Atmos. Chem. Phys. Discuss., 13, C68–C75, 2013
www.atmos-chem-phys-discuss.net/13/C68/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Combustion efficiency and emission factors for US wildfires" *by* S. P. Urbanski

Anonymous Referee #1

Received and published: 4 February 2013

Quite often variable phenomena are assigned average values in models. The average value may not occur for single events, but assigning a large group of events the average value should minimize error in scaled up applications. Wildfires are an important influence on the atmosphere for which average values are poorly characterized. In this work the author measures MCE (which can be used a predictor of emissions) for three wildfires. The number of samples per fire was very high, the data appear to be of excellent quality, and fires were often sampled on multiple days so the fire-average values are likely to be highly accurate within the limitations of the sampling strategy – chiefly that the fires burned for several months and also at night or produced some unlofted emissions. The paper is well-written and should be published after addressing a few general comments and few specific issues.

General comments:

1) Most importantly, the author should determine the scope of the paper, clarify that scope specifically at the outset, and then maintain that scope consistently throughout the paper. Normally it is safest to limit the scope to what was actually studied; in this case the emissions of three gases from three wildfires at high elevation in the mountains of Montana. If additional conclusions about some, specified subset of CONUS wildfires can be supported (which is highly likely), then that will also be very useful. In the western US alone, wildfires burn a variety of fuels including sagebrush, grass, pinion-pine/juniper, ponderosa pine, etc and overall western wildfires fires likely burn over a wide range of MCE. Here based on three fires there is a narrow range of MCE, likely resulting from the small sample size. Most statements in the paper are well-qualified, but a few may be a bit too general. Some of the other general issues noted next are interactive with deciding on the scope.

2) Northern California wildfires were sampled during the ARCTAS campaign in 2008 and that should be recognized in some way. In particular, Hornbrook et al., (2011) provide MCE for 7 wildfires (0.91, 0.90, 0.915, 0.90, 0.88, 0.92, and 0.95 in order of date). Additional emissions info is likely recoverable from that paper, other companion papers, or the ARCTAS archive. For instance, plumes classified as "CARB-BB" in Hecobian et al., (2011) are said to be from California wildfires. Papers with emissions info are referenced within these papers, e.g. Singh et al., (2010). These data could potentially be integrated into an expanded analysis that addresses a broader range of wildfire types. Or if a more specific scope that excludes these fires is decided on, the main features of these fires just over the boundary of what the author addresses should be noted.

References

Hecobian, A., Liu, Z., Hennigan, C. J., Huey, L. G., Jimenez, J. L., Cubison, M. J., Vay, S., Diskin, G. S., Sachse, G. W., Wisthaler, A., Mikoviny, T., Weinheimer, A. J., Liao, J., Knapp, D. J., Wennberg, P. O., Kürten, A., Crounse, J. D., Clair, J. St., Wang, Y., and Weber, R. J.: Comparison of chemical characteristics of 495 biomass burning plumes intercepted by the NASA DC-8 aircraft during the ARCTAS/CARB-2008 field campaign, Atmos. Chem. Phys., 11, 13325-13337, doi:10.5194/acp-11-13325-2011, 2011.

Hornbrook, R. S., Blake, D. R., Diskin, G. S., Fried, A., Fuelberg, H. E., Meinardi, S., Mikoviny, T., Richter, D., Sachse, G. W., Vay, S. A., Walega, J., Weibring, P., Weinheimer, A. J., Wiedinmyer, C., Wisthaler, A., Hills, A., Riemer, D. D., and Apel, E. C.: Observations of nonmethane organic compounds during ARCTAS − Part 1: Biomass burning emissions and plume enhancements, Atmos. Chem. Phys., 11, 11103-11130, doi:10.5194/acp-11-11103-2011, 2011.

3) Once a scope of the paper is determined, the significance of the subset of wildfires that the author elects to discuss should be estimated. For example, high elevation mountain wildfires similar to the ones sampled by the author can sometimes be a major part of area burned in the western US with the 1988 Yellowstone fires coming to mind as an example. On the other hand, a list of the largest wildfires in the US at (http://www.nifc.gov/fireInfo/fireInfo_stats_lgFires.html) implies that grass fires account for most or many of the largest wildfires in CONUS. It should also be made clear what resources are available and on what time scale (operational or how long after the fact) to identify whether a fire is wild or prescribed. The author is well-qualified to summarize this sort of thing and it would be a good addition to the paper for the interested reader.

4) It is not clear to me how to classify the ecosystem for the fires the author sampled, which interacts with both the scope and how emissions for additional species could be estimated. The wildfires (WF) sampled in this work consumed forest fuels at elevations of 1000-2650 m. The alpine tree line in Montana is at 2400-2700 m. Many ecologists classify high elevation forests in the Appalachians and Rocky Mountains as boreal ecosystems, although classification schemes vary depending on the goals of the scheme. From a species overlap (presence of picea often an indicator species for boreal forest), cold-climate leading to slow decomposition and accumulation of heavy

fuels, and long fire-return intervals a “boreal” classification seems reasonable for the fires in this work. Further, the average MCE the author measured (0.883) is almost identical to the MCE recommended for boreal forest fires (0.882) in the A11 emission factor (EF) review used by the author. This suggests that one simple, reasonable way to estimate the EF of unmeasured species for the author’s fires could be to extract EF directly from the A11 boreal forest fire recommendations. This might produce as good or better recommendations than an EF vs MCE equation. A related minor issue is that any comparison of “temperate” WF to temperate prescribed fires (PF) should ideally involve comparing WF and PF that occurred at the same latitude, elevation, and ecosystem. So for example the B11 southeastern prescribed fires should ideally be compared to wildfires at 35 degree latitude and sea level. Lastly on this topic, it could be useful to support the idea that ecosystem classification should consider altitude as well as latitude.

5) More on estimating EF for unmeasured species. This can only be a rough estimate by any method, but it’s a valuable addition that should be included by some method. However, I was not sure the estimation method used was optimal or that the likely error was clear. The author uses EF vs MCE equations from the B11 reference to predict EF not measured in his study. These two studies can be directly compared for CH4. The authors EFCH4 vs MCE slope coefficient is -54 while the B11 study had a slope coefficient of -96(10) for CH4. A variety of papers displaying this type of regression data for CH4 are easily found. The Yokelson et al., (1996) lab study shows air, tower, and lab experiments all yielding slopes near -52. McMeeking et al. (2009) give a slope of -37. The B10 lab study is slope is -49. The Akagi et al., (2013) field measurement slope is -65. The Urbanski et al., (2009) field data gives slopes in the -30 to -70 range. So it’s not clear to me that it is easy to choose a-priori which study to base predictions on – or if the predictions of several studies are useful, which would likely imply a higher uncertainty. The ARCTAS study may provide some insight into this. Two other factors affect the uncertainty of the current predictions based on the B11 equations. (1) Other than CH4 and CH3OH the B11 equations were not that highly correlated. (2) Several studies

show low correlation for EF vs MCE measured in burning duff or dead, down woody debris (here-in "heavy fuels") (Bertschi et al., 2003; B11, Akagi et al., 2013). Some of these additional sources of error are acknowledged on page 45 and elsewhere, but don't seem to be formally incorporated into an error estimate. This overly lengthy comment isn't meant to argue against the author's estimates, but point out the high uncertainty, which should be clear. In light of that, perhaps some conclusions should be tempered or qualified. Given the qualitative nature of these estimates a complex prediction step may be un-needed if using A11 boreal EF, but the complex approach may yield additional insight and be worth retaining.

Specific comments:

P35, L1: trivial, but if there is another adjective besides "heavy" to describe an amount of fuel then "heavy" can be reserved to describe a type of fuel.

P35, L24: I get the author's point, but "failure" seems a bit strong when discussing wildfires in general since the study so far deals with a subset of wildfires.

P37, L1: Clarify that these estimates of WF contributions are before adjusting EFPM based on the author's findings in this work?

P40, L8: Should the year be 2011?

P42, L9: add "of" after "measurement"

P46, L28: add uncertainties?

Section 3.2: Two things might be clarified here: (1) Does the fuel based analysis suggest that WF without heavy fuels would have EF similar to temperate PF? (2) Possibly cite a database that gives the amount of heavy fuels for the western US?

P52, L10: In light of the limitations of this study and the ARCTAS data, I recommend inserting "some" before "western wildfires." Caveats do appear below on same page, but the implications are too general here. This is an example of how maintaining a

consistent scope for the paper will clarify its message.

P52, L25: text is "the failure to use wildfire appropriate EFPM2.5 has significant implications for the forecasting and management of regional air quality. The contribution of wildfires to NAAQS PM2.5 and Regional Haze may be underestimated by air regulatory agencies. This is especially true considering . . ."

Again "failure" and "especially true" seem a bit strong, because when more WF of other types are measured it could turn out that the current values are not the biggest source of error at least for some fires. Interesting forecasting-related questions that the author could potentially summarize at this point include: how does uncertainty in WF EFPM compare to uncertainty in forecasting WF size, or the possible error from using average values for a specific event, etc?

P53, L19: The lab finding that "MCE tend to increase with decreasing fuel moisture" seems inconsistent with author's analysis and with the findings of others (next comment).

P54, L1: Akagi et al., (2011) discuss literature fuel consumption data from Africa that suggest that more of the large-diameter fuels burn late in the dry season. I think this assumption is also built into the latest version of GFED. Since heavy fuels tend to burn with lower MCE then the drying of the heavy fuels should lower MCE as the author argues.

P56, L12: Heavy fuel loading, moisture (and geometry) is widely accepted as a driver of emissions variability; but it is one of many factors, which the author does seem to clarify on P57, L28.

Section 3.4 summary comment: This section contains a lot of good information, points out that heavy fuels impact emissions, and Figure 5 relates PF MCE to heavy fuel fraction. It's possible that the purpose or applications of the section could be clarified a bit more in a focused way. There are limitations to predicting emissions based on the heavy fuel fraction since the “non-heavy” understory and canopy fuels could impact WF emissions differently. Also duff and logs are lumped together in the heavy fuel category when they may contribute differently to emissions under some circumstances (Bertschi et al., 2003, Fig 5, B11 Fig 5). In general the emissions from these “heavy fuels seem less tightly correlated with MCE suggesting a high degree of uncertainty in predictions based on this approach as the above papers note. Overall this section is interesting, but speculative and based on limited data at the moment. It’s not clear if the author is proposing an application for the results of this section. It’s interesting that in this work the author assumes that WF are essentially low MCE PF for purposes of predicting emissions, but also stresses throughout the text how different WF are from PF in terms of more heavy fuel, different weather conditions, etc. Perhaps the overall message is that PF are a good proxy for WF, but only with appropriate caution?

Table 3: title needs a little work.

Figure 5: If available some x-y error bars would be interesting.

Main suggestions summarized:

1. Maintain one consistent level of specificity on the scope through-out the text. As it is more general statements are common early on, followed by caveats later.

2. Add an estimate to the text of what percent of CONUS wildfires are being discussed in the paper (it’s likely significant) and make the title more specific (e.g. at least add “projected” before “emission factors” and “some western” before “US”, which may be better as CONUS).

3. Mention briefly any resources or common sense guidelines available to interested modelers that would allow them to distinguish wild and prescribed fires on a routine operational basis. E.g. I suspect there are very few if any PF in western US during summer.

4. Use the A11 boreal wildfire EF as one or the only method to estimate the missing

EF for the authors WF in Table 3.

5. If EF vs MCE based predictions are retained use (or discuss the impact of using) a larger selection of the available data: e.g. ARCTAS, Urbanski et al., (2009), Akagi et al., (2013), etc to get a better feel for the uncertainty in the predictions.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 33, 2013.