Geoscientific Model Development Discussions

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Comment on gmd-2022-54

Anonymous Referee #1

Referee comment on "Introducing the VIIRS-based Fire Emission Inventory version 0 (VFEIv0)" by Gonzalo A. Ferrada et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-54-RC1, 2022

In this manuscript, Ferrada and colleagues present a newly developed biomass burning emission inventory, the VIIRS-based Fire Emission Inventory version 0 (VFEIv0). There are multiple bottom-up (burned area based) and top-down (satellite based) biomass burning emission inventories out there, but VFEI does offer unique advantages, especially the high native horizontal resolution. Overall, this is a sound effort, and aligns well with the scope of the journal.

The source characteristics of wildfires is a long-standing issue and major source of uncertainty in representing wildfires in air quality models and chemistry-climate models. One key issue faced by the broader community is that, we have multiple regional and global biomass burning emission inventories available and the results are vastly different. Due to the lack of direct emission/flux observations, it is extremely challenging to evaluate the individual inventories. This study (and many others) uses AOD, but quite a few other processes are involved as well (dispersion and transport, aerosol chemistry and physics, etc) and all together they affect AOD. Frankly speaking I am not entirely sure what we can actually learn from all those inter-comparison studies. Put it differently, we are limited by observations more than ever. However, novel approaches reporting (direct) emission flux/rate estimates for wildfires are becoming available, for instance, Stockwell et al. (2022), Bela et al. (2022), Wiggins et al. (2021). Obviously these observations are limited to isolated regions and fire seasons, but these are very rare (direct) emission flux/rate estimates that actually offer direct scientific insight into this otherwise poorly contained issue. In light of this, it will greatly strengthen this manuscript if the authors can take advantage of these unique datasets. Stockwell et al. (2022) and Wiggins et al. (2021) are available for the FIREX-AQ period.

This manuscript is generally well written, although certain aspects of the data analysis can use some improvements. I particularly enjoy Section 5, in which the authors demonstrated how fire source may be misplaced if a coarse resolution inventory is used. I recommend this manuscript for publication after the following comments and concerns are addressed.

Specific comments:

L32: Global warming refers only to the rising global mean temperature. It is my opinion that climate change is a broader and better term in this context which includes several other key changes along with the warming trend.

L65: "…often provide data for a limited number of species…" Technically inventories can provide data for whatever compound/species as long as emission factor/ratio is available. Due to historical or legacy reasons some inventories report data for selected species only (e.g., more comprehensive measurements become available more recently; certain inventories were initially developed for certain models or chemical mechanisms). Even if the emission factor of a compound of interest is not reported in a certain inventory, usually one can easily apply some scaling factor or emission ratio. The uncertainty associated with such scaling is usually smaller than the uncertainty introduced from other processes. Therefore this is not really a limitation or issue, in my opinion. The high native horizontal resolution, however, is a major advantage of VFEI.

L66-67: "These can be a problem in model simulations since missing species can be highly volatile (e.g., hydrocarbons) and, thus, producing unrealistic results in smoke plume composition." I don't quite follow this. Please clarify.

L75: It has been well documented that wildfire emissions do have diurnal variabilities, with major impact on emissions. E.g., Wiggins et al. (2020). The authors should acknowledge that not having sub-daily variability is one important limitation of this inventory. I do understand that it is challenging to develop a global biomass burning emission inventory with sub-daily temporal resolution.

L95: Heil et al. (2010): this citation in the reference list is missing key details. Please refer to the journal guidelines on reference style.

L94-97: Please discuss any potential biases that may be introduced by using MODIS-based conversion factors and FRP derived from VIIRS. Previous studies (e.g., Li et al. 2018) show that VIIRS and MODIS FRPs are broadly comparable but they do show discrepancies in certain regions.

L133-135: Wouldn't this (simply taking an average between day and nighttime) create a systematic bias depending on overpass time? Please elaborate.

L207: please briefly describe what improvements are introduced in FINNv2.4, which lead to a ~2x difference compared to FINN1.5 (Figure 3).

L232-236: please see my comment above regarding the recent biomass burning emission flux/rate measurements. Please consider comparing FVEI to Stockwell et al. (2022) and Wiggins et al. (2021).

Section 4.3: (1) Emission is just one piece of the puzzle. Multiple other factors have profound impact on AOD as well, for instance, the size distribution, optical properties, hygroscopicity of smoke aerosols. Please briefly discuss if WRF-Chem captures these properties of smoke particles. The comprehensive measurements from FIREX-AQ may be useful. (2) The authors only present model simulations using VFEI. Why not show a few more simulations with other emission inventories?

L369: Another specific reason that may at least in part explain the widespread model underestimation in AOD as shown in Figure 9 is the long range transport of Siberian smoke. See Johnson et al. (2021). If I remember correctly, the front of the Siberian smoke reached the Pacific Northwest on 3-4 August 2019. Does the boundary condition used in WRF-Chem includes Siberian smoke?

L399: The authors ought to be careful about the language. Many of the instruments are sensitive enough at such concentrations (e.g., hundreds ppb of CO), therefore the high resolution data may well reflect the real features rather than noise. I understand that the model won't be able to resolve such features, therefore moving averages like the authors did here makes sense. But the reason is not because of the measurements are filled with noise but simply because the model cannot resolve these features.

L404: "This may suggest that the background CO in the model is underestimated, rather than VFEI emissions themselves…" Well if the authors subtract the background and focus on only smoke, one will be able to tell if the bias is driven by background CO or fire emited cO.

Figure 11: Wildfires are challenging to represent in Eulerian models. Even with "perfect" emission inventories, a model may still underestimate the pollutant concentrations in fire plumes for the following reasons: numerical dilution (point sources); numerical diffusion; model places the fire source in the wrong grid(s); issues in plume rise. Therefore it is always challenging (sometimes unfair) to perform precise apple-to-apple comparison like in Figure 11. I would recommend that the authors show the ratios too, e.g., BC/CO, OC/CO, etc.

L413: Again, many of the the high resolution airborne measurements are sensitive enough for fire plumes and hence often reflect real features rather than noise. e.g. Palm et al. (2021), Wang et al. (2021)

L415: "WRF-VFEI produces accurate results when comparing CO concentrations of less than 150 ppb…" I wouldn't say accurate here. If the authors produce the same 2D distribution plots (same as Figure 11), most datapoints with <150 ppb CO would fall below the lower bound of the gray shading.

L417-418: "VFEI slightly underestimates the BB emissions over the North American Pacific Northwest during this period…" Well, without comparing to actual measurements of emissions, one cannot simply say this. Comparing to other inventories does not justify this statement since other inventories are not direct measurements either. Consider evaluating VFEI using Stockwell et al. (2022).

L423: "…using the nearest grid point to the trajectories of the flights, even a slight misrepresentation of the smoke can introduce large biases in the results" This is indeed a simple and widely used approach. I'd argue that using nearest 2D interpolation may reduce this impact. But I also agree that it is challenging for a model with coarse resolution to resolve fine features.

Figure 12: i'm curious why the authors do not show OM or OC, which accounts for the majority of smoke aerosol mass. Figure 11 does show OC. Also following my previous comment: it may be challenging for the model to capture the exact mass concentrations, but the ratios tell us more about the modeling system. For instance, Figure 12 tells me that CO is severely underestimated but less so for BC, implying that BC/CO ratio is perhaps overestimated, which indicates an issue in emission factors, fuel categories, treatment of burning condition, etc. Also the impact of anthropogenic pollution will be reduced by subtracting the background. This is necessary for CO since the CO levels in the background air are usually quite substantial.

Section 5: this is brilliant! Nicely designed and demonstrates the advantage of VFEI.

References

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