Atmos. Chem. Phys. Discuss., 10, C15173–C15180, 2011
www.atmos-chem-phys-discuss.net/10/C15173/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "An investigation of methods for injecting emissions from boreal wildfires using WRF-Chem during ARCTAS" *by* W. R. Sessions et al.

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Received and published: 22 May 2011

Author Responses to Manuscript ACP-2010-715

Referee 1

Thank you for the kind words. Your review has led to many revisions that we believe have greatly improved the quality of the manuscript.

Comment 1) Your comments that explain our findings are very useful, and we have incorporated them into the revised discussions of Figs. 3 and 6. This section has been greatly expanded, with many additional references. You will find that we used your

exact wording in some cases. We thank you for providing these succinct sentences.

Comment 1b) Our original discussion of prep_chem_sources in Section 2.1 was incomplete. We have revised that paragraph to include all of the points that you raised and thereby make a clear distinction between prep_chem_sources and FLAMBE. We then remind the reader of these differences when Fig. 3 is discussed.

Comment 2) We agree that MODE is a valuable evaluation tool. However, there were so many clouds during our ARCTAS cases that its use was limited. We only want to use MODE when we are confident about its utility. We elaborate on this issue in the revision.

Comment 3) This study has really opened our eyes to the many factors that influence injection height. Besides different model resolutions, the type of PBL scheme that is employed also is a major factor. We have expanded this paragraph considerably based on your comments. Someone needs to explore all of the various influences in a systematic way so that users of models will know how to properly interpret their results.

Suggestion 1) The problem is not with MODE, but with the large areas of clouds in the study domain on four of the six study days. This cloud cover either prohibits AIRS retrievals or renders them highly suspect. As a result, MODE has too little "ground truth" to compare our modeled results with. We used MODE on all six days, but there were too few clear areas containing objects on four of the days for us to be confident in the results. Thus, we only show results for our two best days. We have added sentences that make this point clear and that describe some of the challenges of using MODE.

Suggestion 2) We agree that our discussion was too vague and have followed the reviewer's specific suggestion as described below.

Abstract line 20âĂŤWe believe you mean line 22. We stated this poorly, and it has been re-phrased.

Introduction 1âĂŤWe have removed the word “barometer”.

Introduction 18âĂŤWe have used your suggested wording.

Introduction 26554-7âĂŤThe wording has been changed as suggested.

Introduction 26554-26âĂŤThe wording has been changed and the reference added.

Methods 26558-22âĂŤMODIS’s higher spatial resolution has been added.

26561-4âĂŤThis error has been corrected.

26563-10âĂŤWe have added more information about the Freitas 1-D plume model. Further details would require the use of equations which are given in the various Freitas papers that are cited, but which we would like to avoid here.

26563-13 and 26564-20âĂŤWe have removed this type of phrasing throughout the text.

26564-5âĂŤThis sentence has been re-phrased to emphasize the important point.

26565-1âĂŤYes, that is what we mean. The wording has been changed to make this clear.

26565-6âĂŤYou are correct. We have changed the wording to make this clear. Your pedantry certainly is at an appropriate level. We must make this as clear as possible.

26566-9âĂŤWording changed as requested.

26569-9âĂŤWording changed as requested.

26573-10âĂŤIf the simulation were correct, CALIPSO would not have observed it. The satellite was in the wrong location. The sentence has been re-phrased to make this clear.

26575-13âĂŤOur choice of words was poor; it sounded like we were taking sides. This clause has been deleted.

26575-21âĂŤWhoops! Yes, we mean AIRS. This has been corrected.

Summary 26575-20 (actually line 23)âĂŤYou are right; we have changed our wording as requested.

26577-27âĂŤThe sentence has been removed.

Table 1âĂŤWe have added references for the settings.

Fig. 1c—The A and B labels are now mentioned in the text. They also are stated in the caption to the figure to make the geographical reference easier.

Fig. 4 and othersâĂŤWe certainly want the figures to be legible. We will make sure that the editors do that

Fig. 6a—The caption has been revised to describe these terms.

Referee 2

General Comment

The goal of our manuscript was not to introduce new procedures. Instead, the goal was to compare several widely used existing schemes and to determine, as best we can, which scheme or combination of schemes produces results which agree most closely with those from remotely sensed data (CALIPSO). We also wanted to take a case study approach to complement the literature examining longer periods. You have some great ideas about additional studies, but we believe that most will cause the manuscript to depart from its original goal. Your other suggestions were very helpful, and we have incorporate them into the revision

We believe that the manuscript will be useful to the chemical transport community because of the following factors.

1. We know of no paper that has compared results from FLAMBE with those of prep_chem_sources. Although prep_chem_sources is the officially supported scheme for WRF-Chem, our findings suggest that it is not the best choice because it contains too many simplifying assumptions. Chemical transport modelers should be aware of

this.

2. We agree that numerous papers have examined the Freitas et al. 1D plume model. And, we have added numerous references for papers that have done so in the revised manuscript. However, to our knowledge, no previous paper has compared results from the 1D approach with those of alternative, more simple schemes. The literature contains a host of these alternatives; we selected two of them for comparison. We do not believe any comparison of this type has been published.

3. The references that have been added to the revised manuscript deal with much longer periods of time and many more data points. That type of study is valuable. However, we believe that individual case studies such as ours also are valuable because they point out issues that can be lost when long periods of time are considered. The choice of an injection scheme is very important to the transport results, and we believe that readers should be aware that this choice will affect the results of individual cases as well as longer term studies. The meteorological literature is filled with both case studies such as ours and longer term, more statistical studies. These two approaches complement each other. We believe that the chemical transport literature also should contain both types of studies. The revision now clearly states that ours is a case study.

4. The revision contains a greatly expanded discussion of possible reasons why the current results differ from other published studies. We now stress the importance of model space/time resolution and the choice of the PBL scheme when comparing injection and resulting transport. Future research should examine these issues in much greater detail than we have. That will be a major undertaking and will comprise a paper in itself. At least we point out some of the factors that must be considered when such a study is performed.

5. The 1D scheme was developed for use in low resolution global chemical transport models, although it can be used successfully at higher resolution. It does not require

much additional computing power or computing time. The current 1D scheme certainly is not perfect, and improvements certainly can be made to it. But, we wonder why it or something similar has not been used more widely. Why make simplifying assumptions if they are not necessary for computational purposes? Perhaps the current manuscript will make readers think about this.

Suggestions

Turning off the radiative feedback can easily be done, but that is far outside the goal of the current manuscript. We are not attempting to verify the utility of on-line CTMs vs. off-line models, only the effects of different pre-processors and different methods of defining injection height. We believe that your idea, while very interesting, would be a study in itself.

Concerning the impact of different injections on the arctic budget of aerosols, we again believe this is a study in itself.

Extending the study period to the spring and summer of 2008 would transform the manuscript from a case study to something similar to previous long term studies. Ours was meant to be a case study.

We have informally compared, but not presented, results from different model resolutions. The results certainly differ. It is an article of faith (in meteorology at least) that higher resolution models will provide superior simulations. But again, much more than what we have done so far will be needed to produce convincing results. The revision states that such a comparison is sorely needed.

Specific Comments

Introduction about WRF-Chem and offline models

P26556âĂŤWe agree with your comment about improvements related to higher horizontal resolution (Mass paper) and have modified the text to mention the global scale. We also added your comment about numerical diffusion issues at 45 km resolution.

On the preprocessors

We have greatly expanded the description of the two preprocessors (FLAMBE and prep_chem_sources). We did not perform a sensitivity test on the use of fire radiative power. Once again, our goal was to investigate "off the shelf" procedures, not investigate new ones. Although a very useful study, it is outside our scope.

Injection in PBL or above

As suggested, we have added Fig. 6b that is based on GFS-derived PBL heights. We added the Brioude et al. (2009) material to the discussion of this new figure. In our case, results show that the GFS-derived PBL heights are even lower and contain less variability than those from WRF-Chem (Fig. 6a). Brioude et al. found somewhat different results, perhaps due to the different locations that are being compared. In any event, we agree that the way PBL height is calculated has a strong impact on the fraction of plumes that are injected above the PBL. We have done a major modification to this section that agrees with your assessment. Of course, the problem is that no one has evaluated all of the various PBL schemes to determine which is best, and under what circumstances.

Long range transport issues

We have added injection between 0 and 3 km or 0 and 5 km layers to those originally listed in the paragraph before Section 4. We also mention your idea of injection in a $\pm$ 1 km layer of the PBL. However, as noted earlier, we did not find the plume model to add significant run time to our simulations. Nonetheless, it is a good suggestion. We agree that a period with better CALIOPSO data (less cloud contamination) would be preferable. We examined every day during the Spring 2008 ARCTAS period in search of such a day. However, the period chosen was the best we could find. For various reasons we wanted to confine our study to the ARCTAS period. We have not stressed the utility of the CALIPSO data in the manuscript. However, David Winker continues to believe that CALIPSO should be able to detect plumes in the clear air regions of Figs.

14 and 16. Those are the only regions that we discuss. Once again, we want this to be a case study, and not a composite study of the entire spring and summer seasons of 2008.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 26551, 2010.