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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of wildfire NO_x emissions using MODIS fire radiative power and OMI tropospheric NO_2 columns" *by* A. K. Mebust et al.

Anonymous Referee #1

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The authors have used OMI NO2 retrievals and MODIS FRP to determine the NO2 emission ratios from fires in SW USA. This work builds on Ichoku and Kaufman (2005) and Vermote et al. (2009) and is not entirely novel, but interesting enough to warrant publication in ACP, especially since it is the first to focus on NO2. In addition, it is very well written and the authors have carefully addressed most uncertainties, even though some are difficult to quantify with the available information.

If it wasn't for one crucial issue I would have recommended "accept as is". The key problem I see is that the authors have taken emission estimates based on FRP for granted, while in fact it can be argued that this approach is still in a research phase.

Lab set-ups have shown the large potential of this approach and derived a constant value when translating FRP to burned biomass, but whether this holds when using satellite retrieved FRP is not certain.

Issues with this approach have been acknowledged in earlier work. For example, Vermote et al. (2009) found a mismatch between FRP-derived emission factors and those measured in the field (in the opposite direction as found in this paper, possibly indicating that FRP values are too low), but have imho not given a satisfactory reason for this and state this is subject to further research. On continental scales, total emissions based on FRP are about a factor 3 lower than more traditional approaches using burned area, see for example Ellicott et al. (2009). These traditional approaches, for example the global fire emissions database (http://www.globalfiredata.org), suffer from underestimations due to the inability to detect small fires, and inverse modeling studies have identified that these represent a lower bound for emissions (see for example Kopacz et al., 2010, ACP). For this reason it is unlikely that traditional approaches are simply too high, but more likely that the FRP approach yields estimates that are biased low.

So this provides another source of uncertainty which, if I have understood the approach correctly, would point towards even lower emission ratios then found in this paper: NO2 from satellite = FRP x Er (neglecting chemistry). You solve for Er which yields estimates lower than measured in the field. If the satellite FRP retrievals are biased low as suggested above, then one would need even lower Er's to compensate for the FRP which is in reality maybe larger than seen from the satellite. With regard to emission factors or Er's, these are uncertain but it is unlikely that they are up to a factor $\sim$9 off (a factor 3 as found in this study times the bias when comparing global biomass burning estimates from FRP and traditional methods which yields another factor 3).

To increase the impact of this paper it would be good if the authors assess whether FRP derived estimates indeed introduce another bias. This is not necessarily a lot of work as MODIS burned area can be downloaded easily and fuel consumption estimates

ACPD

11, C1088–C1090, 2011

can be found in the literature. If indeed the satellite derived FRP is biased low then some rethinking needs to be done because the suggested reasons for the bias may not suffice. In addition, it would be good if at least one of the three potential causes of the bias described in the summary could be eliminated to prevent too many loose ends, for example by expanding to a larger region to investigate whether fires in California are indeed different from fires in other regions (option b in the conclusiosn)

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5351, 2011.