Review of "Simulating Wildfire Emissions and Plumerise using Geostationary Satellite Fire Radiative Power Measurements: A Case Study of the 2019 Williams Flats fire"

Submitted to ACP

Summary

Accurately forecasting wildfire smoke concentrations with atmospheric models is challenging. Efforts to improve the performance of these tools, such as the analysis described in this paper, are important for helping air quality agencies effectively communicate warnings to the public ahead of severe smoke conditions. My comments highlight a few specific areas where the paper could be improved.

General Comments:

- More references to previous work would be helpful throughout, particularly in the Methods section when specific models/schemes are mentioned.
- Some of the discussion of FRP-based emission estimation approaches like GFAS and how they relate to more biogeochemically comprehensive datasets like GFED needs to be adjusted. The current presentation of these relationships is unclear/misleading. Please see specific comments on lines 117 and 120-122 below.
- A comparison of the emission factors used in the Freitas et al 2011 paper, the FRP approach described in this work, and more contemporary factors (e.g. SERA) would strengthen this paper and make it more broadly useful. Some other fire emissions systems use more recent emission factor estimates, so it would be helpful to understand if incorporating the FRP approach used here would yield similar improvement in those cases, or if much of the improvement observed in this work is in fact due to updated EFs compared to the 3BEM approach.

Specific Comments:

Lines 88-90. This sentence is a bit vague, could you perhaps be more specific about what aspect of wildfire regimes are you referring to here? E.g. changes in land cover/fuel loading, changes in ignition patterns due to expansion of the wildland-urban interface, etc

Line 106. It seems relevant to mention the representation of other meteorological variables here as well, e.g. wind direction.

Line 117. It seems important to point out here that the GFAS system relies heavily on relationships between FRP and land cover-dependent biomass consumption rates derived from the GFED dataset (see Kaiser et al 2012 Table 2 and discussion in section 2.3). Fundamentally you still need some estimate of fire size/location x fuel available x fuel consumed x emission factor to get an estimate of fire emissions. FRP-based approaches leverage relationships between some of these variables built from existing datasets to quickly combine some of these steps rather than calculating them explicitly.

Line 120-122. Again, this is a misleading assertion since FRP based approaches are built on existing datasets with more comprehensive biogeochemical modeling like GFED in order to calculate emissions per detection quickly. It would be more accurate to say something like "FRP based approaches like GFAS are able to leverage key relationships, e.g. land cover specific consumption rates, from more comprehensive biogeochemical datasets like GFED in near-real time".

Line 191-198. It would be helpful to have more references in this section, for example is there a reference paper specifically for development of the "Arakawa Staggered C-Grid" mentioned on line 191?

Line 204. It seems overly specific to associate the model configuration you're using with a particular institution (even if that's where you're running the model), is there something unique but internally consistent about how the University of Wisconsin Madison group runs WRF-Chem compared to everyone else? Is this the official citation? I just haven't seen this done before.

Line 221. Is AOD actually calculated at 550 nm in the radiative transfer code? Or is it calculated at several other wavelengths and then interpolated? My understanding is the more common approach is to interpolate to 550, just clarifying.

Line 222. Please specify, how is hygroscopic growth accounted for? Lookup tables? Is there a reference for the approach?

Lines 225-226. Please include references for these models/schemes.

Line 239. Please provide relevant references demonstrating where in the literature these mechanisms have been evaluated.

Line 280. Can you specify what type of analyses? E.g. observational data from nearby monitors?

Lines 288-292. It might make sense to make the description of the diurnal functions its own section here and go into a bit more detail about how this was done.

Lines 304-305. Did this sentence get cut off? The "2 MODIS" is confusing, 2 what from MODIS? The two sensors on Aqua and Terra?

Line 455-457. It looks like the FRP based approach just generated higher emissions in general, which could be due to any number of factors. It's difficult to say since the values are shown on a log scale, but it looks like the relative change between August 3 and August 8 was actually larger in the 3BEM approach? I'm not sure it's appropriate to draw conclusions about the sensitivity of one approach vs. another to variability in fire behavior based solely on this figure, but perhaps I'm missing something.

Line 636. I recommend softening the interpretation of when things are "improved" or not in the FRP approach, here and throughout - it also looks like there are places where the FRP approach overestimates smoke emissions, so there may be an element of "right for the wrong reasons" in some cases. Trying to identify the specific sub-component of the estimate, e.g. fire size, fire location, fire timing, fire intensity, type of fuel, fuel moisture content, emission factor used, etc is critical for this type of exercise because otherwise you can scale emissions up or down to get "better" performance without knowing if the representation is more accurate at a process level.

Line 966. I'm not following the importance of the statements after "However" in Conclusion #4. In my view the main conclusion from the plume height comparisons was that use of the two different emission schemes didn't substantially alter the plume height representation, indicating that in these cases at least plume height representation in the model wasn't a central factor in the difference in performance between the two approaches compared to the flight data.

Line 975. It might be worth noting that conclusion 6 seems to be more of a second-order issue – other studies have shown that a simple scaling of ~1.5-2 on the OC allows for a decent representation of aerosol mass in many cases. The big first order issues relevant for modeling smoke transport seem to be more related to some of the other variables explored in this work, such as capturing fire size/location/timing, application of specific emission factors based on a variety of characteristics, and how those inputs interact with the representation of local meteorology.

Technical Corrections:

Line 218. Please explain what OA is (I didn't see where the term was introduced).

Line 504. I think you defined AOD above, don't need to redefine here