1723	7	58	9			Regarding the phrase, "would be much closer to 20th century climate"- is this in terms of global surface temperature? regional precipation? weather extremes? The authors should clarify the text. [Government of United States of America]	Accepted. Clarified
7-1724	7	58	10	58	10	Could also cite Ban-Weiss and Caldeira 2010 here. Ban-Weiss GA, Caldeira K (2010) Geoengineering as an Optimization Problem. Environmental Research Letters, 5, 1-9. [George Ban-Weiss, United States of America]	Rejected. We think the focus on precipitation rather than precipitation minus evaporation is clearer in the other citations.
7-1725	7	58	10	58	10	I'd suggest replacing Moreno-cruz et al. 2011 with ricke et al. 2010 which is the original source for the data used in Moreno-cruz et al. 2011 and makes the point that is being referred to in this sentence. [Peter Irvine, Germany]	taken into account. We have added the Ricke reference also because it does make that point and others, but the Moreno Cruz article is also very revealing.
7-1726	7	58	10	58	11	This remark repeats the one on page 54, line 27. It would seem appropriate to be including the paragraph on impacts back in this section or just above rather than on page 54. [Michael MacCracken, United States of America]	Taken into account. We are trying to provide a term summary here, rather that a catalog of side effects. We have added a little text on this topic
7-1727	7	58	10	58	11	In my opinion a wide range of side effects have not been considered in this chapter. Possible ecosystem impacts of SRM for example have not been mentioned, nor has the possibility of unintended or unforeseen	Taken into account. We now have more text on ecosystem impacts in the section, and have added a

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						impacts (Russell et al 2012 doi 10.1007/s13280-012-0258-5, Vaughan & Lenton, 2011, Royal Society, 2009) [Naomi Vaughan, United Kingdom]	sentence to this section (and cited the Vaughan and Lenton 2011).
7-1728	7	58	10	58	12	I'd add mention of elevated CO2 levels as the combination of CO2 and SRM gives rise to many of the "side-effects" or residual changes, e.g. the reduced transpiration, etc. [Peter Irvine, Germany]	Accepted.
7-1729	7	58	13	58	14	See comment on row 38 above. Is acidification a concern or isn't it? [Anthony Del Genio, United States of America]	Taken into account. See response to that comment.
7-1730	7	58	13			From the sentence “SRM will not address the issue of ocean acidification from increasing CO2 and may have other impacts on the climate system” reader can understand that ocean acidification IS a negative impact of SRM, but it is not. SRM is not destined for prevention of ocean acidification. Similarly, no one adaptation measure can prevent ocean acidification, but it is not suggest that it is a shortcoming of adaptation. [Government of Russian Federation]	Accepted. We have revised the sentence
7-1731	7	58	16	58	16	Replace "ecosystem" by "ecosystems" [Jón Egill Kristjánsson, Norway]	Rejected. We are using "ecosystem" as an adjective modifying adaptation just as we use the word "human" to modify adaptation.
7-1732	7	58	16	58	17	Providing some paleoclimate context for these rates of temperature change (Figure 7.24) would be helpful to the reader. [Naomi Vaughan, United Kingdom]	Rejected. There isn't space for more detail here
7-1733	7	58	17	58	17	Add the consequence "This would require to run, govern and finance the SRM-system for centuries - once having started using it" [Government of Germany]	Rejected. This is not within the charter of this assessment.
7-1734	7	58	19			FAQ 7.1: Overall we felt this FAQ is currently too lean, and provides more of a text book response, rather than drawing on the latest quantitative results coming from the Chapter 7 assessment. Specifically, values should be provided in the text in support of the positive net cloud-climate feedback that is mentioned in the Chapeau. [Thomas Stocker/ WGI TSU, Switzerland]	The FAQ has been expanded, and makes more connections to the content of Chapter 7. We think that numerical measures of the strength of the cloud feedback should not be given in the FAQ.
7-1735	7	58	21			The FAQs are very well-written. In FAQ 1, paragraph 3 describes three pathways by which clouds might feed back on climate change. The next two paragraphs partition these feedbacks according to high and low clouds. It would be nice to see some more explicit bridge between these two ways of viewing the problem. [Robert Pincus, United States of America]	Noted. Thanks.
7-1736	7	58	21			FAQ 7.1: A figure would be very useful to illustrate the different characteristics of low/high clouds. This would give this FAQ equal weight in comparison with FAQ 7.2 [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. A figure has been added.
7-1737	7	58	23	58	27	FAQ 7.1 : The statements « the net cloud-climate feedback is positive » and « will not significantly limit global warming » are a bit redundant. I would suggest to keep only the first one, and to replace the second one by « However, the extent to which cloud changes will amplify global warming remains very uncertain. » [Sandrine BONY, France]	Accepted. Change made as suggested.
7-1738	7	58	23	59	18	FAQ 7.1: Should the authors define greenhouse effect for the FAQ? [Government of United States of America]	Accepted. The term "greenhouse effect" has been removed.
7-1739	7	58	24	58	24	As shown in section 7.3.5.4 and 7.4.1.3 have shown that regional effects of aersols are important and that regional models are capable of bridging the gap between fine-scale models used for idelatised cases and the large scale models that represent a realistic meteorology. Thus this effect should be reflected here by saying, global or regional model. [Andrew Ferrone, Germany]	Rejected. We avoid discussing a hierarchy of modeling approaches in the FAQ, and restrict the discussion to global climate models.
7-1740	7	58	24	58	26	It would be good if the words prediction and projection could be used in a consistent way in the FAQs. While prediction might be ok in this context, in general, lay readers will need to understand that in most instances, climate models are used to provide projections (which are conditional upon assumed emissions scenarios) rather than predictions (forecasts). It would be good to reinforce that by using "project" as consistently as possible throughout the responses. [Francis Zwiers, Canada]	Change made as suggested.
7-1741	7	58	25	58	25	We propose to replace "...but are far from perfect..." by for example "but are not able to simulate the properties of clouds sufficiently". [Government of Germany]	Edited to say "…but important errors and uncertainties remain.'

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7-1742	7	58	26	58	27	The last part of this statement is confusing - it states that net cloud-climate feedback will likely be positive and will not "limit" global warming. If the feedback is positive, will it then enhance global warming? Please clarify. [Government of Canada]	Accepted.The wording has been changed.
7-1743	7	58	26	58	27	If the net cloud-climate feedback is positive, then it would be expected to enhance global warming. Why is this not stated explicitly, rather than saying "it will not significantly limit global warming"? This seems to be understating the point that it will in fact not limit global warming at all, but will rather enhance it. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. The wording has been changed.
7-1744	7	58	27	58	27	The final text: 'and will not significantly limit global warming' seems a 'defensive' way to state this -- as if countering arguments that it would. Restate as 'and will amplify global warming' as this is more straightforward and consistent with the findings. [Government of Australia]	Accepted. The wording has been changed.
7-1745	7	58	29	58	29	Please review the wording of this statement. It is confusing to use the word "cloudiness" here rather than "clouds" given the wording of the question. Is it also accurate to state "climate change" rather than "climate system" (which is what is used in the rest of the paragraph)? [Government of Canada]	The wording has been changed.
7-1746	7	58	31	58	31	Is “as water condenses and releases latent heat” should be “as water condenses, converts into precipitation falling down and leaving heat of condensation in the atmosphere” - evaporation of clouds is known to many readers.... [Government of Poland]	Change made as suggested.
7-1747	7	58	32	58	32	"Solar and infrared" is not quite correct, since the near-IR is a non-negligible part of the solar. Either say "solar and thermal infrared" or "shortwave and longwave". [Anthony Del Genio, United States of America]	Change made as suggested.
7-1748	7	58	32	58	32	Some technical terms are clearly unavoidable in formulating this response, but to ensure that the response is as accessible as possible to lay readers, it would be good use consistently the same terms throughout the reponse. For example, here there is reference to solar and infrared radiation, while on line 43, there is reference to infrared and visible radiation. This kind of variation in terms simply imposes an additional burden on non-expert readers that, if at all possible, should be avoided. [Francis Zwiers, Canada]	"Visible" has been replaced by "solar."
7-1749	7	58	39	58	39	It would be helpful to the lay reader if language could be kept as simple and direct as possible. A suggestion for how this sentence could be rewritten to communicate more effectively to lay people is as follows: "All cloud processes have the potential to change as the climate changes". [Francis Zwiers, Canada]	Change made as suggested.
7-1750	7	58	42	58	42	It would be better to say "longwave and shortwave" radiation, but if you are going to say "infrared and visible," you should also add "ultraviolet", which is a non-negligible part of the solar spectrum. [Anthony Del Genio, United States of America]	Modified to say "thermal infrared and solar".
7-1751	7	58	46	58	46	Change "still are" to "are still" [Jón Egill Kristjánsson, Norway]	The wording has been changed.
7-1752	7	58	46	58	48	This statement looks misleading in negative sense as a statement of FAQ, giving impression that we do not know anything about cloud feedbacks. [Teruyuki Nakajima, Japan]	The text has been changed to reflect our increasing understanding of cloud feedbacks.
7-1753	7	58	46			FAQ 7.1: "We still are not sure" -- who is "we" referring to? The Chapter authors, the IPCC, the cloud science community? Suggest to avoid personal nouns and to rephrase as, e.g., "It's still not understood what types of cloud feedbacks…." [Thomas Stocker/ WGI TSU, Switzerland]	Change made as suggested.
7-1754	7	58	48	58	49	This FAQ is very well written, particularly so for a tricky topic. One point of clarification that is required: lines 48-49 refer to models used in the past two assessments. This naturally begs the question "what about models used in this assessment?" If they have not been evaluated yet in this respect, then that could be mentioned, to avoid readers wondering about this. If the evaluation of current models in Chapter 9 is consistent with the conclusions here (either a positive feedback or little effect), then that should also be mentioned. [Government of Canada]	The wording has been changed.
7-1755	7	58	48	58	49	This sentence refers to results from models used in the "past two" iPCC assessments, ie the TAR and the AR4. Is there nothing that can be said about cloud feedbacks in the models assessed in this report (the AR5) ? [David Wratt, New Zealand]	The wording has been changed.
7-1756	7	58	50	58	51	Suggest replacing "but are an aspect …" with "; they result from the functioning of the clouds in the simulated	Change made as suggested.

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						atmosphere and their effects on how light and heat passes through the atmosphere." [Francis Zwiers, Canada]	
7-1757	7	58	52	58	52	Suggest inserting "largely" before "explain" (are cloud feedbacks the only factor that affects sensitivity?). [Francis Zwiers, Canada]	Change made as suggested.
7-1758	7	58	54	58	55	I know what is trying to be said here, but as written it is misleading. Low clouds cool the surface and hence have a profound impact on how much infrared is emitted - I think you mean that the OLR is little changed when low cloud is present, for given atmospheric conditions [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Change made as suggested.
7-1759	7	58	54	59	3	Again regarding diurnal variation, one might add on line 3: In many regions, low-cloud amount tends to decrease systematically in the course of the day, and if this daytime falloff is reinforced in a warming climate, the effective decrease will be stronger. For (convective) high clouds, changes in the diurnal cycle will affect the SW; they will also interact with changes of the surface temperature and low-cloud diurnal cycle in the same region to affect the outgoing LW. [Robert Kandel, France]	Rejected. While this is a plausible effect, it would be speculative at this stage and canot be included in a FAQ without support in the main chapter text.
7-1760	7	58	55	58	57	You are implicitly talking about optically thick high clouds here. But there are a lot of thin cirrus up there near the tropopause that do not reflect much sunlight while absorbing significant thermal IR, re-radiating at a colder temperature, and thus having a net warming effect. As discussed in an earlier comment, those thin cirrus are poorly represented in GCMs and thus contribute a highly uncertain amount to cloud feedback (even though the GCMs themselves do not get a large feedback effect, the consensus in this case should not be confused with understanding). Consequently we should not conclude that they contribute less to climate change just yet. [Anthony Del Genio, United States of America]	The text has been edited in response to this comment.
7-1761	7	58	56	58	56	Replace "emitted infrared radiation" with "infradred radiation emitted to space"? [Francis Zwiers, Canada]	Change made as suggested.
7-1762	7	59	5	59	5	Same comment - surface T is fairly insensitive to changes in high clouds in the current generation of GCMs, but we should have low confidence in that until we simulate thin cirrus with better fidelity and physical realism. [Anthony Del Genio, United States of America]	The clause has been removed.
7-1763	7	59	8	59	9	As discussed in an earlier comment, the positive feedback is mostly due to preventing additional IR emitted by the middle troposphere, rather than IR emitted from the surface, from leaving the climate system. Water vapor already prevents most of the surface radiation from leaving the climate system. [Anthony Del Genio, United States of America]	The wording has been changed.
7-1764	7	59	11	59	11	Insert "also" after "There are" [Francis Zwiers, Canada]	Change made as suggested.
7-1765	7	59	12	59	12	"Changing" should be "increasing" - we understand the sign of the change. [Anthony Del Genio, United States of America]	Change made as suggested.
7-1766	7	59	24	63	4	FAQ 7.3 (Geoengineering): I suggest that somewhere in this FAQ it would be useful to make the point that SRM methods would not offset the ocean acidification effect of continuing anthropogenic CO2 emissions. [David Wratt, New Zealand]	Taken into account - It is discussed in the 3rd para from bottom of this FAQ
7-1767	7	59	26	59	26	If these FAQs are intended to be self-contained statements for people who don't want to read the rest of the chaper, then you need to have a better definition of what aerosols are, since clouds are also small particles suspended in the atmosphere. You discuss the distinction at the start of the chapter, but I think it needs to be stated here as well. [Anthony Del Genio, United States of America]	Accepted.
7-1768	7	59	26	59	30	Suggest adding future-oriented information to the end of this italicized paragraph in order to keep the format consistent with the question on clouds in the section above. [Government of Canada]	Accepted. A short sentence was added to the chapeau.
7-1769	7	59	27	59	27	What does an "equivocal way" mean here? Suggest rewording if possible. [Government of Canada]	Accepted. Reworded.
7-1770	7	59	28			The sentence "Overall, it seems…" could be reworded to avoid the word "seems". [Jan Fuglestvedt, Norway]	Accepted. Reworded.
7-1771	7	59	28			Deposited aerosol particles have substantial climate effects that should be mentioned here: Snow/ice albedo, and fertilisation of terrestrial and marine ecosystems [Jost Heintzenberg, Germany]	Rejected. We need to keep the FAQ simple.
7-1772	7	59	29	59	29	FAQ 7.2 : « aerosols have exerted a cooling influence on the Earth » ...over the past century. [Sandrine	accepted but with a slightly different wording.

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						BONY, France]	
7-1773	7	59	29	59	29	The phrasing seems to imply that aerosols have only acted in the past and are not exerting a cooling influence now. It might be useful to give an indication of the trend in their influence. [Michael MacCracken, United States of America]	partly taken into account by adding the period of cooling.
7-1774	7	59	29	59	30	For the comma onwards, I suggest rewriting this as "which has offset some of the warming that would have occurred in their absence." (The forcing from GHGs is still there …). [Francis Zwiers, Canada]	accepted but with a slightly different wording.
7-1775	7	59	30	59	30	This chapeau text is missing some pointer to what is projected for the future, as was provided in the response to FAQ 7.1. You could lift the text from page 60, lines 22 - 33, and repeat them here in the chapeau. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
7-1776	7	59	33	59	35	The term "smoke" is not specific enough in this context. [Andrew Ferrone, Germany]	rejected. It is specific enough in the context of an FAQ and technically correct.
7-1777	7	59	34	59	34	Smoke is mentioned as example of aerosols. Please check the statement. Perhaps soot or BC/OC is meant. [Government of Germany]	Rejected. Smoke consists of aerosol particles from combustion processes.
7-1778	7	59	34			why there is smoke, should be biomass burning [European Union]	rejected. The definition of smoke is "The vaporous system made up of small particles of carbonaceous matter in the air, resulting mainly from the burning of organic material, such as wood or coal" and is appropriate to use in a FAQ.
7-1779	7	59	42	59	42	Replace "predicting" with "determining". I think it would help lay readers some frequently used words are used in a common way across the FAQs. For example I think it would be helpful to restrict the use of words like predict and project specifically to contexts where predictions or projections of future states are being discussed. [Francis Zwiers, Canada]	Accepted.
7-1780	7	59	42	59	45	As written, it sounds as though the uncertainties around soot RF (lines 43-45) are not accounted for in the conclusion of lines 42-43 which states that "most studies agree that the overall radiative effect from anthropogenic aerosols is to cool the planel." Please be clear about this as it is important. Is this conclusion robust to uncertainties around soot RF or not? Could revised estimates of soot RF change the sign of the net aerosol effect or is it more likely to just change the magnitude of that effect? [Government of Canada]	taken into account. We have swapped the two sentences. The conclusion is robust to uncertainties around the soot RF (see also Chapter 10 and Chapter 13 box).
7-1781	7	59	43	59	45	It may be added that the direct effect of absorbing aerosols is very likely positive (see ch8) [Jan Fuglestvedt, Norway]	Rejected. It would make the sentence somewhat cumbersome. This is mentioned at the beginning of the paragraph and well explained in the figure.
7-1782	7	59	43	59	45	The last sentence does not read well, I'd suggest revising. [Peter Irvine, Germany]	accepted. Small change in words.
7-1783	7	59	44	59	44	Suggest clarifying the linkage between soot and black carbon here. [Government of Canada]	accepted. Soot has been changed to "black carbon" as the term has entered the public debate.
7-1784	7	59	45			"...soot, induces a specific cloud response." This sentenceis confusing. Instead of causing more questions than answers, perhaps it is better to state clearly why soot is so different [Government of United States of America]	rejected. The cloud response is important and discussed in the chapter. The difference between scattering and absorbing aerosols is discussed at the beginning of the paragraph.
7-1785	7	59	45			Absorbing aerosols affect the stability of the atmosphere irrespective of clouds [Jost Heintzenberg, Germany]	rejected. Although the reviewer is right, this is too technical for an FAQ and radiatively it is the change in clouds that matters.
7-1786	7	59	47	59	50	Figure 1 is trivial and doesn't give any additional information: delete; btw: what happens if you have both types of particles? At the same location or at different altitudes? [Andrea Flossmann, France]	rejected. This level of information is appropriate for an FAQ. Discussing the effect of multiple (scattering and absorbing) aerosol layers is beyond the scope of the FAQ.

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7-1787	7	59	47	59	50	Is "aerosol-radiation" should be "aerosol – solar radiation . Some aerosols interact in IR. [Government of Poland]	accepted.
7-1788	7	59	53	59	53	"you might assume" is slightly funny wording. I certainly wouldnt make the assumption given here. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	accepted. Wording changed.
7-1789	7	59	53	59	53	FAQ 7.2: The way the reader is addressed, in this case " You might assume..." is informal and should be avoided. Please consider to rephrasing this and other similar statements and avoid a too casual language style. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Wording suggested by science editor has been changed.
7-1790	7	59	53			why would anyone assume that increase number of CCN would give increasing amount of low clouds? Noone is so stupid. [Andrea Flossmann, France]	taken into account and reworded. Increasing CCN number concentrations leading to increasing amount of low clouds is the basis of the Albrecht's effect.
7-1791	7	59	54	59	54	Please clarify why more CCN would increase only the amount of low cloud not high cloud. Presumably this has to do with the altitude of the aerosol layer. [Government of Canada]	partly taken into account. This is because only the role of aerosols as CCN is considered here.
7-1792	7	59	55	59	56	This sentence is not correct. There is no physical reason, while aerosols should generally produce liquid clouds. As section 7.4.4 shows, aersols also have an impact on ice-clouds. [Andrew Ferrone, Germany]	taken into account. The sentence was reworded to indicate that this was an early view.
7-1793	7	59	55	59	56	This sentence is a little bit confusing. I would suggest to chang part of it from "more aerosols generally produce liquid clouds, which are brighter" to "more aerosols generally produce brighter liquid clouds". [Minghuai Wang, United States of America]	accepted.
7-1794	7	59	55	59	56	I thnk this needs a few more words. Perhaps the sentence beginning with "A robust result…" could be rewritten as "A robust result is that clouds of liquid water droplets tend to have more, but smaller, droplets when there are more aerosols, which makes these clouds brighter when viewed from above. [Francis Zwiers, Canada]	taken into account, but reworded in a slightly different way than proposed by the reviewer.
7-1795	7	59	55			liquid water cloud (not liquid cloud!) [Andrea Flossmann, France]	accepted.
7-1796	7	59	57	59	57	Replace "patheways for" with "types of"? [Francis Zwiers, Canada]	Rejected.
7-1797	7	60	1	60	1	Insert "between liquid and ice" to remind layers readers what phase change refers to. [Francis Zwiers, Canada]	accepted but with a slightly different wording.
7-1798	7	60	5	60	7	Figure 2: same comment as figure 1; if you really want one, you can do better than these kindergarten sketches [Andrea Flossmann, France]	accepted. Figures have been redrawn by graphical editor.
7-1799	7	60	5	60	7	In the right panel of Figure 2, the reduced size of cloud drops should be associated with suppressed or delayed rainfall. Therefore, the illustrated rain should be suppressed or at least shown lighter than the rain in the left panel. [Daniel Rosenfeld, Israel]	rejected. As the FAQ does not discuss precipitation changes, we prefer not showing a precipitation change (one way or the other) on the figure.
7-1800	7	60	14	60	14	Maybe include a few words to explain "gaseous precursors" in lay language. [Francis Zwiers, Canada]	accepted.
7-1801	7	60	15	60	15	The calibrated uncertainty language "very likely" should be avoided in an FAQ. See also line 22. [Thomas Stocker/ WGI TSU, Switzerland]	accepted.
7-1802	7	60	17	60	17	Suggest inserting "that enter the stratosphere" after "volcanic eruptions". [Francis Zwiers, Canada]	accepted.
7-1803	7	60	17	60	18	FAQ 7.2: suggest to add the years for both volcanic eruptions mentioned (1982, 1991); suggest to add a time frame for the induced "sporadic cooling periods". [Thomas Stocker/ WGI TSU, Switzerland]	accepted.
7-1804	7	60	20			AP emissions decrease, thus the RF decreases; however: humidity increases!! Particles get bigger (more hazy). I haven't found this discussion. Is this an important effect? I think: yes, very. [Andrea Flossmann, France]	rejected. At zeroth order, relative humidity stays constant, as discussed in the water vapour feedback section.
7-1805	7	60	21	60	22	One of the first observations noted in the current draft of the SPM of this report is that "… the warming has been particularly marked since the 1970s". There is thus a controdiction with the sentence here saying that "… the global impact of aerosols has been to cool or warm the planet". Repharse such as to reflect that some of	rejected. The statement is about the last two decades (typically 1990 to 2010) andoes not refer to the period starting in 1970.

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						the observed wamring due to GHGs could be masked by aersols. [Andrew Ferrone, Germany]	
7-1806	7	60	22	60	23	The basis as well as the underlying time horizon for this statement are not clear. [Government of Germany]	taken into account. The calibrated uncertainty language has been removed. There is evidence in the report for a stabilisation of aerosol loadings (Ch2) and a decrease in aerosol emissions (Ch 11/12). We have clarified that this relies on projections.
7-1807	7	60	22			FAQ 7.2: "It is very likely" does very likely have the same meaning in an FAQ as it should in the rest of the IPCC synthesis? [Government of United States of America]	accepted, the calibrated uncertainty language has been removed.
7-1808	7	60	23	60	23	It would be useful to explain why it is very likely that emissions of anthropogenic aerosols are expected to decrease (i.e. these are air pollutants that are damaging to the environment and human health). [Government of Canada]	accepted.
7-1809	7	60	23	60	23	Suggest inserting "effectively" ahead of "augmenting". [Francis Zwiers, Canada]	accepted.
7-1810	7	60	23			"augmenting greenhouse-gas induced warming". No. Either "no longer masking greenhouse-gas induced warming" or "Augmenting warming." [Stephen E Schwartz, United States of America]	accepted but with a slightly different wording.
7-1811	7	60	25	63	2	The whole FAQ gives the wrong impression, that GE methods are already at hand. Please check the whole section carefully and remove this impression. [Government of Germany]	Taken into account - text revised
7-1812	7	60	27			Section FAQ 7.3: Unkowns are not emphasized in this Q and A section, I think more material on the uncertainties in the effectiveness, side-effects, etc. is needed [Peter Irvine, Germany]	Taken into account - text revised
7-1813	7	60	27			section FAQ 7.3: The potential for SRM to be more complicated than a simple "anti-global warming" technique are not mentioned. There is great (and terrible?) potential for SRM to be used as a means to engineer the climate in a myriad of ways. This may be outside of the remit of this section but the potential for goals other than mitigating climate change should be mentioned. [Peter Irvine, Germany]	Rejected. This is beyond the scope of the report.
7-1814	7	60	27			section FAQ 7.3: There is no mention of the potential for winners and losers or any of the political implications of these ideas. However, I acknowledge that this may not be the point of this section. [Peter Irvine, Germany]	Noted - As indicated by the reviewer, WG1 would not discuss political implications. We now explicitly state this.
7-1815	7	60	27			FAQ 7.3: This is a very relevant part of chapter 7 as well as of Working Group I Report. The information provided is well presented. However, it is suggested to include some more aspects of the two main geoengineering approaches SRM and CDR. Some missing aspects are: a.) SRM and CDR complement with regard to very relevant features each other - SRM has the potential to reduce temperature increase in the short term, quite quickly but needs to be implemented over a very long time period (longer than thousand years!!), whereas CDR contributes (like mitigation) only after several decades but could bring bring back atmopsheric CO2 concentrations to even pre-industrial level within a few centuries; b) however, even if we deploy a combination of both, CDR and SRM, this cannot avoid significant impacts of greenhouse gas emissions due to climatic and environmental side effects of both approaches. This should reinforce the strong need to avoid greenhouse gas emissions asap and as much as possible if Article 2 of the Convention should be fulfilled. From that perspective it is expected that WG III mreport should inform about the temperature rise to which we are already committed due to the inertia of natural and human systems. c). It should be clarified that the buffering that is an important aspect of CDR has the important side effect, that it also helps to address ocean acidification. [Klaus Radunsky, Austria]	partly taken into account - item a) asks that we elaborate on the complementarity of the CDR and SRM approaches. We now mention the timescales issues for CDR and SRM. The text has been revised in small ways to clarify these issues. item b) The request to make recommendations about article 2 of the convention is beyond the scope of this report and is rejected. item c) whether or not CDR mitigate ocean acidification depends on where the carbon is stored. This is not the case eg with iron fertilization.
7-1816	7	60	27			It would be helpful if WG II report also addresses thresholds and informs to the extent possible about the impacts of ocean acidification. d) The aspect of the regional differences in the effectiveness of SRM that are linked to the regional solar radiation that varies in addition also with time is also not addressed in enough detail. It would be intersting to inform how well all those aspects are represented in current models. It is suggested that the WG III report also informs about the implications of the carbon cycle on carbon accounting and about the risks of introducing perverse incentives. [Klaus Radunsky, Austria]	Rejected - Space is limited and the FAQ is not the place to go in a lot of details. This comment also makes recommendations relevant to the WGII and WGIII reports which cannot be implemented in this report.
7-1817	7	60	29	63	2	FAQ 7.3. This is as much a subject for WG2 and WG3 as for WG1. Its treatment here is important, especially	Noted, no change is made - This comment makes

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						in that it reminds us that geoengineering, especially SRM, has global but regionally diverse effects. Inadvertent geoengineering by GHG emissions is historically entrenched and so far not effectively subject to international law, the Kyoto Protocol notwithstanding. However, one might argue that new national programs of deliberate geoengineering, even when carried out over a nation's own territory or international waters, should be subject to international agreement, since some of the regional effects could well be judged negative by one or more other nations. [Robert Kandel, France]	recommendations relevant to WGII and WGIII, as well as for the Synthesis report.
7-1818	7	60	29			I think the section on geoengineering is well written. [Daniel Murphy, United States of America]	Noted. Thanks.
7-1819	7	60	29			FAQ 7.3: This FAQ would also benefit from including some quantitative information coming out of the chapter assessment, egg, provide numbers which provide evidence for the effectiveness of the different Geoengineering methods and the potential side effects. [Thomas Stocker/ WGI TSU, Switzerland]	a figure has been added that builds on the discussion of the terminantion issue in Section 7.7.
7-1820	7	60	29			FAQ 7.3: It would be good to see more emphasis (including any quantitative information) given to the long-term commitment associated with SRM, which currently appear late (final two paragraphs) of the FAQ. [Thomas Stocker/ WGI TSU, Switzerland]	the issue of timescale is now emphasized in the chapeau. There is not much quantitative yet in the scientific literature on long-term commitment for SRM. It is inevitably linked to particular scenario and it is therefore more relevant to WGIII.
7-1821	7	60	29			FAQ 7.3: We would really like to see FAQ figures 1 and 2 combined into a single figure. This would then allow a second quantitative figure to be added, provided that such information is available from the Chapter SOD. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Figure 1 and 2 have been combined. A new figure has been added to present information about the "termination effect".
7-1822	7	60	31	60	31	The Glossary does not refer the "large-scale" interventions. Please harmonize. [Government of Germany]	Accepted. We have harmonized the definition of geoengineering in Chapter 6, 7, the FAQ, and the glossary.
7-1823	7	60	31	60	32	"Geoengineering" and "climate engineering" cannot be used as synonoyms. "Climate engineering" can be considered a subset of "Geoengineering", but the latter one also include technics that are not related to climate. See Feichter, Johann/Leisner, Thomas (2009): Climate engineering: A critical review of approaches to modify the global energy balance. European Physical Journal-Special Topics 2009, 176, 81-92. [Andrew Ferrone, Germany]	Rejected. It is correct that some authors make a difference between the two terms, however there is no agreed definition yet and IPCC currently refers to geo-engineering to mean climate engineering.
7-1824	7	60	31	60	32	The definition for geoengineering here differs from the one in the Glossary. Suggest making consistent to the extent possible in order for the IPCC to assist in working towards a commonly known definition. [Government of Canada]	Accepted, we have harmonized the definitions.
7-1825	7	60	31	60	32	not only the effects of green house gases, see definition in the Glossary or ch 7, p 53, l 39. [Government of Germany]	Accepted - text revised
7-1826	7	60	31	60	32	not only the effects of green house gases, see definition in the Glossary or ch 7, p 53, l 39. [Government of Germany]	Accepted - text revised
7-1827	7	60	31	60	32	Is this the agreed IPCC AR5 definition for geo-engineering? If so, it should be included in the SPM. If it is not, then better not to say "… is defined as…" but "…may be defined as…", or add something like "for the purposes of this report, we defined geo-engineering as...". [Government of United Kingdom of Great Britain & Northern Ireland]	Partly taken into account. There is no agreed AR5-wide definition for geoengineering. The AR5 WGI definitions have been harmonized
7-1828	7	60	31	60	32	The definition given here differs from the one given in the main text, I'd recommend using the definition used in the main text. "manipulation" is an evocative word that implies sophisticated (and perhaps malign) control, whereas intervention is more neutral. [Peter Irvine, Germany]	Accepted - text revised
7-1829	7	60	31	60	36	Please add a sentence on the general lack of knowledge of GE methods and their risks and the need of further research. [Government of Germany]	Accepted - text revised
7-1830	7	60	31	60	36	Please add a sentence on the general lack of knowledge of GE methods and their risks. [Government of Germany]	Accepted - text revised
7-1831	7	60	31	63	2	FAQ 7.3 Carbon Dioxide Removal Methods: This section should be reviewed by Chapter 6 authors. Should	Accepted - text revised. Chapter 6 authors do

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						PgC be defined somewhere? [Government of United States of America]	contribute to the FAQ.
7-1832	7	60	32	60	32	This gives one motivation for geoengineering, but you could imagine other objectives, such as maximizing global food production. [Francis Zwiers, Canada]	Noted - In the context of IPCC assessment, geoengineering refers only to schemes that would counteract climate change
7-1833	7	60	35	60	36	The phrasing of this last sentences seems biased to me toward the risks of geoengineering without comparably indicating the risks of further global warming. Based on the research of the scientific community, the international community is being asked to completely changeover their primary energy source, which supplies over 80% of the world's energy. International leaders have said it is "dangerous" to go over 2 C warming, and quite a number of scientists would argue that that numberr is too high, that there would be substantial loss of biodiversity, that it might trigger a strong natural carbon feedback and the loss of a large fraction of the ice sheets, requiring relocation of the world's cities. There is no hint at all that the potential side effects of geoengineering are anywhere near this disastorous. While geoengineering cannot offset near all of these consequences, if it could offset even a modest fraction, the benefits would be far greater than any indication of the unintended consequences of geoengineering, or at least most aspects of it and this question is really related to both. Now, there are governance and ethical issues that are appropriately raised elsewhere and some approaches have some important potential downsides, but to phrase the overall balance in this way is, in my view, grossly unproportional. There are also questions about whether at least some of the technologies that could create a significant enough effect to make a difference would even work, so that is also an important risk, at this point. [Michael MacCracken, United States of America]	Accepted - we have updated the sentence. WG1 report is not charged with a risk assessment which is done in Chapter 19 of WG2 report, so we merely document that geoengineering produces risks and side effects without quantifying their relative importance.
7-1834	7	60	36	60	36	Is this an assessed "likely"? [Francis Zwiers, Canada]	Noted - Text is revised
7-1835	7	60	38	61	48	The chapter on CDR methods is interesting but unfortunately out of subject in the "cloud and aerosol" chapter 7. Maybe there is another place for it elsewhere? [Andrea Flossmann, France]	Rejected. This is an editorial decision of the TSU of the IPCC.
7-1836	7	60	40	60	40	The expression "are designed to remove" suggests that such methods exist, which is not the case. Please reformulate "aim at removing" [Government of Germany]	Accepted - text revised. Note however that reforestation / afforestation already exist and part of UNFCCC. Prototypes of chemical CDR exist.
7-1837	7	60	40	60	40	I'd suggest replacing the "often" from "often through a manipulation…" as there is no reference to time here but separate schemes. I'd suggest replacing the "often" with "many would achieve this". [Peter Irvine, Germany]	Accepted - sentence revised to acknowledge both
7-1838	7	60	40	60	48	The section implies that CO2 storage is a minor issue of CDR when in fact it is one of its major problems. Please change wording. [Government of Germany]	Noted - This is only the introductory para. The 3rd paragraph of CDR discussion in this FAQ discusses the storage capacity and permanence vs. non-permanence issues
7-1839	7	60	40	61	48	Need to ensure this is consistent with Chapter 6 which does not consider all CDR methods to be geo-engineering on the basis of differing scale. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted. Chapter 6 LA contribute to the FAQ.
7-1840	7	60	40	61	48	Unlike the other FAQs, the underlying basis for the material on CDR, including appropriate references, is not part of the chapter itself. I'm wondering whether this material belongs in some other chapter, where the associated technical discussion is presented. [Ralph Kahn, United States of America]	Noted. Material for the CDR part of the FAQ lies with Chapter 6.
7-1841	7	60	41	60	41	The storage should be long-term or permanent, and without negative effects on the environment, this information must be added here in the overview section please, not only on the next page. [Government of Germany]	Rejected. This section is not an overview but an introduction. The FAQ should avoid too much repetition. We discuss the capacity and permanence vs non-permanence issues later.
7-1842	7	60	42	60	42	The statement that CO2 removed through CDR methods would have little to no impact on the Earth's energy budget presumably is only valid if the carbon stays in the reservoirs where it is put. For biological storage, this is questionable. Suggest adding to the end of this sentence a caveat to make this clear: "..as long as the carbon remains in these reservoirs". [Government of Canada]	Accepted - text revised

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7-1843	7	60	51	60	52	Suggest removing "to remain effective at counteracting global warming." from sentence [Chareles Thomas (Tom) McElroy, Canada]	Accepted - text deleted
7-1844	7	60	52			Explanation of line 6 comment: People who wish to push the positive view of SRM will take things out of context, so careful editing to remove apparent 'endorsements' of SRM is important. CTM [Chareles Thomas (Tom) McElroy, Canada]	This comment refers to page 62. Noted -- We have attempted to avoid endorsements.
7-1845	7	60	53			Suggest adding "Until such time as a comprehensive plan to reduce greenhouse gases in the atmosphere is agreed [Chareles Thomas (Tom) McElroy, Canada]	This comment refers to page 62. Rejected. The suggested addition is not accurate. Another path would be to use CDR.
7-1846	7	60	53			upon and put into practice, the extent of SRM required and the time line for its application will remain unknown. [Chareles Thomas (Tom) McElroy, Canada]	This comment refers to page 62. Noted. This can be inferred from the sentence.
7-1847	7	60	53			Therefore, until such a plan is put in place, the eventual, negative impact of SRM cannot be estimated. [Chareles Thomas (Tom) McElroy, Canada]	This comment refers to page 62. No Change is made.
7-1848	7	60	53			The use of sulphate aerosol in the stratosphere to modify the Earth's albedo would eventually lead to a significant [Chareles Thomas (Tom) McElroy, Canada]	This comment refers to page 62. Rejected. In the final draft, this is discussed in 3rd para from bottom of this FAQ
7-1849	7	60	53			deposition of acidic preciptation at middle to high latitudes. [Chareles Thomas (Tom) McElroy, Canada]	This comment refers to page 62 See the response to the previous comment
7-1850	7	61	4	61	4	Add: Nevertheless knowledge about other risks is still low [Government of Germany]	Accepted - This point is now made more strongly in the first para of this FAQ conveys this message.
7-1851	7	61	8	61	13	There are intensive discussions about the potential risks of storage in geological formations due to CO2 escaping into the adjacent rock, or back into the atmosphere. Please provide information on these issues. (IPCC reports should be comprehensive) [Government of Germany]	Rejected - This risk is folded into our discussion of "permanence vs non-permanence" which is one of the science considerations for CDR methods. The potential risks of CO2 storage in geological formations is a common issue to CCS (see special IPCC report on the subject).
7-1852	7	61	11	60	12	I think it is key to separate geological reservoirs into at least two categories: the well-known and understood fossil fuel reservoirs and the less well understood deep saline aquifers and any others. I think a cautionary note should be sounded here, the magnitude of the well understood fossil fuel reservoirs is much smaller than the highly uncertain and unfamiliar saline aquifers. [Peter Irvine, Germany]	Rejected - It is felt that it is too much of a detail for FAQ. The term "Geological reservoirs" covers both categories and hence this term is used here.
7-1853	7	61	17	61	18	"reduced or even reverse…" - should be "reversed" for consistency [Mark Lawrence, Germany]	Accepted - text simplified.
7-1854	7	61	17	61	20	The content of these lines is extremely important and needs to be conveyed very clearly. As written, it is not clear. Please explain why uptake of carbon by land and ocean sinks would be reduced by implementation of CDR methods and why CDR methods would need to remove not only the accumulated CO2 in the atmosphere but also that previously taken up by land and ocean sinks. This has huge ramifications, it would seem, for the efficacy of CDR and needs to be further explained. [Government of Canada]	Partly taken into account - Text has been rephrased. Underlying material for the FAQ lies in Chapter 6, as indicated at the top of the FAQ. The FAQ cannot go in all the details.
7-1855	7	61	18	61	18	"to offset" => "to completely offset" [Mark Lawrence, Germany]	Accepted - text revised
7-1856	7	61	20			Why? [Jost Heintzenberg, Germany]	Partly taken into account - Text has been rephrased. Underlying material for the FAQ lies in Chapter 6, as indicated at the top of the FAQ. The FAQ cannot go in all the details.
7-1857	7	61	22	61	32	These paragraphs are a little confusing and should be reviewed. Is the first sentence referring to "biological weathering" and "chemical weathering" or is it referring to "biological CDR methods" and "chemical weathering CDR methods"? The concept of "buffering" is not well-explained so suggest elaborating on this here or in the preceding paragraph. The final sentence on direct air capture appears like an after thought and requires further explanation. Changing between PgC and ppm is also hard to follow in this paragraph and the following	Taken into account - Text has been rephrased

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						one. [Government of Canada]	
7-1858	7	61	24	61	25	This seems like an under-statement of the problems that demands for land could impose, e.g. agriculture, wild lands, etc. etc. [Peter Irvine, Germany]	Noted - It is felt that the point is made.
7-1859	7	61	25	61	25	Please specify "buffering". [Government of Germany]	Taken into account - text revised
7-1860	7	61	27	61	30	This point seems a little confused. If the maximum rate is 100 PgC per century then it is not merely difficult to mitigate climate change rapidly, it is impossible. If the supposed maximum rate is some multiple of this, then it is still impossible to mitigate rapidly, except in geological terms. [Peter Irvine, Germany]	Noted - The point is already made. It is felt that the term "mitigate climate change rapidly" is appropriate because of phrase "—even for a suite of biological methods—" in this sentence
7-1861	7	61	27	61	30	the suite of schemes would surely be competing for the same scarce land resources? [Peter Irvine, Germany]	Noted - already mentioned on lines 24 and 25
7-1862	7	61	32	61	32	Add: "Moreover the question of CO2 storage has to be solved" [Government of Germany]	Rejected - This is certainly an issue but this technological issue (which is also the case for carbon capture and storage) need not be discussed in this FAQ on the science of geoengineering.
7-1863	7	61	34	61	34	Please add that some side effects might still be unkwown at present time. [Government of Germany]	Noted - this message is conveyed in the FAQ chapeau and in the 2nd line of last paragraph on CDR where side effects of iron fertilization is discussed.
7-1864	7	61	34	61	34	Suggest inserting "While the risk is believed to be low, " ahead of "CDR could have", so as to link back to the statement at the top of this page. [Francis Zwiers, Canada]	Rejected. This is not known.
7-1865	7	61	34	61	35	This sentence doesn't explain the impact on plant productivity sufficiently for a non-expert audience to understand. Consideration could be given to deleting this sentence as it is not a relevant comparison for the discussion of unintended effects. [Government of Canada]	Accepted - sentence deleted
7-1866	7	61	34	61	35	This argument is not convincingly, from our point of view. It does not reflect the really important side effects of CDR and should be deleted or mentioned at the end. [Government of Germany]	Accepted - sentence deleted
7-1867	7	61	34	61	35	the first side effect would also apply to mitigation, and seems not to be a side-effect, rather the direct purpose of CDR. [Peter Irvine, Germany]	Accepted - sentence deleted
7-1868	7	61	34	61	39	Please add the information that the potential for some CDR methods where plant act as a carbon sink, might be limited by the area available for plantations. [Government of Germany]	Rejected - not relevant to the discussion here
7-1869	7	61	41	61	48	The points made about macronutrient loss applies to iron fertilization but does not apply to the idea to add these macronutrients directly. [Peter Irvine, Germany]	Accepted -text revised
7-1870	7	61	44	61	44	Is this an assessed "likely"? If not, perhaps another word could be used so that the interpretation of key language does not become muddled in the minds of lay readers. [Francis Zwiers, Canada]	Noted - It is not a "assessed" likely, text revised
7-1871	7	61	45	61	45	"have impact" => "have an impact" [Mark Lawrence, Germany]	Accepted - text revised
7-1872	7	61	46	61	46	Is DMS explained anywhere here in the text? Please clarify what it stands for. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - text revised
7-1873	7	61	54	61	54	Suggest replacing "make the atmosphere opaque… radiation is emitted" with "slow the rate at which infrared radiation is emitted back to space", so that this remains as accessible to lay readers, and to avoid introduction additional terms such as terrestrial radiation. [Francis Zwiers, Canada]	Partly taken into account - text revised. However this explanation is not correct. The rate remains the same once climate has responded.
7-1874	7	61	54	61	57	I think this needs to be made more accessible to lay readers. Here is an attempt - although I'm sure the authors can do this better: "If less incomnig sunlight is absorbed and converted to heat because the planet has been made more reflective, or if heat (infrared energy) can be emitted to space more effectively, the average global surface temperature will be reduced. [Francis Zwiers, Canada]	Partly taken into account - text revised. However better to avoid term "heat".
7-1875	7	62	2	62	2	The wording "which manage" is not appropriate as such methods do not yet exist. Instead, please write	Taken into account - text revised

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						"aiming at managing". [Government of Germany]	
7-1876	7	62	2	62	2	Suggest saying something like "Suggested geoengineering methods that manage this balance between incoming and outgoing flows of energy, the Earth's radiation budget, are based on …." [Francis Zwiers, Canada]	Taken into account - text revised
7-1877	7	62	4	62	4	sunshade mentioned for the first time, would it be better to highlight sulphate aerosol scheme? [Peter Irvine, Germany]	Phrase about mirrors deleted
7-1878	7	62	5	62	5	The FAQ on clouds refers to "high" and "low" clouds rather than specific cloud types like "cirrus". Suggest making the explanations consistent between the FAQs so that they complement each other. [Government of Canada]	Taken into account - text revised
7-1879	7	62	14	62	15	"net flux change of energy…will cool" => "net flux increase of energy…will cool" (a change could go in either direction, cooling or warming) [Mark Lawrence, Germany]	Taken into account - text revised
7-1880	7	62	19	62	27	This sections suggests that there is validated knowledge of SRM techniques at hand. Please reformulate. [Government of Germany]	Rejected. This paragraph just says that the global temperature responds to the energy budget, and that other factors are also important. This is certainly a valid statement. It does not say that any particular SRM method will be successful, and the following sentences and paragraphs highlight the uncertainties.
7-1881	7	62	19			I think the sentence could be reworded so "rather straighforward way" is not used. [Jan Fuglestvedt, Norway]	Accepted - text revised
7-1882	7	62	23	62	24	"some influence": Please delete "some" as this expression plays down potential effets and is therefore not appropriate. [Government of Germany]	Accepted - text revised
7-1883	7	62	26	62	27	This is a little too imprecise given the evidence now available (and discussed in the chapter, suggest rewording from "will cool less than others and…" onwards to read: "will cool less than others; in particular there is evidence that if anthropogenic greenhouse warming were completely compensated by statospheric aerosols, then polar regions would be left with a small residual warming, while tropical regions would become cooler than in preindustrial times." [Mark Lawrence, Germany]	Accepted - text revised
7-1884	7	62	30			I think "we know" sounds a bit too strong. [Jan Fuglestvedt, Norway]	Rejected but slightly reformulated.
7-1885	7	62	33	62	34	the list of other climate features excludes: storms, the statistics of climate and extremes which may all change under SRM [Peter Irvine, Germany]	Accepted - text revised
7-1886	7	62	35	62	36	"will not counter the globally-averaged surface temperature and rainfall equally", this could be phrased better. [Peter Irvine, Germany]	Accepted - text revised
7-1887	7	62	37	62	37	Is this an assessed "very unlikely"? [Francis Zwiers, Canada]	Noted -- we changed "very unlikely" to "improbable" to avoid the use of calibrated language.
7-1888	7	62	38	62	41	Also here effects on the terrestrial biosphere need to be mentioned. All model experiments done with climate models only provide only limited information since coupling with the terrestrial C/N cycles must be considered. I doubt that this has been fully explored and, thus, the picture given is likely to be too positive [European Union]	Accepted - text revised
7-1889	7	62	38			I think the sentence could be reworded so "just like" is not used. [Jan Fuglestvedt, Norway]	Accepted - text revised
7-1890	7	62	39	61	41	Please add that information is gathered from current climate models. [Government of Germany]	Accepted - text revised
7-1891	7	62	39	62	41	"much closer" is undefined and potentially contentious. I'd suggest adding a cautionary note when describing how "close" the climate would be to a low-CO2 state. [Peter Irvine, Germany]	Accepted. "much closer" changed to "generally closer".
7-1892	7	62	40			Regarding the phrase, "would be much closer to 20th century climate"- is this in terms of global surface temperature? regional precipitation? weather extremes? The authors should clarify the text. [Government of United States of America]	Accepted. "much closer" changed to "generally closer".

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7-1893	7	62	43	62	48	This sections suggests that there is validated knowledge of SRM techniques at hand. Please reformulate. [Government of Germany]	Accepted - text revised
7-1894	7	62	43	62	48	I suggest replacing "will" by "would" in line 43, and "will not" by "would not" in line 47. This is because the wording "SRM techniques will have other side effects …" carries a possible connotation that the Lead Authors expect SRM techniques will be implemented. "Would" is more neutral. [David Wratt, New Zealand]	Taken into account -- text substantially revised.
7-1895	7	62	45	62	45	is "dangerous" needed in front of UV? [Peter Irvine, Germany]	Accepted Small quantities of UV are also needed for life.
7-1896	7	62	45	62	45	Maybe insert "the amount of" ahead of "dangerous ultraviolet" [Francis Zwiers, Canada]	Accepted - text revised
7-1897	7	62	46	62	46	"affect terrestrial ecosystems" this could be expanded on, it may also affect ocean ecosystems. [Peter Irvine, Germany]	Accepted - text revised
7-1898	7	62	46	62	46	Do we know if the effect is large enough that SRM via stratospheric aerosols would have a discernable impact? [Francis Zwiers, Canada]	Accepted - There is lack of sufficient knowledge and hence we use the terms "can" and "may"
7-1899	7	62	47	62	48	As discussed in an earlier comment, the chapter gives mixed messages about whether ocean acidification by SRM is or is not an important issue. [Anthony Del Genio, United States of America]	Noted - Ocean acidification is not caused by SRM but by CO2. It is not offset by SRM. This is mentioned in 3rd para from bottom of this FAQ.
7-1900	7	62	47	62	48	This sentence plays down the risks of ocean acidification. Please add that marine eco-systems and marine life would be at risk for high CO2 concentrations. [Government of Germany]	Accepted - text revised.
7-1901	7	62	50	62	50	"Without CDR" suggests that there would be CDR methods available - or anticipated. This impression must be avoided please. [Government of Germany]	Accepted -text revised
7-1902	7	62	50	62	52	some fraction of the CO2 will persist for tens of thousands of years. [Peter Irvine, Germany]	Noted -- while true, this fact is not central to the sentence. The residual fraction is a small part of the problem
7-1903	7	62	50	62	52	this is only true in the case that SRM is used to maintain a certain temperature or hold back global warming, it could be deployed in other ways [Peter Irvine, Germany]	Noted -- but maintaining a certain temperature or holding back global warming are the only strategies being considered in this FAQ
7-1904	7	62	50	62	53	It should be added that high atmospheric CO2-concentrations lead to further ocean acidification which is an important negative concomitant effect of SRM without CDR. [Government of Germany]	agreed, but this is already covered by the sentences on lines 47 and 48 of page 62, SOD.
7-1905	7	62	56	62	56	FAQ7.3: What does "residual climate change" mean here? [Government of Canada]	Accepted - text revised
7-1906	7	62	56	62	57	the relative benefits of SRM (assuming they outweigh the harms) would also increase with rising CO2 levels. [Peter Irvine, Germany]	Accepted - text revised
7-1907	7	62	56			I suggest adding "enhancement of the" before "greenhouse effect". [Jan Fuglestvedt, Norway]	Taken into account - text revised but used the word "increased" rather than "enhanced" in the sentence.
7-1908	7	62	57	62	57	Should this be mentioned? We don't really know where the tipping points lie, or whether, when it became apparent that we were approaching a tipping point, SRM could be deployed quickly enough and produce suffienct forcing, to avoid crossing the threshold. [Francis Zwiers, Canada]	Accepted - text revised
7-1909	7	63	1	63	2	Please add that some side effects might still be unknown at present time. [Government of Germany]	Taken into account - text revised. This is mentioned in the FAQ chapeau.
7-1910	7	63	1	63	2	The importance of mitigation should be highlighted in the last sentence, i.e. the side-effects are at least in part a function of the mitigation scenario (level of ocean acidity, etc.). [Peter Irvine, Germany]	Noted but our report does not compare and contrast strategies for addressing climate change. We have revised the chapter to indicate that it is a risky strategy, but do not compare it to others..
7-1911	7	63	2	63	2	"staving off climate change" reads a bit too informal and overly dramatic. How about "strategy for preventing	Accepted - changed to "counter" (it is not clear it could

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						climate change". [Thomas Stocker/ WGI TSU, Switzerland]	actually prevent some climate change, even if it were successful)
7-1912	7	63	3	106	63	If my revision on page 36 is adopted, add the following references: Andreae et al., 2004 P.7-64, line 49 Andreae, M. O., D. Rosenfeld, P. Artaxo, A. A. Costa, G. P. Frank, K. M. Longo, and M. A. F. Silva-Dias (2004), Smoking rain clouds over the Amazon, Science, 303, 1337–1342, doi:10.1126/science.1092779. Coakley et al., 2000 P.7-69, line 33 Coakley, J. A., Jr., et al. (2000), The appearance and disappearance of ship tracks on large spatial scales, J. Atmos. Sci., 57, 2765–2778, doi:10.1175/1520-0469(2000)057<2765:TAADOS>2.0.CO;2. Feingold et al., 2003 P.7-72, line 53 Feingold, G., W. L. Eberhard, D. E. Veron, and M. Previdi (2003), First measurements of the Twomey indirect effect using ground-based remote sensors, Geophys. Res. Lett., 30(6), 1287, doi:10.1029/2002GL016633. Ferek et al., 2000 P.7-72, line 59 Ferek, R. J., et al. (2000), Drizzle suppression in ship tracks, J. Atmos. Sci., 57, 2705–2728, doi:10.1175/1520-0469(2000)057<2707:DSIST>2.0.CO;2. Kaufman and Fraser, 1997 P.7-80, line 3 Kaufman, Y. J., and R. S. Fraser (1997), The effect of smoke particles on clouds and climate forcing, Science, 277, 1636–1639, doi:10.1126/science.277.5332.1636. Lebsock et al. 2008 P.7-84, line 9 Lebsock, M. D., G. L. Stephens, and C. Kummerow (2008), Multisensor satellite observations of aerosol effects on warm clouds, J. Geophys. Res., 113, D15205, doi:10.1029/2008JD009876. Li et al., 2011b P.7-84, line 41 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 (article), doi: 10.1038/NGEO1313. Nakajima et al. 2001 P.7-89, line 38 Nakajima, T., A. Higurashi, K. Kawamoto, and J. Penner (2001), A possible correlation between satellite-derived cloud and aerosol microphysical parameters, Geophys. Res. Lett., 28, 1171–1174, doi:10.1029/2000GL012186. Niu and Li, 2012 P.7-90, line 12 Niu, F., and Z. Li, (2012), Systematic variations of cloud top temperature and precipitation rate with aerosols over the global tropics, Atmos. Chem. & Phys., doi:10.5194/acp-12-8491-2012. Qian et al. 2009 P.7-92, line 46 Qian, Y., D. Gong, J. Fan, L. R. Leung, R. Bennartz, D. Chen, and W. Wang (2009), Heavy pollution suppresses light rain in China: Observations and modeling, J. Geophys. Res., 114, D00K02, doi:10.1029/2008JD011575.	Taken into account. The Andreae et al (2004) and Li et al. (2011) studies are now referred to. We can only cite a subset of existing references and have tried to include foundation as well as the most recent studies. The Lebsock et al. Study is cited although in a separate section.

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						Rosenfeld, 1999 P.7-95, line 18 Rosenfeld, D. (1999), TRMM observed first direct evidence of smoke from forest fires inhibiting rainfall, Geophys. Res. Lett., 26, 3105–3108, doi:10.1029/1999GL006066. Rosenfeld, 2000 P.7-95, line 18 (after Rosenfeld, 1999) Rosenfeld, D. (2000), Suppression of rain and snow by urban and industrial air pollution, Science, 287, 1793–1796, doi:10.1126/science.287.5459.1793. Rosenfeld et al., 2001 P.7-95, line 20 Rosenfeld, D., Y. Rudich, and R. Lahav (2001), Desert dust suppressing precipitation: A possible desertification feedback loop, Proc. Natl. Acad. Sci. U. S. A., 98, 5975–5980, doi:10.1073/pnas.101122798. Schwartz et al., 2002 P.7-96, line 39 Schwartz, S. E., Harshvardan, and C. M. Benkovitz (2002), Influence of anthropogenic aerosol on cloud optical depth and albedo shown by satellite measurements and chemical transport modeling, Proc. Natl. Acad. Sci. U. S. A., 99, 1784–1789, doi:10.1073/pnas.261712099. Yuan et al. 2008 P.7-105, line 41 Yuan, T., Z. Li, R. Zhang, and J. Fan (2008), Increase of cloud droplet size with aerosol optical depth: An observation and modeling study, J. Geophys. Res., 113, D04201, doi:10.1029/ 2007JD008632. [Zhanqing Li, United States of America]	
7-1913	7	71	35			The authors should consider including the following reference before line 35: Dubovik, O., Holben, B., Eck, T.F., Smirnov, A., Kaufman, Y.J., King, M.D., Tanré, D., and Slutsker, I., 2002. Variability of absorption and optical properties of key aerosol types observed in worldwide locations. J. Atmos. Sci. 59: 590–608. [Government of United States of America]	Accepted. The paper is now referenced.
7-1914	7	72	34			Add the following reference [Zhanqing Li, United States of America]	Rejected. Reference missing.
7-1915	7	73	28			These points were also made by Ingram (2012) [William Ingram, United Kingdom]	Rejected. Presumably wrong page number.
7-1916	7	74	28	74	28	"in press" => 12, 7351-7363 [Paul Ginoux, United States of America]	Done
7-1917	7	76	11			The authors should consider including the following reference before line 11: Hasekamp, O.P., 2010. Capability of multi-viewing-angle photo-polarimetric measurements for the simultaneous retrieval of aerosol and cloud properties. Atmos. Meas. Tech. 3: 839–851. [Government of United States of America]	Accepted.
7-1918	7	78	14			After 10 months, still no page numbers, but doi 10.1007/s00382-012-1456-3 [William Ingram, United Kingdom]	Done.
7-1919	7	78	15			Out online for 4 months, doi 10.1007/s00382-012-1456-3 [William Ingram, United Kingdom]	Done.
7-1920	7	79	43	79	43	Kandel R, Viollier M. Observation of the Earth's radiation budget from space. C. R. Geoscience (2010), doi:10.1016/j.crte.2010.01.005 [Robert Kandel, France]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1921	7	81	33	81	33	Include reference to: Kirkevåg, A., T. Iversen, Ø. Seland, C. Hoose, J. E. Kristjánsson, H. Struthers, A. M. L. Ekman, S. Ghan, J. Griesfeller, E. D. Nilsson, and M. Schulz: Aerosol-climate interactions in the Norwegian Earth System Model – NorESM. Geosci. Model Dev. Discuss., 5, 2599-2685, 2012.doi:10.5194/gmdd-5-2599-2012 [Trond Iversen, United Kingdom of Great Britain & Northern Ireland]	Done

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7-1922	7	81	51			IThe authors should consider including the following reference before Knopf and Koop 2006: Knobelspiesse, K., Cairns, B., Mishchenko, M., Chowdhary, J., Tsigaridis, K., van Diedenhoven, B., Martin, W., Ottaviani, M., and Alexandrov, M., 2012. Analysis of fine-mode aerosol retrieval capabilities by different passive remote sensing instrument designs. Opt. Express 20: 21457-21484. [Government of United States of America]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1923	7	84	2	84	3	Insert the reference "Lawrence M. G., and Lelieveld, J., 2010. Atmospheric pollutant outflow from southern Asia: a review, Atmos. Chem. Phys. 10: 11017-11096." [K KRISHNA MOORTHY, INDIA]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1924	7	84	13	84	13	Add the following reference: Lee, K.-H., Z. Li, M.-S. Wong, J. Xin, W.-M. Hao, F. Zhao, 2010, Aerosol Single Scattering Albedo Estimated across China from a Combination of Ground and Satellite Measurements, J. Geophys. Res. 112, D22S15, doi:10.1029/2007JD009077. [Zhanqing Li, United States of America]	Accepted. Cited, but year is incorrect, the study is from 2007.
7-1925	7	84	39	84	39	Add the following references [Zhanqing Li, United States of America]	Noted.
7-1926	7	84	39	84	39	Li, Z., and L. Kou, 1998, The direct radiative effect of smoke aerosols on atmospheric absorption of visible sunlight, Tellus Series B*, 50, 543-554. [Zhanqing Li, United States of America]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1927	7	84	39	84	39	Li, Z., et al., (2007), 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, United States of America]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1928	7	84	39	84	39	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, United States of America]	Accepted. Reference added.
7-1929	7	84	39	84	39	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. [Zhanqing Li, United States of America]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1930	7	84	39	84	39	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, United States of America]	Accepted. Reference added.
7-1931	7	87	7			Please add: Markowicz, K. M., Zielinski, T., Blindheim, S., Gausa, M., Jagodnicka, A.K., Kardas, A., Kumala, W., Malinowski, S.P., Petelski, T., Posyniak, M., Stacewicz, T., 2012. Study of vertical structure of aerosol optical properties with Sun photometers and ceilometer during MACRON Campaign in 2007. Acta Geophysica, 60 (5): 1308-1337. [Government of Poland]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1932	7	87	25			Please add: Mazzola, M., Stone, R.S., Herber, A., Tomasi, C., Lupi, A., Vitale, V., Lanconelli, C., Toledano, C., Cachorro, V.E., O'Neill, N., Shiobara, M., Aaltonen, V., Stebel, K., Zielinski, T., Petelski, T., Ortiz de Galisteo, J.P., Torres, B., Berjon, B., Goloub, P., Li, Z., Blarel, L., Abboudm, I., Cuevas, E., Stock, M., Schulz, K.-H., Virkkula, A., 2012. Evaluation of sun photometer capabilities for retrievals of aerosol optical depth at high latitudes: The POLAR-AOD intercomparison campaigns. Atmospheric Environment, 52: 4-17 Sp. Iss. SI JUN 2012. [Government of Poland]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1933	7	88	18			The authors should consider inserting the following reference before Mishchenko et al. 2007: Mishchenko, M.I., and Travis, L.D., 1997. Satellite retrieval of aerosol properties over the ocean using polarization as well as intensity of reflected sunlight. J. Geophys. Res. 102: 16989-17013. [Government of United States of America]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1934	7	88	20			The authors should consider inserting the following reference after line 20: Mishchenko, M.I., Geogdzhayev, I.V., Liu, L., Lacis, A.A., Cairns, B., and Travis, L.D., 2009. Toward unified satellite climatology of aerosol properties: What do fully compatible MODIS and MISR aerosol pixels tell us? J. Quant. Spectrosc. Radiat. Transfer 110: 402-408. [Government of United States of America]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.

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7-1935	7	88	21			The authors should consider inserting the following reference before line 21: +G1606 [Government of United States of America]	Rejected. Reference not provided.
7-1936	7	88	44	88	45	Correct the reference for editorial as "Moorthy K. K, Satheesh, S.K., Babu, S. S. and Dutt, C.B.S., 2008. Integrated Campaign for Aerosols, gases and Radiation Budget (ICARB): An Overview, Journal of Earth System Science, 117: 243-262. [K KRISHNA MOORTHY, INDIA]	Accepted. Editorial.
7-1937	7	88	46	88	46	Insert the reference "Moorthy, K. K., Sreekanth,V., Chaubey, J.P., Gogoi, M M., Babu, S. S, Sobhan KK, Bagare, S. P., BC. Bhatt, V K. Gaur, T. P. Prabhu, and N. S. Singh, 2011. Fine and ultrafine particles at a near–free tropospheric environment over the high-altitude station Hanle in the Trans-Himalaya: New particle formation and size distribution, J. Geophys. Res., 116: D20212, doi:10.1029/2011JD016343." [K KRISHNA MOORTHY, INDIA]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1938	7	90	12	90	12	Add the following reference [Zhanqing Li, United States of America]	Noted.
7-1939	7	90	12	90	12	Niu, F., and Z. Li, (2012), Systematic variations of cloud top temperature and precipitation rate with aerosols over the global tropics, Atmos. Chem. & Phys., doi:10.5194/acp-12-8491-2012. [Zhanqing Li, United States of America]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1940	7	93	1	93	3	The reference Querol et al., 2006 that appears here does not provide any information on the part of the text where it has been included (Fig 7.12 caption). In fact it has nothing to do with it.The right reference is probably: Querol X., Alastuey A., Viana M.M., Rodríguez S., Artíñano B., Salvador P.,, Santos S.G.D., Patier R.F.,, Ruiz C.R., Rosa J.D.L., Sánchez de la Campa A.., Menendez M., Gil J.I. 2004. Speciation and origin of PM10 and PM2.5 in Spain.Journal of Aerosol Science 35, 1151-1172 [BEGONA ARTINANO, SPAIN]	Taken into account. The correct 2006 reference is now cited.
7-1941	7	97	61			Please add: Smirnov, A., Holben, B., Slutsker, I., Giles, D., McClain, M., Eck, T., Sakerin, S., Macke, A., Croot, P., Zibordi, G., Quinn, P., Sciare, J., Kinne, S., Harvey, T., Smyth, T., Piketh, S., Zielinski, T., Proshutinsky, A., Goes, J., Nelson, N., Larouche, P., Radionov, V., Goloub, P., Krishna Moorthy, K., Matarrese, P., Robertson, E., Jourdin, F., 2009. Maritime Aerosol Network (MAN) as a component of AERONET. Journal of Geophysical Research-Atmospheres, Vol. 114, D06204, doi: 10.1029/2008JD011257. [Government of Poland]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1942	7	106	42			The authors should consider inserting the following reference before line 42: Zhao, T.X.-P., Heidinger A.K., and Knapp K.R., 2011. Long-term trends of zonally averaged aerosol optical thickness observed from operational satellite AVHRR instrument. Meteorol. Appl. 18: 440–445. [Government of United States of America]	Rejected. Thank you for the suggestion, but we can only cite a subset of available references.
7-1943	7	107	1	107	6	The Table is very confusing. Why are some sources (sea spray, PBAP) separated at the right side of the table? Why are Dimethylsulphide, Monoterpenes, Isoprene listed. Only a small fraction of them are expelled as droplets (aerosol, injuries), otherwise they are released as gases (which later transform into SOA). SOA has nothing to do in this Table. Make a clear distinction between primary sources and sources of gases for SOA. Why is given minimal space only to the major sources (mineral, sea-spray, PBAP) and much more space to the minor sources. I have discussed that Table with a colleague. He said (translated): "The Table seems to be a smart (very polite translation) hodgepodge of terms and numbers, which has been mentioned in connection to secondary aerosols". This Table in its confusion seems to be a mirror of the whole chapter. [Ruprecht Jaenicke, Germany]	Taken into account. Table 2 has been splitted in two tables. As only a small fraction of BVOC condense as SOA, it is important to have SOA as a separate entry. The caption has been modified to justify this.
7-1944	7	107	1			table 7.2: are all acronyms defined before this table is included? [Peter Irvine, Germany]	Accepted. All acronyms are now defined in the table captions.
7-1945	7	107	1			table 7.2: "including spores 28" is this intentional? [Peter Irvine, Germany]	Accepted. Typo has been corrected.
7-1946	7	107	3	107	5	Table 7.2. PBAP is not defined here. The global flux range is indeed very large (50-1000 Tg/yr). [Erik Swietlicki, Sweden]	Accepted. All acronyms are now defined in the table captions.
7-1947	7	107	4			Table 7.2. It is confusing that anthropogenic NMVOCs are given in Tgyr-1 and BVOCs are given in TgC yr-1.	Taken into account. Units has been homogeneised for

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						Also confusing that SO2 given in Tgyr-1 while DMS is given in TgS yr-1. Tis should be harmonized. In addition, the headings of the Natural global sources (last 3 columns) should start in the top line, otherwise the Tbale is very difficult to read. [Urs Baltensperger, Switzerland]	SO2 and DMS. The Table was broken down in two tables for clarity.
7-1948	7	107	11	107	14	Table 7.1. Here I miss the emissions of other BVOC (sesquiterpenes, alcohols. Aldehydes, ethers etc) as well as of the Intermediate VOCs some of them can produce aerosols (see discussion in Kanakidou et al, 2005; Hallguist et al, 2009 papers and references therein as welll as in the 'Jathar, S. H., Farina, S. C., Robinson, A. L., and Adams, P. J.: The influence of semi-volatile and reactive primary emissions on the abundance and properties of global organic aerosol, Atmos. Chem. Phys., 11, 7727-7746, doi:10.5194/acp-11-7727-2011, 2011'. Also I consider that the 50 Tg-C of SOA as low limit is high - even the paper in reference for that gives a 12 Tg-SOA/y as the lowest estimate. [MARIA KANAKIDOU, GREECE]	Taken into account. The lower bound for SOA production has been changed to 12 Tg SOA / yr. The caption now clarifies that there are other BVOCs not listed in the table.
7-1949	7	107				Table 7.2: In the present form, it is not clear that there are two tables, one on the right side, for anthropogenic emissions, and a second one on the left side for natural emissions, please split in two tables. [Andrew Ferrone, Germany]	Accepted. The table was broken down in two tables.
7-1950	7	107				This table appears to be two tables in one and this is quite confusing. The last 3 columns should be either integrated into the table or a new table should be added. [European Union]	Accepted. The table was broken down in two tables.
7-1951	7	107				The accuracy of some of the entries in the table (5 digits) is excessive. Given that these are estimates the corresponding accuracy should be used. [European Union]	Accepted. The estimates have been rounded.
7-1952	7	107				For the particulate emissions it should be clarified if this is PM10, total suspended particulates, or something else. This is very important for Dust, Sea Spray and PBAP. [European Union]	Accepted. The caption says that the emission rates are sensitive to the cut-off diameter. Unfortunately different models may have different cut off diameters.
7-1953	7	107				The use of only the Spracklen et al. (2011) estimate for the SOA source needs to be better justified or the range should be lowered start at 12 Tg yr-1 to include all the published estimates (Kanakidou et al., 2005). [European Union]	Accepted. We have decreased the lower bound of the SOA source.
7-1954	7	107				Table 7.2: The formatting of this table needs to be improved. The "All Europe" and "All N. America" labels and values should be better aligned. A vertical rule before the "Source" column would be helpful. [Larry Horowitz, United States of America]	Accepted. The table was broken down in two tables.
7-1955	7	107				Table 7.2: Add "(monoterpenes and isoprene)" after "except for BVOCs" in table caption. [Larry Horowitz, United States of America]	Accepted.
7-1956	7	107				Table 7.2: Clarify that SOA source provided is not an "emission" but a "production rate". Clarify what is meant by the "natural" source of SOA here (presumably meaning from BVOC). [Larry Horowitz, United States of America]	Accepted.
7-1957	7	107				Table 7.2: Define "BVOC", "PBAP". [Larry Horowitz, United States of America]	Accepted
7-1958	7	107				table 7.2: A little reformatting might do good: "All Europe"+ "All N America" seem misplaced ; Right part of the table should be better separated [Michael Schulz, Norway]	Accepted. The table was broken down in two tables.
7-1959	7	107				table 7.2: Where would I find anthropogenic biomass burning emissions? The BC emission range seem to me very low. [Michael Schulz, Norway]	Accepted. BB was added to the table. BC emission range broadened?
7-1960	7	107				Table 7.2 [Erik Swietlicki, Sweden]	Noted.
7-1961	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, United States of America]	Partly taken into account. Fig. 7.10 breaks feedbacks down by cloud altitude, by mechanism, and by SW/LW. We did not include a figure showing the effect of CO2 but have added discussion to the text on this.
7-1962	7	108	1	108	4	PBAP are also very sensitive to size cut-off [Ruprecht Jaenicke, Germany]	Accepted. This is now stated in the caption to Figure 7.2b.

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7-1963	7	108	1	108	4	I was missing references for the quoted values in Table 7.3. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Rejected. The values are approximate and need not references.
7-1964	7	108	1	108	4	The distinction into CCN active/inactive seems to stem from a static framework. Emission of CCN "inactive" BC can very well increase CCN by acting as seed for CCN formation. Aerosol microphysics is dynamics so I would reconsider this framework in Table 7.3. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted. More flexibility in terms has been allowed.
7-1965	7	108	1			table 7.3: dust row, last column: "absorption" should read "absorbing" [Peter Irvine, Germany]	Rejected. "absorption" is the climate property as per the column descriptor.
7-1966	7	108	4	108	4	Table 7.2 - PBAP listed at the end of the table is also organic aerosol - it should be together or if kept separately should follow the organic aerosol and clearly state it is part of it. [MARIA KANAKIDOU, GREECE]	Accepted. Terrestrial PBAP is made as a subset of organic aerosols.
7-1967	7	108	4	108	4	Table 7.2 - Brown carbon above organic aerosol is also part of organic aerosol -although there is no consecus on the exact chemicalidentity of brown carbon it has not been put in doupt its organic content. [MARIA KANAKIDOU, GREECE]	Accepted. Brown carbon is made as a subset of organic aerosols
7-1968	7	108		108		Table 7.3: Black carbon should not have mass size distribution starting from 0 nm. This gives the impression of infinitely small BC particles. If applicable, (and a proper reference for mass emissions of such can be found), perhaps <1 nm could be used. Most generally, it might be better to refer the emissions to happen in particle sizes <80 nm, without giving a lower bound. [Ari Asmi, Finland]	Accepted.
7-1969	7	108				Table 7.3: In the thriod colum, please put the primary category always in first place. [Andrew Ferrone, Germany]	Accepted.
7-1970	7	108				Black Carbon. The range for freshly emitted BC is too wide (there are no BC particles with sizes of a few nanometres as far as I know). It is better to just say Aitken mode instead of 0-80 nm. [European Union]	Accepted. Size range has been replaced to < 100 nm.
7-1971	7	108				Organic Aerosol. The SOA is not in the nuclei mode but mainly in the accumulation mode. It does exist too in the Aitken and nuclei mode but in much smaller mass concentrations. [European Union]	Taken into account. We indicate that SOA is mostly in the accumulation mode.
7-1972	7	108				Sulphate. There is primary anthropogenic sulphate in the end of the tailpipe or stuck produced by high temperature oxidation of the sulphur before it is emitted to the atmosphere. This is primary anthropogenic sulphate usually in the Aitken mode. There is some in the accumulation mode too. These should be included here. [European Union]	Accepted.
7-1973	7	108				Table 7.3: The authors should consider giving the full name of primary biological aerosol particles (PBAP) in the table text (last row) not only in the heading [Government of United States of America]	Rejected. Full name is provided in the figure caption.
7-1974	7	108				Table 7.3: Global burden values are missing from Table. [Larry Horowitz, United States of America]	Taken into account. The burden column has been removed.
7-1975	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, United States of America]	Rejected. The word "tropopause" has multiple definitions, the principal of which is the level at which significant overturning (convection) ceases. Thus by definition FAT is connected to the tropopause. The cold-point in the tropics is a somewhat different beast but not necessarily relevant.
7-1976	7	109	1	109	2	Under category AFari+aci in liquid stratiform clouds published since AR4, please include reference to Kirkevåg et al (2012) (see reference above), and to: 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. [Trond Iversen, United Kingdom of Great Britain & Northern Ireland]	Kirkevag et al. 2013 and Seland et al. 2008 are added
7-1977	7	109				table 7.4: in addition to ACCMIP and CMIP5 one should mention AeroCom. Myhre et al. 2012 reference should be included. But maybe I misunderstand the "placeholder" meaning… [Michael Schulz, Norway]	Rejected. Myhre et al (2012) is cited where appropriate. Table 7.4 is for studies pertaining to ERFari+aci.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7-1978	7	110				Table 7.5: It appears as though "MPI/MPI-M" is missing values [Government of United States of America]	Taken into account. This line has been removed as no CMIP5 estimate is available for the MPI model.
7-1979	7	110				Table 7.5: The entry for the GFDL-AM3 model is from ACCMIP (as indicated). The corresponding value from the CMIP5 fixed-SST experiments is -1.60 W/m2 (for all anthropogenic aerosols) and -1.62 W/m2 (for sulfate aerosol). There results are now available in the CMIP5 archive. In addition, the Levy et al. (submitted to JGR) paper cites an aerosol AF value of -1.8 W/m2 from a set of fixed-SST experiments using interannually-varying SSTs (the value is -1.78 W/m2 for the period 1996-2005 versus 1860 emissions). These widely-varying estimates for aerosol AF for a single model suggest that methodological issues across model studies may introduce considerable variations (and uncertainty) to estimates of this quantity. [Larry Horowitz, United States of America]	Taken into account. The reviewer is correct for the GFDL-AM3 and both estimates are now in the table. The issues associated with diagnosing ERF are briefly mentioned in Section 7.1.
7-1980	7	112	5	139	7	The first thing I do with a document like this is to look over the figures and read the captions. The general conclusions of the chapter must be evident from this perusal, as a large majority of readers will get no further. The current draft fails this test badly. The figures and captions must be reviewed carefully and both the drafted figures and their captions considered carefully, ideally by a single lead author who can judge the collective clarity of this "storyboard". [Neil Donahue, United States of America]	Taken into account. Figures and captions have been revised and checked for their internal consistency by a couple of LA.
7-1981	7	112				Figure 7.1: The meaning of the arrows on this figure are difficult to asses: Do the green arrows represent the aerosols feedback chain? In this case the green arrow between aersols and clouds should be reversed. Or do this arrow rpresent the feedback involving aersols? Please clarify this point in order to make the figure less confusing [Andrew Ferrone, Germany]	All arrows are now labelled. Caption has been modified.
7-1982	7	112				Figure 7.1: There is no physical reason to distinguish between surface temperature and other state variables of the atmosphere. Thus we suggest to put one box and just indicate a few examples, like temperature, wind, moisture etc. [Andrew Ferrone, Germany]	Rejected. We define feedbacks as going through a change in global-mean surface temperature (see text)
7-1983	7	112				Figure 1: Red arrows are evident, but not brown arrows. Please clarify. [Government of United States of America]	All arrows are now labelled.
7-1984	7	112				Figure 7.1: Illustration of Aerosol-Radiation Interactions appears to be not entirely correct because the effects of aerosols on radiation are only one-way and cannot be qualified as “interactions,” as shown in the figure. On Page 7-10, Lines 14-15, the effects of clouds on Earth’s radiation budget are referred to as “cloud radiative effect (CRE).” Does CRE differ from the conventional term, “cloud radiative forcing (CRF)?” If so, some explanation should be in order. [Kuo-Nan Liou, U.S.A.]	CRE is used instead of CRF, as indicated in the text and glossary.
7-1985	7	112				I have real trouble indentifying which are the boxes with shadows (forcings) and distinguishing brown arrows (adjustments) from other colors; is the tan for lapse rate feedback a brown or a different color; why a brown from gmst from the sfc to atmos state variables; if from gmst I would expect it to be a feedback. So in my view the figure needs work. maybe use dashed lines; some devise other than gray shadows. [Stephen E Schwartz, United States of America]	All arrows are now labelled.
7-1986	7	113	1	113	2	The figures for simi-direct effect and cloud albedo effect are not well depicted with very small cloud changes which are difficult to see for the former effect and no shrinking cloud droplets or increasing number of cloud droplets for the latter effect. [Teruyuki Nakajima, Japan]	Not clear whether this applies to this figure. This figure is about terminology not showing the effect.
7-1987	7	113	4			Figure 7.2. This Figure lacks sufficient information. Differences between blue and brown arrows not exlained, differences bewteen brown and black dots (aerosol particles) not explained [Urs Baltensperger, Switzerland]	Taken into account. Caption was modified.
7-1988	7	113				Figure 7.2: There is no explantion of the arrows. Please specify what they do represent. [Andrew Ferrone, Germany]	The arrow is to indicate the coupling between the surface and the cloud which results in some adjustments.
7-1989	7	113				Colors in the blue part of the figure are not very distinct, difficult to read [Government of Poland]	Taken into account, the text is now in bold font.
7-1990	7	113				Figure 2: I suggest moving the "AR4" and "AR5" labels to the left side of the figure. I did not notice them at first. [Government of United States of America]	Rejected, but "AR5" has been added to the figure caption to catch the attention of the reader.
7-1991	7	113				Figure 7.2: The authors should consider including an explanation of how "AF_aci" subsumes "AF_ari".	Taken into account. The caption has been simplified

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						[Government of United States of America]	and points to the text where these important concepts are discussed.
7-1992	7	114	4	114	15	People read left to right, yet the transects are described in the text from right to left (NE to SW). Why? [Neil Donahue, United States of America]	Caption clarified to indicate cloud types are explained right to left. This is logical for the trades, where right is upwind and left is downwind.
7-1993	7	114	9	114	9	I'm fairly sure "upwind" should be "downwind" [Neil Donahue, United States of America]	Caption clarified to indicate it (correctly) refers to upper-level outflow.
7-1994	7	114				Letters in the graph not very visible, difficult to read [Government of Poland]	Taken into account. All font size are now as per IPCC style guide.
7-1995	7	114				Figure 3: The authors should consider adding labels to Fig.7-3a to indicate the location of the extratropical cyclone, subtropical coastal stratocumulus, shallow cumulus, and ITCZ-to make it more clear for the reader. [Government of United States of America]	Rejected. The transect are already labelled.
7-1996	7	114				Figure 7.3: In the caption, note that terms like 'mixed-phase' are described in more detail in the chapter. [Government of United States of America]	Rejected. A lot of the items in this figure are discussed in more detail in the chapter. There is no reason to single out the term "mixed-phase".
7-1997	7	114				figure 7.4b: It looks like the ratio LWP and IWP is increasing close to the poles. Is that realistic? [Michael Schulz, Norway]	Taken into account. The measurements are uncertain towards the pole and have been cut in the revised figure to remove this issue.
7-1998	7	115	1	115	12	Concerning Figure 7.4 d it may be worth mentioning that, even in the area where precipitation is noted “mostly liquid” it may be initiated through the ice phase in many cases. [Ottmar Möhler, Germany]	Partly taken into account. Not mentioned in the caption but ice cloud microphysics is discussed in various places of sections 7.2 and 7.4.
7-1999	7	115				Figure 7.4: Pleas also indicate the -38°C line on the lower right plot. [Andrew Ferrone, Germany]	Done.
7-2000	7	115				Figs 7.4a, 7.5, 7.6: choice of map projection and continent placement makes it difficult to visualize Europe. [Jean-Christophe Golaz, United States of America]	Rejected. The choice of map projection is that of IPCC. The choice of continent placement was made to clearly show all three important oceanic basins and their cloud regimes.
7-2001	7	115				Figure 7.4: Parts (a), (c), and (d) contain no references to the origin of the work. The authors should consider having parts (c) and (d) reference Haynes et al. (2009) if they were created using CloudSat "2C-precip-column" attenuation-corrected reflectivities, or follow the methodology described therein: Haynes, J. M., T. S. L’Ecuyer, G. L. Stephens, S. D. Miller, C. Mitrescu, N. B. Wood, and S. Tanelli (2009), Rainfall retrieval over the ocean with spaceborne W-band radar, J. Geophys. Res., 114, D00A22, doi:10.1029/2008JD009973. [Government of United States of America]	Taken into account. The origin of the data is now provided in the figure captions.
7-2002	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 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, United States of America]	Taken into account. This comment presumably is in the wrong place. The limitations of using satellite observations to diagnose Rfaci and ERFaci are discussed in the text (section 7.5.2) and highlighted in the ES. The study by Penner et al and other relevant studies are cited.
7-2003	7	116	1			figure 7.5: could labels be added above the panels? [Peter Irvine, Germany]	Done. Labels were added above the plots.
7-2004	7	116		116		A little cnfusing here. Not sure whether the radar only or the radar+lidar are being used since "GEOPROF" is stated in the caption (radar only product), while it seems to be implied that both are used. [Brian Kahn, United	Accepted. The origin of the data is now provided in the figure captions.

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						States of America]	
7-2005	7	116				Figure 5: The clarity of this figure could be improved by labeling things better. "High", "Mid", and "Low" should be placed to the left of the appropriate maps. "DJF" and "JJA" need only appear once at the top of the respective columns. [Government of United States of America]	Done. Labels were added to the plots.
7-2006	7	116				Figure 7.5: Are there citations for the GEOPROF and GOCCP datasets? [Government of United States of America]	Taken into account. The origin of the data is now provided in the figure captions.
7-2007	7	117	1	117	7	The color-bars do not allow to distinguish small positive from negative values. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted. A blue-red color bar is now used.
7-2008	7	117	1			figure 7.6: could labels be added above the panels? [Peter Irvine, Germany]	Done. Labels were added to the plots.
7-2009	7	117				Fig 7.6a,b,c: poor choice of color. Scales differ widely between panels. Difficult to differentiate between positive and negative values. [Jean-Christophe Golaz, United States of America]	Accepted. A blue-red color bar is now used.
7-2010	7	117				Figure 6: The clarity of this figure could be improved by labeling things better.. "SW", "LW", "Net", and "Precipitation" should be placed near the appropriate maps. Giving all cloud radiative effect maps the same color scale range, and perhaps making values near zero white rather than green would make things consistent and clear. If not, at least the zero contour should be displayed clearly. Also, the authors should consider showing the global mean values with each figure. [Government of United States of America]	Accepted. A blue-red color bar is now used. Labels have been added to the plot.
7-2011	7	117				Figure 7.6c: It is confusing that both yellow and orange colors actually show negative values in the figure. I would suggest changing the color scheme so that it is clear to the readers that the net CRE is negative over most of the globe. [Kuo-Nan Liou, U.S.A.]	Accepted. A blue-red color bar is now used.
7-2012	7	118	10			Figure 7.7: Conventional GCMs are also used as NWP models on scales down to days? Same with global cloud resolving models. The vertical extent of the brackets might want to be larger. [Andrew Gettelman, United States of America]	Taken into account. The figure has been greatly simplified and the caption was amended.
7-2013	7	118				Figure 7.7: Please define MMF and add a range to represent regional climate models in this graph, as they have been shown to be able to bridge the gap between fine-scale models used for idelatised cases and the large scale models that represent a realistic meteorology in section 7.4.1.3. [Andrew Ferrone, Germany]	Taken into account. The figure has been greatly simplified and the caption was amended. We do not discuss regional climate models, except for those models with high resolution, so there is little point in including these in this schematic.
7-2014	7	118				I found the cloud background in Figure 7.7 rather confusing. It does not appear to add anything to the figure so it should probably be deleted. [European Union]	Taken into account. The figure has been greatly simplified and the background was removed..
7-2015	7	118				Figure 7: although you mention 'multi-scale modelling framework' in the figure caption, you should also add '(MMF)' in the caption since that acronym appears in the figure itself and has not been defined in the text. [Government of United States of America]	Taken into account. The figure has been greatly simplified and the caption was amended.
7-2016	7	119	1			Figure 7.8. The feedback parameters on the X-axis should be clearly defined. The different parameters are listed in the caption, but it isn't clear which symbol is which parameter in the figure. Based on the values shown I'm assuming that λQ, λL, and λT are the water vapor, lapse rate, and Planck response feedbacks, respectively, but this should be labeled. [Hugh Morrison, United States]	This figure has now been redone properly
7-2017	7	119	2	119	11	Symbols on horizontal axis should be defined. [Leo Donner, United States of America]	This figure has now been redone properly
7-2018	7	119				Figure 7.8: The authors should define the symbols on the x-axis. [Government of United States of America]	This figure has now been redone properly
7-2019	7	119				figure 7.8: needs better caption or legend on symbols used [Michael Schulz, Norway]	This figure has now been redone properly
7-2020	7	119				I have a version of this figure with the cloud and albedo feedbacks on it as well (http://people.oregonstate.edu/~shellk/RH_feed_comp_all.jpg; I can provide a better copy). I'm still lacking much of the CMIP5 data locally, but I'm happy to share my scripts for calculating these feedbacks and/or run them if someone has all the data readily available. [Karen Shell, United States of America]	This figure has now been redone properly

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7-2021	7	120				Figure 7.9: It says 'see text', but it does not state specifically where. Please clarify. [Government of United States of America]	Corrected.
7-2022	7	120				Figure 9: Is there a need to notify the reader that the values from Zelinka et al. (2012) are from slab-ocean models simulating an abrupt doubling of CO2 whereas those from Soden and Vecchi (2011) are from fully-coupled models simulating a 1% per year forcing scenario? Is there / should there be a discussion in this chapter or in any other chapter concerning whether feedbacks computed in slab ocean models are closely related to feedbacks in more realistic model set-ups? [Government of United States of America]	Taken into account. The literature does not suggest that this makes a significant difference; this is now noted briefly in the chapter text.
7-2023	7	121	12			Figure 7.10: Might add rising tropopause altitude in brown. [Andrew Gettelman, United States of America]	Rejected. The figure has been simplified to make its points clearer. The tropopause is no longer labelled as such. The cloud feedback is because of the vertical shift in clouds rather than due to the tropopause rising although the two are obviously related.
7-2024	7	121				Letters in the graph not very visible, difficult to read [Government of Poland]	Accepted. All figures now use font size specified in the IPCC style guide.
7-2025	7	122	1	122	7	The confusion between particle number and particle mass is especially problematic in this figure. The boxes include "secondary particles", "primary particles" and "processed aerosols". Secondary particles are particles that nucleat in the atmosphere, but SOA, which is listed as a subset of secondary particles, is any organic aerosol mass that condenses onto existing particles. While there may well be some role for organics in the nucleation of new particles, this remains an active and uncertain topic -- it is more accepted that organic condensation plays an important role in the growth of new particles from 1-2 nm up to CCN sizes. That,however, would be part of "processed aerosols". At a minimum, replace "processed aerosols" with "processed particles". [Neil Donahue, United States of America]	Taken into account. "processed particles" has been changed to "aged particles. Otherwise this diagram is not specific in terms of number vs mass concentrations.
7-2026	7	122	2			Figure 7.11: In this figure, aci appears as AF, not RF. This seems rational to me. But RF(direct effect) of aerosols is AFari? [Andrew Gettelman, United States of America]	It is ERF in both cases, consistent with Fig 7.3.
7-2027	7	122				Figure 7.11: Primary Biological Aerosol Particles are missing in this graph. [Andrew Ferrone, Germany]	Taken into account. PBAP is a special case of POA, and need not to be added as per Table 2.
7-2028	7	122				Figure 7.11: Another important process is sedmination, which should also be indicated by an arrow towards the ground. [Andrew Ferrone, Germany]	Rejected. Sedimentation comes under deposition. There are many other processes not on this diagram.
7-2029	7	122				Letters in the graph not very visible, difficult to read [Government of Poland]	Taken into account. All font size are now as per IPCC style guide.
7-2030	7	122				Figure 7.11: Neither NH3 or HNO3 are low volatility gases. These gases partition to the aerosol phase because they are soluble and undergo condensed phase reactions forming NH4NO3 and partially or totally neutralizing sulfuric acid. The text should be revised to reflect this. [Government of United States of America]	Rejected. This level of detail too complicated for this diagram.
7-2031	7	122				Figure 7.11 General conversions for flowcharts need to be employed. I had trouble in understanding this figure. What are the differences between rectangular boxes and rounded boxes and “document boxes” in the figure? Normally “processes“ are expressed as rectangular boxes, rather than circles or ellipses. [Myeong-Jae Jeong, Republic of Korea]	Taken into account. Boxes represent variables rather than processes. All boxes are now rounded.
7-2032	7	122				Figure 7.11 I think the link shows “the interactions between clouds and aerosols“ should be explicitly included in the figure. [Myeong-Jae Jeong, Republic of Korea]	Rejected. This figure cannot show all existing processes. See Figure 7.16 for more details.
7-2033	7	123	1	123	1	In the Figure under Asia there are three plots, no comments in the text that explains what "High" means [Elisabetta Vignati, Italy]	Rejected "high" refers to regions with altitude higher than 1700 meters as indicated in the caption.
7-2034	7	123	1	123	1	I find the plotting number in the X axis confusing; do they mean the same compounds in all figures? [Elisabetta Vignati, Italy]	Taken into account. The numbers mean the same compounds, except for number 6, which can be either mineral or sea-salt, according to the colour code, which has been modified to make this point clearer.

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7-2035	7	123	1	123	1	No consistency between EC measurements here and BC in the text [Elisabetta Vignati, Italy]	Noted but EC and BC have been defined in section 7.3.1
7-2036	7	123	1			figure 7.12: list of references is too cumbersome [Peter Irvine, Germany]	Rejected. Need using the data from these references to support the figure
7-2037	7	123	4	123	4	What does it mean "annual seasonal or monthly mean mass surface concentration"? If there is no way to appreciate whn is annual or seasonal or monthly mean this may be left out [Elisabetta Vignati, Italy]	Raw data used here some are monthly, and some are seasonal mean. These are what we can get although it is not perfect. The "annual" has been removed.
7-2038	7	123	4			Figure 7.12. Colors are difficult to discriminate. Suggets to use the AMS colors (sulfate red, nitrate blue, ammonia hellow, organics green, etc.) [Urs Baltensperger, Switzerland]	Accepted. The diagram revised.
7-2039	7	123	6			figure 7.12, line 6: "for each location, the panels…" it is not clear here what "the panels" are. [Peter Irvine, Germany]	The diagram has been changed to have new global map associted showing what "the apnels" are.
7-2040	7	123	8	123	9	Following reference provides latest information about urban BC and inorganic aersols over large part of California be included: Sahu, L. K., et al. (2012), Emission characteristics of black carbon in anthropogenic and biomass burning plumes over California during ARCTAS-CARB 2008, J. Geophys. Res., 117, D16302, doi:10.1029/2011JD017401. [Lokesh Kumar Sahu, India]	Rejected. All the data we include here are ground-based long-term measurements.
7-2041	7	123	23	123	25	duplicate of lines 20-23 [Elisabetta Vignati, Italy]	Modified.
7-2042	7	123	25	123	26	Following latest study providing continuous hourly BC and OC measurements for two years at Bangkok, which is unprecedented for any urban site in SE Asia be included. Sahu, L. K., Y. Kondo, Y. Miyazaki, P. Pongkiatkul, and N. T. Kim Oanh (2011), Seasonal and diurnal variations of black carbon and organic carbon aerosols in Bangkok, J. Geophys. Res., 116, D15302, doi:10.1029/2010JD015563. [Lokesh Kumar Sahu, India]	Accepted. The data has been included in our new diagram.
7-2043	7	123				Figure 7.12: The numner on the x graph are confusing, please take them away, the legend on the bottom right is sufficent to understand the colors. [Andrew Ferrone, Germany]	The diagram has been modified
7-2044	7	123				Figure 7.12: A total for each region would help to put these numer in context and assess their relative contribution. [Andrew Ferrone, Germany]	The diagram has been modified to have new global map associted.
7-2045	7	123				I am assuming that what is shown in these figures is organic aerosol (OA) and not organic carbon (OC). This should be corrected in the caption if appropriate. If it is OC the units should be ugC m-3. [European Union]	It is OC, not OA. The caption has been clarified in that respect.
7-2046	7	123				Figure 7.12 The color scheme used in the figure needs an improvement. For instance, yellow can be used to represent dust and black or gray can be of use for EC. Also, three rows, instead of two, can be used to enhance the figure. [Myeong-Jae Jeong, Republic of Korea]	Accepted. The color code was changed
7-2047	7	124	1			figure 7.13: black on dark blue is not clear in lower panels [Peter Irvine, Germany]	Taken into account. The black dots have been removed.
7-2048	7	124	1			figure 7.13: could the regions shown in the lower panels be highlighted on the globe shown above? [Peter Irvine, Germany]	Accepted. The regions are now highlighted and labelled.
7-2049	7	124				Figure 7.13 caption: The wavelength for the AOD should be indicated. Is this is at 550 nm? Also, the wavelength for the Aerosol extinction should be indicated. Is this is 532 nm? [Government of United States of America]	Accepted. The wavelength is now indicated in the caption and in the case of the AOD on the plot itself.
7-2050	7	124				Figure 7.13 I have strong suspicion about the quality of the result shown in Figrue 7.13(a): AOD over Indo-Gangetic Plain and eastern China overwhelm that over the Saharan desert. Even the enhanced AOD over the central Africa due to biomass burning is higher than AOD around the Bodele depression region. I am not sure how much we can rely on the Figure 7.13 (a) and what kind of information can be delivered via this figure. [Myeong-Jae Jeong, Republic of Korea]	Noted but no action taken. The AOD map shows a multi-year average. AOD over the Sahara is sporadically large, but it always large in some Asian polluted regions. This said it is true that satellite measurements over the desert are uncertain, which limits our confidence in aerosol reanalysis over such

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							regions.
7-2051	7	125	1			figure 7.14: the text of the panels is disrupted or overshoots in some panels [Peter Irvine, Germany]	Accepted. The figure has been simplified and the text does not overshoot.
7-2052	7	125				Figure 7.14: The meaning of the full black lines is not given, nor the defintion of the error bars. [Andrew Ferrone, Germany]	Accepted. The caption has been modified to include this information.
7-2053	7	125				Figure 14: What is the point of Fig.7.14? The models seem to be all over the place compared with the observations. Further clarfication by the authors is warranted. [Government of United States of America]	Taken into account. The text has been modified to make the point of this figure.
7-2054	7	125				Figure 7.14 caption: The authors should indicate that the black solid line and horizontal thick black lines represent the airborne SP2 measurements and standard deviations. [Government of United States of America]	Accepted. The caption has been modified to include this information.
7-2055	7	125				figure 7.14: It might be useful to indicate that measurements and models are not from the same year. [Michael Schulz, Norway]	Accepted. The caption has been modified to include this information.
7-2056	7	126	1	126	1	the figure captions are not well readable [Elisabetta Vignati, Italy]	Taken into account. This figure is now published and has been removed from this chapter.
7-2057	7	126				Figure 7.15: the symbols and legend are too small. What do the colored regions correpond to? Points seem to fall all over the place. Can it be simplified to make the point? [Government of United States of America]	Taken into account. This figure is now published and has been removed from this chapter.
7-2058	7	126				Figure 7.15, caption: The caption indicates temperatures and relative humidities are plotted. However, the y axis lists S values between 1.0 and 1.8. This canot be relative humidity. What is plotted on the y axis? [Government of United States of America]	Taken into account. This figure is now published and has been removed from this chapter.
7-2059	7	127		127		Figure 7.16 : Is there any special meaning in the color of text ? [Sandrine BONY, France]	Taken into account. This figure has been simplified. The color code is now explained in the caption.
7-2060	7	127				Figure 7.16 lacks information on the significance of red and blue colours [Ari Asmi, Finland]	Taken into account. This figure has been simplified. The color code is now explained in the caption.
7-2061	7	127				Figure 7.16 I think “Cloud-processed Particle Production” need to be included in this figure, which can be discriminated from “New Particle Production” near clouds. I believe significant number of aerosol population are processed by clouds (e.g., Jeong and Li, 2010). There are number of studies (Hoppel et al., 1994; Krämer et al., 2000; Jeong and Li, 2010) discussed the importance of cloud-processed particles, which normally have larger sizes than newly formed particles. [References: a) Hoppel, W. A., G. M. Frick, J. W. Fitzgerald, and R. E. Larson (1994), Marine boundary layer measurements of new particle formation and the effects nonprecipitating clouds have on aerosol size distribution, J. Geophys. Res., 99(D7), 14,443–14,459, doi:10.1029/94JD00797. b) Krämer, M., N. Beltz, D. Schell, L. Schütz, C. Sprengard-Eichel, and S. Wurzler (2000), Cloud processing of continental aerosol particles: Experimental investigations for different drop sizes, J. Geophys. Res., 105(D9), 11,739–11,752, doi:10.1029/1999JD901061. c) 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]	Taken into account. Cloud processing is implied by droplet coalescence and scavenging and mixing processes, as now indicated in the caption.
7-2062	7	128	1	128	8	The model names in Fig. 7.17 are not consistent with the corresponding AeroCom phase II papers. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Accepted. This figure is now in the supplementary material. Model names have been updated.
7-2063	7	128	1	128	8	It would be nice to show a lat lon plot with the corresponding zonal / meridional means by model on the sides. [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Rejected. Lat-lon plot of RF is shown in Chapter 8.
7-2064	7	128				Figure 7.17 Regarding agreement between models. I can’t help but notice that compared to Fig. 7.17, model predictions were more tightly grouped when predicting total AOD (Fig 9.29), which is potentially a harder problem. Exclduing three outliers the range was 0.09 – 0.16. It helps to have a global constraint. The authors might consider reflecting this in the text.	Taken into account. We now explicitly discuss the fraction of AOD that is anthropogenic in AEROCOM models, and refer to Fig. 9.29. Note that the CMIP5 models are usually less sophisticated in terms of their

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						[Government of United States of America]	aerosol representation than AEROCOM models.
7-2065	7	128				Valuable figure; I am pleased to see the abscissa being sine of latitude; allows eyeball integration. [Stephen E Schwartz, United States of America]	Noted. Thanks.
7-2066	7	128				Forcing needs to be defined: TOA? surface? [Stephen E Schwartz, United States of America]	Taken into account. TOA as throughout the chapter, added to the caption.
7-2067	7	129	1	129	8	It is not clear from the caption of 7.18 if the models are the same as in Fig. 7.17 [Philip Stier, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Caption has been updated to mention AeroCom phase II models. The figure has been modified as well.
7-2068	7	129	1			figure 7.18: how is modified total modified? [Peter Irvine, Germany]	Taken into account. Caption and text has been modified to clarify this.
7-2069	7	129	4	129	4	spell out abbreviations in legend [European Union]	Accepted.
7-2070	7	129				Figure 7.18: The intrepretation of standard devtion is not relevant in a context of non-gaussian distributions. Replace by percentile ranges as is usually done in box-whysker plots. [Andrew Ferrone, Germany]	Accepted.
7-2071	7	129				The fossil fuel combustion forcing shown corresponds to OA and not to OC. [European Union]	Accepted. Label has been changed.
7-2072	7	129				Figure 7.18: The authors should consider describing what is meant by “modified total” in figure caption; report mean, median, range values from figure in separate table, not only in the text. [Government of United States of America]	Taken into account. Caption and text has been modified to clarify this.
7-2073	7	129				TOA? surface? [Stephen E Schwartz, United States of America]	Taken into account. Caption has been modified to clarify this.
7-2074	7	129				I am surprised at the low forcing by SOA. I went to Myhre et al 12; the atmos burden of OA in that paper is 23% that of sulfate. Compare pie charts of Zhang GRL 07 or Jimenez Science 09 comparable to or exceeding sulfate. So no wonder such a low forcing for OA. This will skew estimates of direct aerosol forcing low, by perhaps 50%. Need to discuss why relied on a paper with such a low value for OA. [Stephen E Schwartz, United States of America]	Taken into account. Models do account for the absoprtion of solar radiation by POA and SOA, which explains why OA contributes less to the total RF than sulphate. Absorption by OA is discussed in Section 7.3. Estimates are corrected for the possible underestimation of OA in models.
7-2075	7	130	1			figure 7.19: Overall the upper panel of this figure is confusing, I'd suggest revising. There are three/four information bearing approaches (colour, vertical dividing lines, shape of point, and location on x axis) that all carry the same message, i.e. we have different groups of estimates. I'd reduce the complexity of this upper panel. [Peter Irvine, Germany]	colors are now explained and the figure has been simplified.
7-2076	7	130	1			figure 7.19: the description of the lower panel is missing. [Peter Irvine, Germany]	corrected
7-2077	7	130	4			Figure 7.19: the distinction as I understand it does not seem to make sense. [Andrew Gettelman, United States of America]	Taken into account. Colors are now explained and the figure has been simplified.
7-2078	7	130				Figure 7.19 The blue and orange bands in the top figure are not identified. [Government of United States of America]	Taken into account. Colors are now explained and the figure has been simplified.
7-2079	7	130				Figure 7.19: Is the "+CNV" designation intended to refer only to deep convective (ice) clouds, or also to shallow cumulus? Some models (e.g., GFDL-CM3) include aci in shallow convective (liquid) clouds, but not in deep convection. [Larry Horowitz, United States of America]	can be both. The figure has been clarified.
7-2080	7	130				Figure 7.19: Caption for lower panel is missing. [Larry Horowitz, United States of America]	corrected
7-2081	7	130				FIG 7.19: On P45, L22, the Rfari is given as 0.4+/-0.3 W/m2. Why in Fig. 7.19 does the error bar appear much smaller? [Robert Wood, United States of America]	Partly taken into account. Rfari on top panel of Fig 7.19 is from models whereas in section 7.5 it comes from multiple lines of evidence and expert judgement, so there is no inconsistency. Text has been clarified in that respect.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7-2082	7	130				FIG 7.19: Inverse estimate. How can the error bar on this be so small? Is the error bar consistent with the feedback uncertainty across models? [Robert Wood, United States of America]	the inverse estimates are deleted from this figure
7-2083	7	130				Why is there a drastic reduction in the spread of estimates of Afaci+ari compared with Afaci in the AR5 models? This would imply that models with a strong aci have a weak ari. This seems like a fascinating result and surely worth a mention. What is the physics behind this? [Robert Wood, United States of America]	Taken into account. we revised our strategy of estimating ERFari+aci so that now the spread is much larger
7-2084	7	130				Fig 7.19: What do error bars on lower panel represent? For example, there appear to be 3 models (8% of the total) with estimates smaller (in magnitude) than the extent of the error bar on Afaci in the lower panel. [Robert Wood, United States of America]	Accepted. Caption for lower panel got accidentically lost by the TSU.
7-2085	7	131	1			Figure 7.20. This figure is rather confusing. Specifically, what does “warming” refer to in the figure? Is this surface warming? How is this differentiated from the effects of GHGs, or clouds and aerosols? GSGs, clouds, and aerosols are well-defined properties of the atmosphere, while warming is a possible effect of changes in these (or other) properties. [Hugh Morrison, United States]	Taken into account. Figure has been deleted as its point was not clear.
7-2086	7	131	10	131	10	"increase, respectively decrease, precipitation should be "increase or decrease precipitation, respectively" [Neil Donahue, United States of America]	Taken into account. Figure has been deleted as its point was not clear.
7-2087	7	131		131		Figure 7.20 : Nice figure ! One comment about the caption : « ..blue or red if their change over the 20th century is thought to have changed a precipitation driver.. » : Why over the 20th century ? The figure explains equally well what will happen over the 21st century...except that the importance of aerosol effects will be reduced relatively to that of GHG effects. The meaning of the brown color should be given in the caption as well. [Sandrine BONY, France]	Taken into account. Figure has been deleted as its point was not clear.
7-2088	7	131				Figure 20. The figure caption explains red, green, and gray coloring, but “warming” and “GHG” also are rendered in brown. Either the figure needs to be changed or the figure caption should include the meaning of the brown coloring. [James Coakley, United States of America]	Taken into account. Figure has been deleted as its point was not clear.
7-2089	7	131				oops, I goofed. Try again. Figure 20. The figure caption explains red, green, and gray coloring, but “warming” and “GHG” also are rendered in brown. Either the figure needs to be changed or the figure caption should include the meaning of the brown coloring. [James Coakley, United States of America]	see above.
7-2090	7	131				Figure 7.20: The meaning of the yellow (or orange) text color is not given. [Andrew Ferrone, Germany]	Taken into account. Figure has been deleted as its point was not clear.
7-2091	7	131				Letters in the graph not very visible, difficult to read [Government of Poland]	Taken into account. Figure has been deleted as its point was not clear.
7-2092	7	131				Figure 20: What does the brown color mean? [Government of United States of America]	Taken into account. Figure has been deleted as its point was not clear.
7-2093	7	131				Figure 7.20 First line of caption: Personally I would put this differently, saying something like 'radiative drivers cool and destabilise the atmosphere, resulting in increased latent heating, precipitation and surface evaporation.' [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Figure has been deleted as its point was not clear.
7-2094	7	131				Figure 7.20 It would be helpful if the sources from which this figure was compiled were made clear. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Figure has been deleted as its point was not clear.
7-2095	7	131				Figure 7.20 It is nice to see that this figure acknowledges the relevance of top-of-atmosphere radiative fluxes as well as surface fluxes to changes in global precipitation. However, from the perspective of the atmospheric energy budget ultimately driving changes in global precipitation, it would be helpful if this figure could be modified to also show the impact of the various drivers on the atmospheric energy budget. This could perhaps be done by adding something like 'atmospheric radiative cooling' on the left hand side between the surface and top-of-atmosphere radiation drivers. This would be blue for warming and red for water vapour (Prevedi 2010), red for CO2, black for scattering aerosol, and red for absorbing aerosol. It would also nice to depict in some way the balance between radiative cooling, sensible heating and net latent heat heat release in the atmosphere. This could perhaps be done by reorganising the diagram to place something representing the conservation of the tropospheric energy budget and the radiation in the centre, and linking it to the deep	Taken into account. Figure has been deleted as its point was not clear.

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						convection via a latent heat release arrow and the surface sensible heat flux via another arrow. [Mark Webb, United Kingdom of Great Britain & Northern Ireland]	
7-2096	7	132	1			figure 7.21, what's the meaning of grey shading? The temperature decreasing region? [Junying Sun, China]	Taken into account. Caption modified.
7-2097	7	132				Figure 21: The authors might want to specify that half of the response *over ocean* is evident before any warming is felt. [Government of United States of America]	Rejected. It is not specific to the ocean.
7-2098	7	132				Figure 21: The authors should consider revising the caption text to better explain the figure. [Government of United States of America]	Taken into account.
7-2099	7	132				figure 7.21: What kind of overturning is meant here? Atmospheric? [Michael Schulz, Norway]	Accepted. "atmospheric overturning"
7-2100	7	133				figure 7.22: Some info from the caption could be copied into the figures for lisibility, eg 4xCO2, GEOMIP G1 [Michael Schulz, Norway]	Accepted.
7-2101	7	134	8			"... model panels." will be meaningless to readers. The phrase should be changed to clarify the meaning of the stippling in the figure. [James Coakley, United States of America]	Accepted. "in panels" has been deleted.
7-2102	7	134				fig 7.23: Add number of models used [Michael Schulz, Norway]	Accepted.
7-2103	7	135	4	135	7	About the expression: ""Figure 7.24: Timeseries of surface temperature (°C, left) and precipitation change (mm day–1, right) for GeoMIP experiment G2, relative to each model's 1 × CO2 reference simulation. Solid lines are simulations using SRM to balance a 1% yr–1 increase in CO2 concentration until year 50 after which SRM is stopped. Dashed lines are for 1% CO2 increase simulations with no SRM." The figure are included as top and botttom, not left and right. So, the text must be: "Timeseries of surface temperature (°C, top)" and "precipitation change (mm day–1 4 , bottom)". [Rubén D Piacentini, Argentina]	Taken into account. Panels have been labelled a) and b).
7-2104	7	137	1	137	2	The figure is not well depicted to explain the aerosol-cloud interactions. No change in precipitation?; what do black circles and white circles mean?; no change in cloud amount? [Teruyuki Nakajima, Japan]	Taken into account. There is only one type of aerosols. We have deliberately not indicated a change in precipitation to be consistent with the text. Communicated to TSU.
7-2105	7	137	1			FAQ 7.2, figure 2: the text below the right hand cloud includes a typo: delete "a" between "in" and "larger" on first line. [Peter Irvine, Germany]	Accepted.
7-2106	7	137				The artwork for this figure needs to be improved. The aerosols in this figure are so large that when you go from the clean to the polluted cloud, the aerosols become as large as the cloud droplets. [Anthony Del Genio, United States of America]	Taken into account. Communicated to IPCC TSU.
7-2107	7	138	4	138	4	Insert "proposed" after "some" (we don't know if they all work, so they shouldn't just be characterised as CRD methods, because that does imply that they all work. [Francis Zwiers, Canada]	Accepted.
7-2108	7	138	4			In the figure for the alternative g, please, make a box describing a carbon storage in soil under the trees. [Ilkka Savolainen, Finland]	Rejected. Figure has been modified, which makes this comment less relevant.
7-2109	7	138				Figure caption description does not match colours in figure. What do the colours mean - and are they intended to match confidence levels? [ben booth, United Kingdom]	Rejected. It is unclear which figure this comment refers to.
7-2110	7	139	6	139	6	Without changing the diagram, it seems to me that the possibility of altering the tropospheric aerosol loading should also be indicated, esepcially given that human-generated aerosols are already creating a counter-balancing effect. While this might not be the best option for a number of reasons, it is also a viable option and has received some consideration in suggestions to have power plants put out more SO2 (in my view, not the optimal approach were it to be done, but a possibility). Including it actually helps indicate why going to stratospheric aerosols is preferred for a number of reasons, but that is no way not to include tropospheric aerosols as a possibility. [Michael MacCracken, United States of America]	Rejected. This figure is not meant to be exhaustive and this possibility is not discussed in the FAQ.