Atmos. Chem. Phys. Discuss., 12, C1597–C1600, 2012
www.atmos-chem-phys-discuss.net/12/C1597/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Determinants and predictability of global wildfire emissions" *by* W. Knorr et al.

Anonymous Referee #2

Received and published: 17 April 2012

The authors have assessed how important uncertainties in the parameters used to estimate fire emissions are for emissions assessment. They have focused on three burned area products, a fuel model with varying degrees of combustion factors, and made some assumption on the combustion efficiency. With this, they aim to understand "how well can we currently model chemical emissions from wildïňĄres based on ïňĄre observations, and what is the consequence of these ïňĄndings for our ability to simulate emissions under climate change".

Fire emissions estimates are critical for atmospheric studies but unfortunately these estimates vary to a large degree. The community could therefore gain from a study as presented here. However, I doubt the paper can fill the gap in knowledge. This is mainly because there is no systematic assessment of uncertainties in the different

‘Seiler and Crutzen’ parameters. Instead, the authors have mostly used the range in the data available (burned area) or in the assumptions made in various papers that have appeared on this subject over the last decade.

The presented range and relative importance of the different parameters is not necessarily realistic which makes it difficult to rank them, and unfortunately also means that the outcomes are not robust. This is most obvious for the assumptions on the burned area. The authors have not assessed what the best dataset is (which may well be beyond the scope), but have used the range in, and mean of, the three different products to get an assumed uncertainty. However, is this uncertainty realistic? I doubt it. Most studies that have investigated burned area by comparing global coarse resolution estimates with regional Landsat-derived estimates found that the MODIS products underestimate burned area [Roy and Boschetti, 2009; Giglio et al., 2010] to some degree and that the L3JRC product should probably not be used at all, especially not in the boreal and temperate regions [Chang and Song, 2009; Roy and Boschetti, 2009]. The real range and uncertainty is thus likely different from what is reported in the current paper. In addition, the claim that “The assumption here is that diïňĂerences between global burned area products are similar in magnitude to the uncertainty in computed burned area of some prognostic modelling framework used to study ïňĄres under future or past climates” is not substantiated.

The uncertainty assessment in the fuel load calculations are also doubtful in my mind. Ideally, in a study that “explores the sensitivity of global chemical emissions from wildïňĄres to various uncertain model inputs and parameterisations” I would hope to see an intercomparison between modeled fuel loads and measured ones in the field, but this was only done for one moist savanna site. As a side note, I would argue that models build to model ‘natural vegetation’ will overestimate fuel loads in savanna sites because they do not factor in grazing or fuelwood collection.

Regarding the combustion factor for herbaceous litter and live grass, the authors refer to [Shea et al., 1996] and set the combustion factor to 100%. In [Shea et al., 1996], however, the combustion factor ranges between 64 and 92%. The approach to model the combustion factor of wood is interesting, but I was left wondering what the basis is for the parameterization. The authors refer to [Shea et al., 1996] focusing on African savannas but fires may behave different in other savanna types or regions, and whether a savanna parameterization holds for forests is not discussed at all. This is acknowledged to some degree ("While two to four samples are certainly too few to derive a robust margin of error, this study nevertheless attempts to derive a ïňĄrst estimate of the contrasting contributors to uncertainties of wildïňĄre chemical emissions"), but it is not difficult to argue that these kind of assumptions makes it impossible to meet the objectives of the paper.

Following up on the point made above, in the discussion the authors argue that "neglecting the sensitivity of simulated emissions on ïňĄne and herbaceous fuel combustion factors is justiïňĄed". Having seen savanna fires in the field, this is surprising as litter and live grass represent probably 90% of the fuel loading in savannas, and savannas are thought to be responsible for about 50% of global carbon emissions from fires. The 1952 gC/m2 fuel load for savannas and the small contribution of litter and live grass to this mentioned in p4259 seems to reconcile this, but it cannot be the fuel loading of savannas which is an order of magnitude lower (unless one includes trees in the fuel loadings, but they do not burn), see [Shea et al., 1996].

In summary, I feel the work presented is important, but before it can be carried out the real uncertainty in the parameters used to compute emissions has to be assessed. I know the burned area community is working hard to do exactly this, and hopefully it will be done for fuel loads, combustion factors and emission factors as well. Until those communities have a better handle on the uncertainties, a study as presented here where lots of non-validated assumptions on the uncertainties are made, has no clear added benefit in my opinion and certainly cannot rank the uncertainties. In addition, the second objective "what is the consequence of these ïňĄndings for our ability to simulate emissions under climate change" is not discussed at all. Finally, the authors

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refer to using atmospheric constraints to possibly improve this work in the future, but seem unaware that this is an active area of research [Kopacz et al., 2010] that could have been used in this paper, for example to dismiss the L3JRC burned area in boreal and temperate regions.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 4243, 2012.