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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Online coupled meteorology and chemistry models: history, current status, and outlook" *by* Y. Zhang

**Y. Zhang**

Received and published: 31 March 2008

Anonymous Referee #1 Received and published: 20 March 2008

Comments: The manuscript by Zhang is intended to give an overview of the history and the current developement status of models taking the feedbacks between climate, chemistry, aerosol, clouds and radiation into account. After a general overview the author restricts himself the five models developed in the US. The contents and capabilities of these five models are discussed in broad detail. Afterewards shortly some case studies are shown and a summary of what is needed as future developments is given in the end.

Although the detailed description of the five models is interesting to read (even as it is a little bit too lengthy sometimes), it does not include any news but just rounds up



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information that can be optained from individual publications. As it only contains five models restricted to US developments the article contains to few models to be called a review article of the current status of online-coupled models. The weak point of this article is the case study section. First of all, for the Caltech unified GCM which is discussed in Sect. 3 no case study at all is shown. For the other models independent case studies are discussed. This does not give any insides answering the question how the models perform in comparison to each other. After reading the very lengthy first 3 sections of the article I expected such a comparison, otherwise I see no reason for putting together a description of this five partly dissimilar models. For the manuscript to be published in ACP it is indispensible to rewrite the case study section completely and give results for all five models for the same case or even better for two or three cases illustrating the performances of the different models in different situations.

Reply: This is a review paper on published results, it is beyond the scope of work to perform any new simulations to intercompare the results from the five models with in-depth reviews. In addition, given numerous differences in model treatments and different spatial scales of these models, comparative simulations with the five models for the same case are not possible. The purpose of the case study section is to illustrate some of the feedbacks discussed previously using a few examples, the author believe that it is not necessary to include case studies from all models discussed. Among the five models reviewed, Caltech unified GCM is the one that accounts for the least feedbacks (e.g., it does not account for aerosol-cloud interactions). The case studies using several other models, on the other hand, provide more representative examples of feedbacks than the Caltech unified GCM. These include, for example, the feedbacks of aerosols to PBL meteorology by WRF/Chem, the feedbacks of aerosols to wind fields and precipitation by GATOR/GCMOM, and the feedbacks of aerosol/cloud to indirect aerosol radiative forcing by MIRAGE/CAM3. These case studies represent the current status of model capability in simulating such feedbacks with the state-of-the sciences treatments, therefore, are useful part of this review paper.



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Comments: In the following major points that should be all addressed within the revisions are discussed:

Major comments: • The distinction between the introduction and the section 2.1 is not quite clear. To avoid recurrences these two sections should be combined. This would also help to shorten the mauscript. Especially the introduction is too much focused on the work done in the US, whereas an introduction is expected to show an overview of all available scientific developments.

Reply: Introduction gives some general background on the importance of various feedbacks in the atmosphere and current major model deficiencies in simulating such feedbacks. It also states the motivation and significance of this review. Section 2.1 already starts the review and provides a review on history of online coupled models, which should not be combined with introduction section. The revised version will make the above distinction more clear. Introduction section provides a brief overview for major scientific developments for aerosol feedbacks (both experimental and modeling studies), regardless of the origin of the work. While it uses some models from the US as an example, it is not restricted to US models. The reviewer’s comment “the introduction is too much focused on the work done in the US” is factually incorrect, as some non-US work were explicitly mentioned in this section, e.g., Feichter et al., 2003 (Germany), Johnson et al., 1999, 2001 ; Sanderson et al., 2006 (U.K.), Audiffren et al., 2004 (France), Giorgi et al., 1993 a,b; Giorgi and Shields, 1999 (Italy), Rosenfeld and Lensky, 1998; Rosenfeld and Woodley, 1999; Rosenfeld, 1999, 2000; Givati and Rosenfeld, 2004; 2005; (Israel), Jauregui and Romales, 1996 (Mexico) and Langner et al. (Sweden).

Comments: • Section 2.2 includes only the history of the five discussed models, thus the title of the section 2.2 should at least be rephrased from “History of representative online coupled modeles in the US” to “History of the represented online coupled US-models”. But as Section 2.2 is focused on these five models it could be combined with Section 3 as the history of the model is closely



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linked to the capability of the model in the current status.

Reply: Section 2.2 includes the five and all other representative online models in the U.S. (not restricted to the five models) and the review is an overview of the history of online models in the U.S., rather than that of the five models. My original title is appropriate. Section 2 reviews the history of online models worldwide including that of U.S., section 2.2 should remain in this section.

Comments: • In Section 3 all statements that refer to developments not yet ready to use should be omitted, as the discussion of futher models is skipped with the argument that these models were still under development. In order to illustrate the somewhat complex structure of WRF/Chem it would be helpful to have a graph showing which aerosol module does work with which gas phase chemistry scheme.

Reply: The statements on future model capabilities have been removed in the revised. As stated clearly in page 1847, lines 2-5, “RADM2 and RACM have been coupled with MADE/SORGAM and CBM-Z has been coupled with MOSAIC and MADRID; CB05 is being coupled with MOSAIC and MADRID”. There is no need to add a separate graph to describe the same thing in an already long review paper.

Comments: • As already discussed shortly in the introduction, Section 4 does not at all provide the information expected from a models comparison as given in this paper. For Caltech unified GCM no case study at all is shown in the paper. So please remove this model completely from the discussion or include it into the case study section. In regard of the realisation of the case studies, it does not help much to see only short sketches of simulations showing a different case for each model. The paper would provide really new information, if it would show the same case studies simulated with all five models (and diverse different model configurations). As all models have been developed for different purposes it would be quite illustrative to see that different models perform best in different situations. You could take the case studies shown here but simulate them with all five models and compare the results. In addition, the



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discussion of the results should be much more detailed showing links between the implemented processes and the results.

Reply: As stated previously, the purpose of the case study section is to illustrate some of the feedbacks discussed previously using a few examples, rather than doing an intercomparison of all five models via one or more case studies. For this purpose, it is not necessary to include case studies from all models discussed. The comparative inter-comparison of the five models is beyond the scope of work for a review paper. In addition, given the complexities and major differences in many aspects (e.g., scales, physics treatments, and configurations) among the five models, such an intercomparison is not feasible at present.

Comments: • Section 5 is out of context, maybe it could be combined with the Section(s) 1/2.1. Otherwise it would be optimal, if the ideas given in Sect. 5 could be based on findings in the case studies section. At the moment Sect. 5 is a list of future developments needed but without any explanations why they are needed. Give reasons what do we gain from each of the demanded developments? If you think it to trivial, delete this section.

Reply: Section 5 reviews the major challenges in online coupled modeling on all scales, it is highly relevant to this review. Such challenges are based on the review of the history and current status, and key outstanding issues for the online coupled model development and application that were presented in the previous sections. To address the reviewer’s comments, the reasons for future model developments will be provided in the revised version.

Minor comments: Comments: p. 1834 line 7-9 Give reason why you are concentrating on US models:

Reply: The five US models were selected in this review because they represent the state-of-the science online models worldwide. Online model development work in European and Asia are generally behind the U.S. The above reasons have been pointed



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out clearly in the revised version.

Comments: p. 1835 line 1-4 Different models give different results anyway. Cite only paper that show for the same model that accounting and not accounting certain effects does change the results considerably. Do all the models show the same impact when neglecting the same feedback.

Reply: It has been increasingly recognized that the fossil-fuel particulate black carbon (f.f. BC) is a strong absorber that will lead to a net warming by accounting for its direct and indirect effects (e.g., IPCC, 2001, 2007; Jacobson, 2002, Chung and Seinfeld, 2005). If its major processes are treated with a physically-based approach and all important feedbacks are accounted for correctly in different models, the simulated climate responses of f.f. BC should be the same, at least in the direction, if not the magnitude. Current studies show a BC forcing with a range of 0.4 to 1.2 W m-2 (V. Ramanathan1 and G. Carmichael, GRL, 2008). As explained in Ramanathan and G. Carmichael (2008), many GCM's obtain low forcing values for BC relative to some other papers - much of it is due to the missing treatment of mixing state. The contradictory results in f.f .BC forcing of Jacobson (2002) and Chung and Seinfeld (2005) with Penner et al. (2003) provide a good example of models with and without feedbacks and should be included in this review. Very few models show results that account or not account for certain feedbacks in the open literatures, two (Mickley et al., 1999) and Shindell et al/ (2001) were cited in p1838, lines 8-12.

Comments: p. 1835 line 7 rephrase “among multimedia”, e.g. “among different regimes”

Reply: The “among multimedia” has been changed to “among the earth systems”

Comments: p. 1835 line 16-17 “can nucleate many small cloud droplets”: rephrase, e.g. “leads to formation of many small cloud droplets”.



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Reply: The suggested change has been made.

Comments: p. 1836 line 2-7 Be more specific. Most of the differences you explain here very laboriously are covered by the differences between CTMs and GCMs.

Reply: Lines 2-7 and this paragraph intended to review the general major model deficiencies of current GCMs and CTMs (rather than deficiencies of specific models), they of course should cover the major differences between CTM and GCMs. The current statements are very specific to those differences, it is not clearly what additional specifics the reviewer would require.

Comments: p. 1836 line 8-22 I did not look in all the citations you are giving here. But I expect from an introduction to include the current status of science throughout the world and you are missing here major european and asian developments. Most of them you are citing in Sect. 2.1, but I think they have to be named and classified here.

Reply: The statements in lines 8-22 are applicable for all models in the world. A few representative models from Europe and Asia will be added in the revised version, however, since this is the introduction section, not all the models reviewed under section 2.1 are necessarily included here.

The author is aware of European and Asian work on online model development and application. Many of them have been cited in pages 1842, lines 18-22. For example, the COST action 728 (http://www.cost728.org) report and web site include many recent European online models. However, most of them did not become coupled until recently, very few are truly online-coupled, not even "significantly coupled", they are overall behind US models such as (e.g., GATOR-GCMOM, WRF/Chem, MIRAGE). While more European and Asian work can be certainly included in the revised paper, a review that focuses on the five US models represents the current status of online coupled model development in the world.

Comments: p. 1837 line 11 what do you mean by “outputs”?



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Reply: “Outputs” refer to simulation results from a mesoscale meteorological model.

Comments: p. 1838 line 23-29 what are you talking about here, simply the concept of CTMs or of the concept of the socalled “couplers” as OASIS etc. ?

Reply: Those are the two major classifications of coupling method between a meteorological model and an air quality/atmospheric chemistry model. The first type of coupling between a meteorological model and an air quality model via an interface is conceptually similar the OASIS coupler. But the second type of coupling in an integrated model system is different from the OASIS coupler.

Comments: p. 1839 line 8-14 If you want to make this classification you should also include a distinction between models with and without aerosols.

Reply: Since aerosols are part of the air quality models, the suggested classification does not justify to be separate from the two coupling methods. Whether the models treating aerosol or not and at which level of detailed are indeed given in Table 1 with specific examples of the models.

Comments: p. 1839 line 16 How will you measure how “realistic(ally)” a model simulates individual feedbacks ?

Reply: The reviewer may have misunderstood the meaning of “realistically simulated” here. It refers to whether the known feedbacks are included or not in the model, which can be judged by the description of such feedbacks in the model in the relevant publications. Insufficient evidence exist to judge whether the simulated feedbacks are realistic, given a lack of observed feedbacks that can be used for a rigorous model evaluation.

Comments: Table 1 You are here in a “non US” section, so you should include european and asian major developments. (E.g. Joeckel et al.(Atmos. Chem. Phys., 6, 5067-5104, 2006), Stier et al. (Atmos. Chem. Phys., 5, 1125- 1156, 2005),



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Lohmann et al. (Atmos. Chem. Phys., 7, 3425-3446, 2007), Teyssèdre et al. (Atmos. Chem. Phys., 7, 5815-5860, 2007), ..., and some of the ones mentioned below on page 1840 of your paper). Table 1 This is the only place where you correctly write “Schlesinger”. Correct the wrong spelling of “Schlesimger” in the rest of the paper and the references.

Reply: This table uses a number of examples to illustrate the treatments of online coupling of gas, aerosol, radiative, transport and meteorological processes, it did not intend to include all the models reviewed in this paper. It does include some European work (e.g., Pitari et al., 1995). More European and Asia work will be added in the revised version if all information needed in this table are available from publications of these models.

Comments: p. 1841 line 12-16 I do not think that paper published in the 70’s are really “current” status.

Reply: “current” has been deleted.

Comments: p. 1842 line 22 “most representative”: This statement is an inappropriate subjective rating.

Reply: This statement has been rephrased.

Comments: p. 1844 line 4-8 This is not a full sentence.

Reply: The lines 4-8 in p1844 are indeed a full sentence with subject, verb, and object.

Comments: p. 1844/1845 Mention again the WRF/Chem is a mesoscale model.

Reply: This has been done.

Comments: p. 1845 line 21-24 If you think this notable, how are the chemistry calculations done in the other models? Really on another grid?

Reply: This is indeed a notable feature of WRF/Chem. In other models, the transport



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of meteorological variables and chemical species is not always treated using the same physics parameterization, they often need interpolation in space and time. Horizontal grid interpolation is sometime needed, as some models (e.g., NMM-WRF is used to drive CMAQ) have very different horizontal grids.

Comments: p. 1846 line 15 Which “other trace gases”? Provide a list if they are not too many.

Reply: Other trace gases include nitrous oxide, methane, CFC-11, CFC-12, CFC-22, and CC14, which have been added.

Comments: p. 1847 top Provide a diagram illustrating the possible combinations of chemical solvers and aerosol models. The diagram could also be extended to show dependencies on other processes.

Reply: The text in p1847 has described clearly which gas-phase chemistry is coupled with which aerosol modules. I do not see a need to provide a separate diagram to show the coupling. Also note that such diagrams are not given for other models.

Comments: p. 1847 line 20 ff. You are only talking about US models, so please restrict your statement to the US: e.g. “... have been developed in the US...”)

Reply: “In the U.S.” has been added.

Comments: p. 1849 line 10 ff. You are desribing all other models very detailed, so plaese provide also more details for MIRAGE2.

Reply: More details have been added for MIRAGE 2 and main differences between MIRAGE 1 and 2 in the revised version.

Comments: p. 1849 line 16 Begin a new paragraph here, as the description of the models ends here and a concluding remark starts.

Reply: Suggested change has been made.



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Interactive Comment



Comments: p. 1850 line 2 rephrase “representative”

Reply: Those models are representative, I do not see a need for rephrasing.

Comments: p. 1853 line 17-24 I do not understand these sentences. Rephrase them.

Reply: These lines have been rephrased to allow an easy understanding. The definitions of externally-mixed, internally-mixed, and core treatments have been provided, which will also facilitate the understanding of these lines.

Comments: p. 1858 line 5-8 Please prove this statement by the correct citation.

Reply: The reference of Zaveri et al. (2005a) has been added for this statement.

Comments: p. 1859 line 2-5 If MADRID is currently incativated than do not talk at all about it. Your are not discussing whole model systems with the argument that they are still under development. So also do not talk about unfinished parts of the model you are discussing.

Reply: The statement on MADRID 2 has been removed.

Comments: p. 1860 line 4-5 This sentence seems not to be useful in this context.

Reply: This sentence has been removed.

Comments: p. 1860 line 7-8 rephrase. This is not the only sentence in Sect. 3, which would be much easier to read, if you would make some more words and elaborate a little bit more.

Reply: This sentence has been elaborated a bit more in the revised version.

Comments: p. 1862 line 27-28 This is not a full sentence.

Reply: The sentence is indeed a full sentence. It has been modified a bit to avoid confusion.

Comments: p. 1865 line 11 in layer 1 − $>$ at the surface



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Reply: “in layer 1” is indeed more accurate than “at the surface”, as the height of the layer 1 “15 m” is mentioned, and one cannot say “vertical intervals varying from 15 m at the surface to 600-680 m near/at the domain top”

Comments: p. 1865 line 11 give height of domain top

Reply: The height of the domain top is ˜16 km, which has been added in the revised version.

Technical corrections: Comments: p. 1837 line 10 replace the slash by ”or” p. 1843 line 12 a dash is missing between cloud and radiation

Reply: The suggested changes were made.

Comments: p. 1844 line 5-6 substitute the slashes by words/ write full sentences

Reply: The “slashes” have been replaced by “dashes”. “urban/regional” has been changed to “urban and regional”

Comments: p. 1847 line 3/line 12 Use unique spelling “CBM-Z” or “CBMZ”

Reply: “CBM-Z” is now used consistently in the text.

Comments: p. 1847 line 12 Did you introduce the abbreviation TUV ?

Reply: Yes, it was defined in p. 1846, line 6.

Comments: p. 1852 line 24 approach ‘− > approaches

Reply: The suggested change was made.

Comments: p. 1852 line 25 “a” sectional approach, delete “for typical applications”



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Interactive Comment



Reply: “a” was added before “sectional approach” . “for typical applications” should remain, other more or few size sections may use for other types of applications (e.g., few size sections are used for long term simulations).

Comments: p. 1853 line 12 delete “(i.e. well-mixed)” p. 1862 line 29 occur − $>$ occurs p. 1863 line 1 drop − $>$ drops

Reply: The suggested changes were made.

Comments: p. 1865 line 13-15 rewite as follows: “Clouds barely occur during this episode. Thus the cloud microphysical scheme is turned off which includes that aerosol-cloud interaction and aerosol indirect effects are not simulated.”

Reply: The sentence has been rewritten.

Comments: Figure 3 enlarge figure, it is not readable.

Reply: The figure has been enlarged.

Comments: p. 1866 line 2/line11/line 16 Qv or Qv ?

Reply: It should be Qv, this was a typo by the journal.

Comments: p. 1866 line 23 What is the use of the primes ?

Reply: “primes” should not be used, this was a typo by the journal.

Comments: everywhere be consequent, replace MIRAGE by MIRAGE2, CAM by CAM3

Reply: MIRAGE has been changed to MIRAGE 1 or 2, they are different versions of MIRAGE. CAM is a generic name, whereas CAM3 refers version 3 of CAM, they cannot be replaceable.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 1833, 2008.