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Geoscientific
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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 #1

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The authors present the 2005-2010 FINN global biomass burning emissions inventory at 1 km spatial resolution and daily temporal resolution. The subject matter is appropriate for GMDD and will be of interest to readers. While I have no major issues with the emissions model used to produce the data set (indeed, I think this portion of the work is quite good), I believe the approach used to produce the daily burned area input data used by the model is flawed (see detailed remarks below). While I fully appreciate the fact that for near-real time applications (as this emissions model may be used) one must always sacrifice some degree of accuracy for speed, there is a point at which the accuracy simply becomes too low to be useful. The authors should demonstrate that their method for estimating burned area globally has not lapsed into

this realm.

Specific Remarks

Page 2445, line 3: “For each fire detected in this region, the fire is assumed to continue into the next day at half of its original size.” But is this assumption reasonable? The source cited here (Al-Saadi et al., 2008) doesn’t justify it either.

Page 2446, lines 19-27: The conversion from active fire counts to area burned described here is the same ad hoc procedure used by Wiedinmyer et al. (2006) and Al-Saadi et al. (2008) to estimate area burned in (primarily) the United States. This procedure was justified in part by referring to an “average area burned per Terra MODIS fire count” proportionality factor of 0.84 km^2/pixel reported in Giglio et al. (2006) for CONUS. However, Giglio et al. found quite different factors (0.29 to 6.6 km^2/pixel) in other regions of the world. It therefore seems unreasonable to expect the CONUS-specific conversion used by the authors to serve as a reasonable choice for the entire world. Of particular concern is the fact that in tropical forests (where bare fraction = 0%) the authors use a conversion factor of 1 km^2 per MODIS fire pixel, which seems much too high – see Giglio et al., (2006,2010) cited in text, and Roy et al, (2008, RSE). For African savanna fires I expect the approach described in the manuscript to have the opposite problem, greatly underestimating area burned (perhaps grossly so).

Since the emissions estimates are critically dependent on area burned, the authors should demonstrate that their burned area estimates are actually reasonable, preferably by validating them for representative areas. It would also be good to include a summary of the FINN burned area to help disentangle the reasons for the discrepancies between the different emissions data sets. A new table showing annual burned area totals (preferably on a regional basis) would be a good way to do this.

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Page 2457, line 8: “For many chemical species, the FINNv1 emission estimates agree well with other inventories; specifically the GICC and the GFEDv3.” This is only shown for the entire globe, a scale so large that the two data sets could still be completely inconsistent (e.g., the statement would apply just as well if all of the GFEDv3 emissions were in Antarctica). Please perform the comparison on a regional basis. One possibility would be to use the regions shown in van der Werf et el. (2010) and earlier GFED papers.

Table 8: Again, the comparison with GFED3 emissions would be much more useful if they were broken out regionally.

Technical Corrections

Page 2441, line 14: “largescale” → “large scale”.

Figure 4: The CO emissions shown the lower left panel seem to be cut off at about 60 degrees north in the eastern hemisphere. Please check.

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