Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-8-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Inverse modeling of fire emissions constrained by smoke plume transport using HYSPLIT dispersion model and geostationary satellite observations" *by* Hyun Cheol Kim et al.

Meelis Zidikheri (Referee)

meelis.zidikheri@bom.gov.au

Received and published: 17 May 2020

General overview: This paper uses an inverse modelling approach to estimate fire emissions at various locations as a function of time and height by making use of satellite measurements of smoke optical depth. The method was applied to a November 2016 wildfire event in the USA. Various sensitivity tests were performed including the effect of different satellite observation time windows, maximum source height, and domain size. The results were compared to results obtained with the smoke forecasting system currently in use and it was found to yield better agreement with observations

as expected. Having estimated the fire emissions, forecasts (or rather hindcasts) were produced by making use of these optimal emissions. It was found that the forecasts were generally better than corresponding forecasts obtained by the conventional system, but the improvements were modest in my view, particularly for longer lead times. Nevertheless, I think this is a solid piece of work, and would recommend that it be accepted for publication. However, there are some issues that need to be addressed by the authors as listed below, which in my view would improve the paper. In addition, in my view the manuscript would benefit from another round of proofreading. Main issues: 1. How the experiments with different assimilation time windows are run needs to be made clearer. Is it the case that in all experiments the emissions are initiated at the same locations and times, the only difference being the satellite data assimilation time window? If that is the case, can you clarify why longer time windows results in better agreement with observations for the analysis? I would have thought it would be easier to fit the model to data from one timestep than to fit the model to data from different timesteps. I can not quite understand the explanation given in the paper (e.g. Line 240), which I interpret to mean that this is due to emissions at (x1,t1) affecting the smoke field at (x2,t2). This is true, but I would expect better results for the smoke field at (x2,t2) if there was no observation at (x1,t1) because the emission at (x1,t1) would only be constrained by the observation at (x2,t2) and not (x1,t1) and (x2,t2) simultaneously. You probably also need to clarify which data points are included in the verification statistics (for example, when you assimilate data on day 0, do you calculate verification statistics based on day 0 only or do you also include days -1, 2 etc?). 2. In performing the forecasts, it is not completely clear whether you are using NWP analysis data to drive the forecasts, or you are using NWP forecast data. If this system is to be used operationally, then clearly NWP forecasts will need to be used, which we expect will result in poorer forecasts than when the NWP analyses are used. I think this needs to be clarified. 3. Nothing has been said about analysis and forecast errors. Can this system be used to predict errors (uncertainty) or is it purely deterministic? I think some comments on this issue in the paper would be useful. Detailed comments: 1. In the ab-

stract you have several acronyms that are only defined later in the text. These must be defined in the abstract or alternatively think about removing them altogether from the abstract. In my view they are not that important for the abstract and can be removed. For example, is it important to know that the model has been developed by NOAA in the abstract? 2. Line 32: Remove capital letters from words 2-6 in the sentence starting with "Meeting. . .". 3. Line 43 (or thereabouts): You need to state that HYSPLIT is a Langrangian model (and maybe elaborate on what means) as in the next paragraph you start talking about Eularian models. 4. I also notice that you don't leave spaces between paragraphs, which makes it hard to read at times. Fix this if you can. 5. Line 54: You distinguish "top-down" and "bottom-up" approaches to estimating fire emissions. I think it would useful as well to discuss the advantages and disadvantages of these approaches somewhere in the introduction. 6. When discussing the methodology it might be worthwhile to explicitly mention that the "inverse system" enables the calculation of fire emissions that produces an "analysis" smoke field covering the observational time window (near past to present) and that these optimal emissions are then used to forecast the future smoke field. I do not think this has been made explicit although it may be obvious to the expert reader. 7. Another point that I think needs to be made clear in the methodology section is how the experiments with different observational time windows (oday = 0, -1, 2 etc) differ from each other in the model setup. For example, for oday = 0, is it that there are no emissions prior to day 0, or is it that emissions prior to day 0 are unconstrained (i.e. they use prior values), or is it that the emissions prior to day 0 are in the model but they are constrained by observations on day 0 only. I suspect it is the last scenario, but this needs to be made clear. You could include this information in the paragraph starting at Line 177, which I think needs more clarity. For example, you could indicate that oday = 0 means that emissions from the model start time until day 0 (for example, November 13) are constrained by observations on day 0 only; oday = -1 means that emissions from the model start time until day 0 are constrained by observations on day 0 and day -1 (November 12) etc. As I mentioned, it is not clear that is indeed the experimental setup, but whatever it is, it needs to made clear. When

you calculate the verification statistics for the oday = 0 type experiment, for example, do you include all data (for days 0, -1, -2 etc) or just data for day 0? Please make this clearer. 8. It would also be useful in the context of Section 3 (or elsewhere like Figure 1 or in the conclusion) to say something about how the two systems (Blue Sky/Initial? and Inverse) really need to complement each other in practice. Apart from providing the all-important first guess (or 'prior') to the inverse system, I would imagine that the Blue Sky system would also be needed to provide the first operational forecast when there are not yet any (or sufficient) observations to assimilate. In addition, in situations where there is extensive cloud cover, I would imagine that there would not be observations of smoke that could be used. This comment is related to Comment #5 above. 9. Line 219: The notation associated with this equation needs to be improved. Although I think I understand what you are trying to say, as it stands it is ambiguous. Firstly, it is not clear what "smoke" is supposed to be. I think you are looking at smoke mass loading, or it could be optical depth. Whatever it is, it needs to be made clear. I would use a single-letter symbol to represent the smoke variable. Since "c" is used in the first equation (Lines 148-149) that is what should be used here as well for consistency. Similarly, I would use a single letter to represent the TCM matrix coefficient (T seems like a good choice). Apart from that the equation doesn't look mathematically correct. Summing over the indices i, k, t should produce a scalar, independent of i, k, t but we know that the smoke field (mass load or optical depth) is a function of location, i, and time, t. So something is missing. I think some more work needs to be done here so that the equation makes sense. 10. Line 226: change "during" to "when". 11. Line 226: Last sentence of paragraph. Some more needs to be said here. Why is there no data on November 17? In addition, you are implying that no satellite data was used in the analysis (presumably because there was none – you must clearly state that if that is the case, and if so, why). And yet in the caption to Figure 5 we are told that a 48-hour observation time window was used. If that is the case, why could not the data for November 16 be used to constrain the analysis? Is it because your algorithm does not perform optimisation if any time steps are missing in the observational time

window? You might have mentioned this elsewhere, but how frequent are the satellite observations? Every hour? Every 10 minutes? 12. Paragraph starting at Line 234: This is related to Comment #7. If you are using emissions initiated at the same time in all experiments in which you vary the length of observational time window, it is surprising, I think, that you get a better fit with a longer observational time window. Intuitively, all things being equal, you would expect to get a better fit with a smaller number of data points during the analysis. Of course, for the forecast you would expect better results with more data points during the analysis because you are less exposed to overfitting problems with more data points. It looks like you're talking about the analysis here so can you make it clearer why using data from earlier times (say day -1, -2 etc) makes the analysis better on day 0 for example? I find that a bit unclear. I can see that if you are using data from all days to calculate your forecast verification statistics and not only on the days that data is assimilated, you would get better results by using more data because you are better constraining the smoke over a longer period. If that is the case, please make it clearer which data points are included in the verification statistics. 13. Paragraph starting at Line 242: When I first read this, it sounded like you are varying the vertical resolution of the sources in these experiments. But is turns out you are just varying the maximum heights of the sources. I think you should consider rewording this so that it is immediately clear what you are doing. In some sense, I think varying the vertical resolution and keeping the maximum height the same would be a more interesting experiment. Rising plumes tend to have a neutral buoyancy level at which most of the mass tends to be detrained from the plume. This is true for volcanic plumes; I don't know if it's true here. So, changing the vertical resolution may have a significant impact on the results. 14. Line 255: You must say something about the outcome of the "third test" here. Would you say coverage does not matter? Or is it better for bigger coverage (Domains 3 and 4)? If the results are inconclusive, say so. 15. Line 271: You need to say something about "p" here even if briefly; for example, "p" is the persistence rate that measures . . . as explained further in Section xxx. 16. Lines 298-307. Given the significant uncertainty in "p", an ensemble modelling approach might be fruitful in

the forecast mode. Have you looked at this? If not, it might be something you could look at in the future. 17. Line 325: What is "PM"?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-8, 2020.