We greatly appreciate the insightful comments of the two reviewers of the paper. They have made some valuable suggestions that have led to big improvements to the manuscript and the description of this model. Below are the responses to the comments (in blue). We would like to be clear that the focus of FINN is a customizable emissions product at high temporal and spatial resolution that can be delivered quickly. Burned area is not the primary product of this model.

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
Received and published: 26 January 2011

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.

This is an extremely valid point made by the reviewer, and the assumption used for this study has yet to be verified. We have added some text to the paragraph to highlight this. Since we have a lack of daily observations at the equator, and yet need daily estimates for forecast and air quality applications, we have smeared the emissions across a two-day period. Other methods smear the emissions over 8-, 16-, and month-long time periods, which will give more accurate results. However, we took this approach to capture the synoptic changes in fire emissions, and in an attempt to capture the synoptic scale atmospheric distributions. Further study is needed to better evaluate this assumption. Table 1 of Yokelson et al. (2010) suggests that fires in the tropics may be undercounted.

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 km2/pixel reported in Giglio et al. (2006) for CONUS. However, Giglio et al. found quite different factors (0.29 to 6.6 km2/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 km2 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).

The assumed burn area is very uncertain, and our default area burned assigned to each pixel may be under- and over-predicting the overall area (regionally-dependent as suggest here by the reviewer). Burned area per pixel still needs much more verification both regionally and by ecosystem type--- beyond satellite data. It should be noted that FINN is not strictly using 1 km2 per MODIS fire pixel, because FINN is excluding any pixels that fall within the 1km2 diameter of a previous detection. The Giglio et al. proportionality factors only consider Terra MODIS data (not Aqua). Terra is a morning overpass, which is not the optimal time to capture burning fires. Fires tend to lay down when relative humidity is high (evening, morning) and burn optimally at solar noon when relatively humidity is typically lowest. The FINN algorithm is using both instruments, which leads to more than twice the number of fire detections. For large fires in upper latitudes, there are duplicate overpasses and duplicate fires detections (these excluded), and in equatorial regions, we do not have daily global coverage. The objective of FINN is to capture the best available daily fire emissions. While we agree that the assumed burn area is highly uncertain, we believe that the assumptions made within this model framework are well within the uncertainty associated with burned area estimates. We have tried to adequately address the shortcomings of this approach in the uncertainty section.

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.

We agree with the reviewer that a summary of the area burned from FINN be included in the output. As a result, the new Table 8 has been added to include regional summaries of emissions and area burned. However, validation at these large regional scales is not yet possible, other than an evaluation with other satellite data or reported information (e.g., USFS BAER data or the USFS MTBS in the United States), which have their own uncertainty.

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.

The reviewers have a great suggestion here, and a regional analysis and results are now included in the paper. The new Figure 3 has been added to include a regional comparison between the annual average CO2 emissions (2005-2009) from GFEDv3.1 and FINNv1, using regions similar to those used by in van der Werf et al. (2006; 2010) (regions now in Figure 1). Text has been added to the discussion to address this new figure. FINNv1 predicts higher emissions in South America and Southeast Asia, whereas GFEDv3.1 produces higher estimates in Africa and boreal North America. The lower FINNv1 emission estimates in Africa could be the result in an underestimate of the area burned in this region where the assumed burn area per pixel could be too low for savannah fires, as mentioned by the reviewer above. Despite the fact that FINNv1 is higher than the emissions produced by GFEDv3.1 in Southeast Asia, Kopacz et al. 2010 suggest that GFEDv2 is low in east Asia.

Kopacz, M. D. J. Jacob, J. A. Fisher, J. A. Logan, L. Zhang, I. A. Megretskaia, R. M. Yantosca, K. Singh, D. K. Henze, J. P. Burrows, M. Buchwitz, I. Khlystova, W. W. McMillan, J. C. Gille, D. P. Edwards, A. Eldering, V. Thouret, and P. Nedelec (2010) Global estimates of CO sources with high resolution by adjoint inversion of multiple satellite datasets (MOPITT, AIRS, SCIAMACHY, TES). *Atmosp. Chem. & Phys.* 10, 855–87, www.atmos-chem-phys.net/10/855/2010/

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

In addition to the annual global total comparison, Figure 3 has been added to compare the regional annual average CO2 emissions from FINNv1 to GFEDv3.1. Additional discussion has been added to the text to address some of the regional differences between the model outputs. Regionally, the models differ, particularly in the southern hemisphere.

Technical Corrections
Page 2441, line 14: “largescale” ! “large scale”.

Corrected.

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.

The authors greatly appreciate this comment. There was a small mistake in the 2008 summer files which have now been corrected. The figure has now been fixed and the resulting emission estimates corrected.

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
Received and published: 3 March 2011

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 recommend 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?

We have edited this section, and we have added the sentence:
"We have created an emissions modeling framework that provides highly resolved emission estimates from open burning needed for specific model applications."

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

This was a number provided upon the recommendation of L. Giglio to A. Soja when they were working on the JARS manuscript (Soja et al. 2009). This citation has been added to the manuscript.

Soja, A.J., J. Al-Saadi, L. Giglio, D. Randall, C. Kittaka, G. Pouliot, J.J. Kordzi, S. Raffuse, T.G. Pace, T.E. Pierce, T. Moore, B. Roy, R.B. Pierce, J.J. Szykman (2009) Assessing satellite-based fire data for use in the National Emissions Inventory, *J. Appl. Remote Sens.*, 3, doi:10.1117/1.3148859.

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 hotspot 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, _ 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.

The authors appreciate these thoughtful comments of the reviewers. In this response, we try to address at least some of these concerns. Additionally, the text in this section has been edited to more clearly describe our methods.

First, all detections for both the Terra and the Aqua overpasses were used in the model. Our methods take all fire detections (from both overlapping passes, from different satellites) for each day and remove those that fall within a 1km2 diameter of one another. Therefore, for each 1 km2 "pixel" there can be only 1 fire per day. If a fire occurs the following day in the same location, it is counted again. Therefore, fires that prevail in the same location over several days are accounted for. This will ultimately include some uncertainty in the overall area and biomass burned. However, with the use of these satellite data, we are unable to better define the fire spread through a pixels.

As we responded to Reviewer #1, The Giglio et al. proportionality factors only consider Terra MODIS data (not Aqua). Terra is a morning overpass, which is not the optimal time to capture burning fires. Fires tend to lay down when relative humidity is high (evening, morning) and burn optimally at solar noon when relatively humidity is typically lowest. For large fires in upper latitudes, there are duplicate overpasses and duplicate fires detections (these excluded), and in equatorial regions, we do not have daily global coverage. The objective of FINN is to capture the best available daily fire emissions. While we agree that the assumed burn area is highly uncertain, we believe that the assumptions made within this model framework are well within the uncertainty associated with burned area estimates.

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 am 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.

We appreciate the need to compare the burn area (and other outputs) calculated with FINN to other regional estimates. However, as discussed on page 2455, the evaluation of burned area from various satellite products can lead to significant differences. The authors have looked at a comparison of satellite and reported burned area for the United States, and really depended on where the fires were and how they were reported. Other studies have shown great discrepencies between burn area products. For example, Roy and Buscetti (2009) state that in Africa "The linear regression lines all pass close to the origin (absolute values of the regression line intercept less than 0.015) but with variable slopes from 0.136 (L3JRC), 0.595 (GlobCarbon), to 0.750 (MODIS). A slope of one indicates that the product estimates the same extent of area burned as the Landsat independent reference data…" In this study, all underestimate burn area when compared to ANOTHER satellite product. Ultimately, we need to have better verification of the burn areas per pixel detection with ground truth. We have tried to address these issues in the limitations and uncertainty section of the manuscript. We have now included in the new Table 8 the total area burned for global regions similar to those used by van der Werf (2006; 2010).

Reference now added to the text: Roy, D.P. and L. Boschetti (2009) Southern Africa Validation of the MODIS, L3JRC, and GlobCarbon Burned-Area Products. IEEE Transactions on Geoscience and Remote Sensing, 47(4), 1032-1044.

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?

The residue/crop ratio can differ for various crops, and these differences may be due to climate, fertilizer use, or whether the crop is a "high-yielding" variety (Yevich and Logan, 2003). We expect that the North America fuel load of 500 g/m2 is towards the lower limit and would result in a conservative global crop residue fuel load. A sentence has been added to the text to address this.

Yevich, R., J.A., Logan (2003) An Assessment of biofuel use and burning of agricultural waste in the developing world. *Global Biogeochemical Cycles*, 17(4), DOI: 10.1029/2002GB001952.

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.

The sugar cane fuel loading was only applied to a small section of southern Brazil in Sao Paulo State where it is a dominant crop. The reviewer makes a good point though, in that this loading would not be appropriate for some areas of Brazil where other crops dominate. We have added text to this section to highlight the fact that the region to which the sugar cane assumption was applied is a limited one.

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.

This is a great comment by the reviewer. Since each pixel assigned a fire can include multiple land cover types, we assume that the fire is equally distributed between the amount of forest and the amount of herbaceous land cover. This calculation is taking into account the amount of each in the grid cell. Therefore, the density of the forest (from the fuel loading) is then further scaled to the MODIS VCF coverage, which may be more specific and current than those used by Sitch et al. We also note that, despite this issue, the fuel loadings applied in FINNv1 are consistent with those provided in other studies (e.g., Akagi et al., 2010).

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?

We included this comparison since chemical transport modelers typically use NMHC and NMOC as the same inputs to models. The purpose of this comparison is to show that the inclusion of the new emission factors gives much higher emissions for inputs to models. The comparison of NMHC from both models is now included in this Table.

Figure 3. In the July panel the Europe/Asia NH emissions end abruptly at _ 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.

The authors greatly appreciate this comment. There was a mistake in the 2008 summer emission files for which have now been corrected. The figure has now been fixed and the resulting emission estimates throughout the paper corrected.

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.

The reviewer makes a good point here. This is definitely regional. Based on this comment, we do include average annual emissions of by region (as defined now in Figure 1, based on the regional analyses). Additionally, we have followed the reviewer's suggestion and added a new bar plot that shows the CO emissions (averaged for 2005-2009) by generic land cover type for the entire globe, for the southern hemisphere, and for the northern hemisphere. As shown in this figure, the tropical forests are the dominant source of CO emissions.

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

These have been added to Table 2.

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

Unfortunately, the version of EDGAR used here only has that reference. We have included the web site from where one can download EDGAR data: http://edgar.jrc.ec.europa.eu/index.php.

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