Geosci. Model Dev. Discuss., 6, C77–C80, 2013
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Geoscientific
Model Development
Discussions

# *Interactive comment on* "One-dimensional simulation of fire injection heights in contrasted meteorological scenarios with PRM and Meso-NH models" *by* S. Strada et al.

**Anonymous Referee #1**

Received and published: 13 March 2013

The paper deals with a pressing problem of vertical profile of the smoke injection from wild-land fires. The authors continue investigating the episodes, some of which they already looked at in previous publication. The study is based on numerical simulations using two plume-rise models, which are compared with each other. I have read the paper with high interest and should give credits to the authors for their detailed analysis. In fact, too detailed sometimes: tiny specifics of selected cases evidently cannot be generalized, so the interest of a general reader to those is bound to be low. I also found one major problem of the study: the authors do a great deal of modelling but have not demonstrated that the results have any connection to reality. The model-measurement comparison is entirely absent. This is particularly surprising because the episodes



are quite well recorded from meteorological standpoint: the authors use sounding and ECMWF analyses, as well as in-situ air quality information that are available (in-situ AQ information, however, is rather referenced to the previous study). But not a single sentence said about the actual subject of the paper: the vertical profile of the smoke injection. As a result, at the end of the paper the reader is left guessing, which of the model features are valid, important, and represent the reality. I was grossly missing such comparison in previous works of Freitas et al and Strada et al too. But Freitas et al, 2010, at least promised some comparison in the forthcoming publications. So, it is finally time to show something. Since handling the observational information for specific episodes may be not trivial, especially if it is to be deciphered from instruments like MISR, its addition is a significant work and a major change to the manuscript. Below, I also put a few comments, some of those are significant, especially those pointing out at weaknesses of the approach. They also should be addressed before considering the paper publication.

Abstract Too long and not describing the actual work. Everything down to line 12 (p.722) is a generality and should be deleted.

Introduction p.724 l.25-30 and 725 l.1-5. That sounds to me like a mixture of atmospheric dynamics and smoke injection height. The authors say that fires affect the dynamics, i.e. can significantly change the vertical mixture of the air. The next sentences, however, point out the experiment with three chemical transport models, which have wrong injection height. But these are two problems connected only indirectly. p.724 l.15-20. I would be much more careful with volcanic analogy: volcanic plumes are much denser, contain very coarse particles and usually dry. Also, their injection height is largely controlled by the initial kinetic energy of explosion, which makes the analogy even weaker. p.728 l.10 kilometric

Section 2. Data sets A great attention is given to meteorological conditions but nothing is said about observing the plumes themselves. In-situ data mentioned by previous work of Strada et al, 2012 are evidently not sufficient to obtain the 3D picture, especially at such scales. This is related to my main objection stated above. Sounding profiles are taken several tens of km away from the fires. Why do the authors silently assume that they represent the conditions in the vicinity of the fires? I understand that it is convenient – and probably true for the upper troposphere – but for boundary layer and lower troposphere already 10 km distance is much too far. Section 3. Descriptions of the models p.733 l.1 "mainly" ? p.733 l.4-6 1-D models are based on several major simplifications including cross-section integrated circular-shaped plume. Since the later is evidently wrong in case of fires, I would not call these models "ideal". Understandably, everybody takes such models or falls back to even cruder approaches but saying that this is the ideal thing to do is going too far. I would remove these lines. Section 3.1 The authors should say upfront that Meso-NH model is equipped with subgrid convection dynamics. Otherwise the reader gets confused: in the introduction the authors criticize the approach of meso-scale modelling due to its low resolution, insufficient for resolving the plume - yet for the analysis they select one of such models. p.733 l.26-27. I did not get this. How did the authors force the final atmospheric state? Isn't it a result of the model integration? What kind of forcing was used to bring the model to the undisturbed state by the end of an hour if the fires lasted longer? This looks like a significant problem of the setup. p.734 l.15-16. Why these values? Section 3.1.1 I see no reason for this section to exist. Equations are from textbooks, none of them is used further. May be, the last paragraph can be added to the end of the previous section, the rest should be deleted. p.739, l.10 parametrized Section 3.2 The section is much too long. The 1D PRM has been presented several times and the equations are standard, so the section can be shortened at least by half without any loss of information. p.746, l.1-2. I did not understand the conceptual difference. The sentence is unclear. p.746, l.15-23. I would say that the water vapor mixing ratio is not comparable between the models: comparison of the updraft and the environment does not make much sense. p.747, l.11-20. Turbulent parameters are of interest by themselves but they evidently cannot be marked as the comparison variables since PRM does not have them. They should be removed from this section. p.748, l.14 feeds . . . slows down Overall for section 4.1:



what discussed is not metrics but rather the compared variables. Metric is, for example, a root of mean-square-error RMSE. The section title should be changed. I would also move this sub-section into the methodological section 3. There is no results here.

Sections 4.3 – 4.5. Long and boring description of the results, which, in many cases, cannot be generalized. The reader gets quickly tired of unimportant details and jumps right to Discussion. Should be shortened.

Section 5. Also quite lengthy and would benefit from better structuring with a few subsections identified and the text rearranged respectively. And, of course, this is the place for actual measurements of the fire injection profiles and respective model evaluation.

Conclusions Too long. Many repetitions of other sections, some discussion, etc. Should be significantly shortened and made more concrete. To the end of the day, what should the users, meaning the atmospheric modellers, take as a lesson? What should and what should not be used?

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Interactive comment on Geosci. Model Dev. Discuss., 6, 721, 2013.