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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.

Anonymous Referee #2

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Review of "An investigation of methods for injecting emissions from boreal wildfires using WRF-Chem during ARCTAS" by Sessions et al.

General comment:

This manuscript investigates parameterizations of injection height of fire plumes during the summertime ARCTAS campaign. The authors used the WRF-Chem mesoscale model to run a 1D plume rise module that is implemented in WRF-Chem and simulate the downwind transport of the fire plumes. The authors compared 2 preprocessing algorithms (the one included in WRF-Chem and FLAMBE) that calculate differently the emission of chemical species or aerosols based on satellite data. They compared the

WRF-Chem results with injection heights retrieved from the satellite product MISR and found that FLAMBE was the best preprocessor between the two. In the second part of the paper, the authors compared 3 different way to inject fires vertically: the 1D plume rise module, a homogeneous injection in the PBL and a homogeneous injection between 3 and 5km above ground level. The authors used WRF-Chem to simulate long range transport over 10 days of Russian and Canadian wildfires using those parameterizations and compared the WRF-Chem output with satellite data. They conclude that the 1D plume rise module improves the model simulations.

This paper doesn't introduce any new concept, new model, new parameterization or any new science results. The 1D plume rise module has already been evaluated with other models. The only novelty that I see in the paper is evaluating the 1D plume rise module in WRF-Chem and to use 2 different preprocessing tools. The most interesting result, in my opinion, is that the injection above the PBL is larger than in previous studies. But this difference is not well investigated in the paper. Even though the paper concludes that the 1D plume rise module has the best results, the differences with the injection between 3-5km is so small (Figures 10 to 16) that I am wondering if using a 1D plume rise module is that important for long range transport of fires. As the authors said in the conclusion, "the current results are based on a small study period during the Arctic summer". In my opinion, evaluating a set of options with a model on a 2 week time period is not enough to have a science question that is suitable for ACP. This kind of manuscript is still useful though and I would recommend that the authors submit their paper to Geoscientific Model Development which is a more appropriate journal for this kind of analysis. A section with scientific results is definitively missing. If the authors don't include any interesting science question in a revised manuscript, I would have to reject the paper.

Suggestions: The paper focus on the transport of fires into the arctic. We have learnt from POLARCAT that wildfires have a strong impact on the aerosol budget in the arctic. We know that having an accurate transport of fire plumes is important in this region.

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The authors say in the introduction : "[WRF-Chem] incorporates radiative and chemical feedbacks into the atmospheric energy budget that an offline model cannot do". This is exactly what makes WRF-Chem so powerful. If you can show differences in transport into the arctic due to the radiative feedback from aerosols (there is probably an option that activates or not this feedback), your paper will be interesting. You should also compare the impact of the different injection height parameterizations on the arctic budget of aerosols and the average vertical distribution of aerosols in the arctic. To do that, you should extend the time period to spring and summer 2008 (or at least a season) instead of a 2 week time period. You would have plenty of CALIPSO vertical cross sections of fire plumes uncolocated with clouds. That would make your injection height validation more convincing. Large fire plumes has been found in the arctic since April 2008. An other suggestion is comparing the injection height of the 1D plume rise module with different horizontal resolutions (MISR data being the reference) by doing a grid nesting over the Russian or Canadian fires. At least we would know if the horizontal resolution of models is an important parameter for calculating injection heights.

Specific comments:

These are suggestions to improve the paper.

Introduction on the use of WRF-Chem compared to offline models

P26556 "... The importance of WRF's increased resolution to improve the forecast skill of low level winds has been demonstrated by Mass et al. (2002)." Your sentence can be misleading. The paper from Mass has shown the effect of reducing the resolution from 36km to 12 and 4km on the forecast skill of WRF. It doesn't talk about systematic improvement compared to global models. Furthermore, I don't see any improvement of the resolution by using WRF at 45km compared to the 0.5x0.5 degree NCEP GFS data ... The authors mention that WRF doesn't have uncertainties coming from spatial and temporal interpolations that Lagrangian models have. I agree but in contrary WRF,

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being an Eulerian model, has numerical diffusion especially at 45km. Is 45km good enough to neglect numerical diffusion for a long-range transport study? The authors should add a comment about it in their introduction.

On the preprocessors

The first part of the paper evaluates 2 preprocessors that treate differently the fire emission and hourly heat fluxes. The paper lacks details on the differences of the treatment of burned area between "prep chem model" (used in WRF-Chem) and FLAMBE. I would have liked to see also a sensitivity test on the use of the fire radiative power to calculate the sensible heat flux of each fires rather than using vegetation categories like in FLAMBE.

Injection in PBL or above

Val Martin et al. (2010) and Kahn et al. showed from Satellite data and GEOS 4 that the mode of injection height was close to the PBL height. They found that 5 to 18% of the fire plumes was above the PBL height. Brioude et al. (2009, ACP) evaluated the plume rise module that is used by the authors with the same NCEP GFS dataset and they showed that 30% of the plumes is injected above the PBL height and that the mode of injection height is matching the PBL height. Because the mode of injection height is close to the PBL height, the way that the PBL height is calculated has a strong impact on the fraction that is above or below the PBL. It seems that in page 26564 and 26565 the authors are questionning the PBL height in WRF-Chem and GEOS 4 to explain the differences. Perhaps the authors could show the results of figure 6 with the PBL height from the NCEP GFS? Furthermore, it would be interesting to see the probability density function of injection height in Figure 5 relatively to the PBL height. Varying the horizontal resolution of WRF to see any differences on the injection height would be interesting.

Long range transport and injection height parameterization

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Having a simple parameterization for fire plume injection height is important because the use of a 1D plume rise module can be time consuming in a model. Concerning injection height parameterization, the authors compared two "traditional" parameterizations (homogeneous injection in the PBL, homogeneous injection between 3 and 5km above ground level) with the plume rise module. I would say that homogeneous injections between 0 and 3km or 0 and 5km are more popular in the recent literature than 3-5km. At least such a simple parameterization would result in an injection both in the PBL and the free troposphere. I would like to add that, as I said before, previous studies have shown that the mode of injection height is close to the PBL height. Perhaps it would be good to use an homogeneous injection at + or - 1km relatively to the PBL height (+-1km is roughly the width that is found in Fig7 of Val Martin et al., 2010). It would be interesting to see the difference of this simple parameterization with the plume rise module for long range transport cases.

The second part of the paper compares the 3 different injection parameterizations by comparing the long range transport of fire plumes with satellite data. The plots that compare the vertical cross sections from WRF-Chem and CALIPSO are not convincing at all. Most of the time the discussion on aerosol detection from CALIPSO is based on few grid cells and not a single continuous aerosol layer is detected. The reason is that clouds are always colocated with the aerosol layers during the time period of interest. I understand that over a time period of 2 weeks the authors didn't find any aerosol layers in CALIPSO with no clouds. The authors should extend their time period of interest to the spring and summer seasons of 2008. Fire plumes have been measured in the arctic since April 2008. It would increase their chance to have aerosol layers in CALIPSO and would make this section more convincing.

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