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Geoscientific Model Development Discussions

Interactive comment on "The Fire INventory from NCAR (FINN) – a high resolution global model to estimate the emissions from open burning" *by* C. Wiedinmyer et al.

Anonymous Referee #2

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Summary

The paper presents the FIre Inventory from NCAR, FINNv1, a model for global, high-resolution (daily, 1-km resolution) open biomass burning (BB) estimates. FINNv1 is designed to support atmospheric chemistry and air quality modeling at local to global scales. The model employs MODIS active fire detections to derive burned area. Because the MODIS active fire product is produced as a rapid response product, it is timely enough to support forecasting and 'near-real-time' applications. FINNv1 uses an updated emission factors which includes comprehensive EF for non-methane organic compounds (NMOC), as important and valuable feature not available in other

BB emission inventory. Additionally, FINNv1 goes a step further by providing NMOC emissions profiles as lumped species for 3 chemical mechanisms used in Atmospheric Chemical transport models.

General Comments

Inclusion of the most recent, comprehensive EF for NMOC is great. Providing species profiles for chemical mechanism is great as well and will support consistent of application among different users of the EI. The near-real-time aspect of the model, at the global level, is potentially a great benefit for air quality forecasting. The failure to provide a cursory evaluation of the MODIS active fire detection to burned area method used by FINNv1 is a concern. I reccomend this manuscript for publication after the authors address specific comments below.

Specific Comments

Ln 6-9: "Despite all these various efforts, the uncertainty associated with open burning emissions remains high, and often modelers do not have the spatial and/or temporal resolution needed to accomplish the required scientific goals."

How does FINNv1 address this need?

Ln 25: Why the choice of 20% confidence interval for rejection threshold?

P 2445, Ln 9: "Therefore, for each day, multiple detections of the same fire pixel are identified globally and removed as described by Al-Saadi et al (2008)."

Al-Saadi et al. (2008): ".. multiple detections of the same fire (i.e., within the same nominal 1 km x 1 km pixel area) are identified and removed" Need more detail on how possible double counts, either by consecutive satellites (Terra/Aqua) or overlapping of satellite passes (e.g. overlap of consecutive Terra passes). If a hotspots falls within 0.5 km of a previous detection during that day (local day? Also are only daytime OP used or both day and night?) it is rejected as a duplicate detection? What about fires that trigger detections over multiple days? Has this been verified to be unimportant or, if it is important how has it been treated? A single MODIS active fire detection may have one or more subsequent active fire detections, which occur one to several days later, that fall within a distance of 0.5 km of the initial detection. It would appear the approach is susceptible to double or multiple counting due to spatially proximate active fire detections that occur over a period of days. Multiple counting can result from a couple sources.

A small burning area, $\sim$ 100 m2 , only a tiny fraction of a 1-km2 pixel, may trigger an active fire detection (Giglio 2003) under optimal conditions. Within the pixel containing an initial fire detection, only small fraction of that pixel area may actual burn during the day of initial detection. It may take a fire front multiple days to progress through a pixel. A fire progressing at a slow or moderate rate may move through the initial pixel over several days, potentially triggering one or more additional active fire detections.

Some vegetation types contain coarse woody debris and/or duff layers that may continue to burn for extended periods following the passage of the initial fire front. Residual, post-frontal combustion may produce sufficient heat to trigger an active fire detection in a subsequent overpass.

Multiple detects from these issue will probably may not be important in fast moving savanna / grassland fires or agriculture, and could be offset by missed detections. But the impact could be significant on forested regions.

Giglio et al (2006) reported on fire persistence, a measure of the number of days a 1-km cell registered a MODIS active fire detection. Their analysis found that the forested areas of Boreal North America, Boreal Asia, and NW US had high mean fire persistence which they and attributed this to heavy fuels and slow movement of fires in these forest dominated regions. Their analysis, which used only MODIS – Terra observations, suggests multiple counting over 2+ days may be an issue in the FINNv1 burned area estimation algorithm.

The authors should address this issue. What impact could it have? Fire perimeters are

available for Canada and the US. With minimal effort these perimeters could be used assess the role (or lack thereof) of multiple counting in forest regions. Maybe the use of the VCF washes out multiple detects. Whatever the case, the impact of fire persistence / multiple counts needs to be explicitly dealt with by the authors.

Giglio et al (2006) compared monthly total MODIS (Terra only) hs versus mapped burned scars and found a linear relationship between aggregate hs and burned area. The coefficient was highly dependent on region and vegetation type and varied from 0.4 – 6.6. This suggests accuracy of the FINNv1 method could vary dramatically and systematically across regions. The authors need to include some burned area evaluation that shows how well the FINNv1 burned area compares with some reasonable 'ground truth' proxy in different regions / cover types. As mentioned above, reasonable ground truth data (incident fire perimeters or burn severity products e.g. www.mtbs.gov) is readily available for wildfires in Canada and the US. The MODIS burn scar product (MCD45) could be used to spot check FINNv1 burned area in other regions. I not recommending an exhaustive evaluation, but in light of the potentially for large, region dependent error in the FINNv1 estimated burned area, the paper should include an evaluation of the burned area versus 'ground truth' across important regions.

Pg 2447, ln 9: With the exception of crops in Brazil sugarcane, FINNv1 uses crop fuel load of 500 g/m2 from Wiedinmyer (2006) which was a regional model for North America. Could the authors comment on the applicability of this North America crop fuel load number to other regions of the globe?

Pg 2447, ln 10: Croplands in Brazil were assumed to be all sugarcane. Is this a good assumption? Are other crops (e.g. soybean, wheat, cotton, others?) insignificant relative to sugarcane due to area planted or agricultural practices that do not involve burning? A sentence justifying this assumption is warranted.

Pg 2447 Ln 25: "The amount of woody fuel available to burn at fire is determined by the fraction of tree cover and the fuel loading for specific land cover type and global region;"

Multiplying the Hoelzemann (2004) woody fuel loads by the MODIS based fractional forest seems to be incorrect application of these data. The Hoelzemann fuel loads are based on the LPJ global vegetation model (Sitch et al., 2003). According to Sitch et al., the individual carbon pools are scaled to population density, i.e. the average density of forest cover is implicitly included in the calculation of the carbon pools (fuel loads) for each land cove type. The approach used in FINNv1 would unnecessarily scale down the woody fuel loads.

Table 8. Why not compare FINNv1 NMHC vs GFEDv3.1 NMHC, instead of NMOC/NMHC or both since the GFEDv3.1 NMHC definition doesn't equate exactly with either FINNv1 definition?

Figure 3. In the July panel the Europe/Asia NH emissions end abruptly at $\sim$ 67.5 deg. Is this an error in the rendering of the figure or is this the model prediction? If this is real (real in the model that is) explain why.

P 2453 ln22 - 26. I don't believe these will necessarily tend to cancel. Especially if multiple counting is an issue (see comments above and Giglio 2006). These may offset in regions where Converting an active fire detection to a burned area is also a large uncertainty (see e.g. Giglio 2006) and comments above.

I suggest the authors include a bar plot showing avg annual CO2 (or CO) emissions by cover type (maybe broken out by hemisphere as well). This would be quite helpful in assessing the relative importance of different fire activity.

Technical Comments Table 2. Should use footnotes in the table to signify which fuel loads are different from Hoezelmann (2004).

Reference: Olivier et al. (2005) could only find unhelpful reference on Edgar web-site (http://edgar.jrc.ec.europa.eu/publications.php). Does not come up in ISI Web of Knowledge. Difficult to find/obtain.

Refs:

Gilgio et al (2003) Remote Sesning of Environment 87 273-282 Giglio et a. (2006) Atmospheric Chemistry and Physics 6 957-974

Interactive comment on Geosci. Model Dev. Discuss., 3, 2439, 2010.