Atmos. Chem. Phys. Discuss., 11, C14372–C14375, 2012
www.atmos-chem-phys-discuss.net/11/C14372/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropical biomass burning smoke plume size, shape, reflectance, and age based on 2001–2009 MISR imagery of Borneo" *by* C. S. Zender et al.

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Received and published: 17 January 2012

In this paper the Zender et al. team used a MISR plume mapping tool to study the dimensions and retrieved properties of individual smoke plumes in Borneo. I think the actual measurement of plume physical dimensions is quite good. The MISR team has put a lot of effort into that tool. If the paper stuck to this, I would have few issues, except perhaps the issue of proper descriptions of sampling and sampling bias. In this regard, the authors briefly acknowledge but by no means emphasize the implications of sampling bias inherent in the measurement. This occurs at two scale. First, the 1030AM overpass of MISR implicitly favors morning fires, which are the vast minority.

We have known for some time that there is a strong diurnal cycle in burning. For this particular region, I cite my own ACPD paper where we are finishing the review process. Jukka Mietinens papers clearly show that in El Nino years, it is peat fires which are burning for days at a time which dominate emissions. Also, (although I don't hold these authors beholden to this), Ed Hyer has a paper in press which looks at MTSAT geo data and he found mammoth differences based on fuel type. So for this paper, we expect "long plumes" in the morning because it is peat fires, burning overnight, which likely dominate the sample. If this is the case, then I expect their sampling to be el nino dominated. So in regards to this paper, these caveats need to be fully discussed. What fraction of the 300 fires were from El nino years? How do plumes differ based on fuel source (was it peat, ag, or other deforestation?). Where on the island where they measured? How does this jive with the 1030 overpass time? And perhaps most importantly, how should modelers use this information? How does this fully relate to Tosca's plume work? These are all fixes which can be done in a month.

Where I think the paper is on VERY shaky ground is on the optical retrievals and the implications for smoke particle evolution. It will take considerable effort to fix this part, and I am not even sure it is even doable with the data they have access to. At NRL we specialize in error matrices for satellite products, and optical retrievals on plume structures is a very hard gig. The short of it is that first and foremost, retrievals fail for very high AOTs. Essentially eventually scattering becomes semi-infinite and absorption dominates on AOT. So, people often use retrievals on the plume edge (such as here). But, for this intermediate range where even retrievals are possible, multiple scattering dominates and hence errors grow rapidly. For high AOTs, MISR underestimates by a large fraction, while MODIS overestimates. Implicit in this is that the optical models are not correct. For MISR in particular, it was my understanding that there was not much faith in the wo product by the developer. They may feel more comfortable in a relative sense, but I personally have my doubts. Looking at radiance is better (which the current paper does), but they need do the RT to interpret what changes mean. I do not think they could do an adequate job on this in a month unless they brought it folks

from the MISR team. Even them I am a bit dubious it could be salvaged in a month.

The authors need to be a bit careful about the implications of their findings and I think they misunderstand the nature of smoke particle evolution. For example, they state that wo becomes 3% greater downwind. So even if this is true, the implication of wo going from 96%-99% is that secondary mass formation increases by a factor of three (even generously accounting generously for coagulative increase in the mass scattering efficiency)! The Carnegie Melon guys get this in the lab for very fresh smoke, but this has not been directly observed in the environment. I for one found 40% increase from the top of the plume to downwind. So this would be quite extraordinary. That does not at all mean that it is not happening, but evidence is tenuous. It could be increases in hygroscopicity as well correlated with inorganic like so4 (which has been pretty well shown now in the lab by Gavin McMeeking's-CSU work), and we think that SO2-SO4 conversion is very rapid the peat plumes. Further, there findings on AOT , wo and radiance are incompatible with a 15-25% increase in AOT in along plume direction. Based on their findings, it should be much higher. Again, because AOTs cannot be retrieved at high values, what they are likely looking at is how diffuse the aerosol boundary is at the edges. This sampling bias is exactly the same as why you see peaks in AOT off the coast of California away from the source. But, again, I would suggest them working closely with the MISR team on these issues.

The paper in many locations often places clauses and adjectives on the nature of fire and smoke evolution which sort of take the field for granted. In many cases, they cite my 2005 papers incorrectly. For example, I am cited as saying that there are only three fuel types (page 30992-ln 10), then they list in the paper 4. What I did was point out that this is where there are measurements of physical properties, but I did not go comprehensively into fuel types. I bring up (section 8) other fuels like peat, but requires future study (which it does, hence 7SEAS and SEAC4RS). I am also quoted as saying AOTs are higher in Brazil than Africa (even here inappropriately cited to part 2, when really should be part 3). But regardless, I never said that. I listed a mean AOT

from sun photometer in Table 3, but in Africa Mongu is outside of the main plume (I assume this is where they got it). On page 31009, ln 15-22, they say that smoldering produces a higher fraction of hygroscopic organic to hydrophobic black carbon. Even here one must be careful. First, smoldering combustion produces no black carbon at all, so what back carbon is there is from flaming on the periphery. Second, particles tend to be internally mixed. Even from framing combustion where one can get pretty much BC dominated aerosol particles, Martins showed that these can get coated with secondary organics pretty quickly. Finally, it is implied here that primary organics are hygroscopic. I guess they are if you think of f(80%) as 15% as hygroscopic. There are too many cases of misunderstandings in the paper to list here, and I want to avoid ranting. Regardless, the chemistry is relatively complex and I think a re-review of the currently literature would be helpful to the authors.

The motivation of the paper is even a bit questionable. In the previous modeling paper in ACP paper by Tosca, I as a reviewer pointed out that the whole climate simulation was unphysical, but was worth publishing as a baseline. At the MISR user's conference a month ago, I met Tosca and we agreed on this point. Indeed, adding el nino emissions to non el nino years ignores the covariences in the large scale meteorology of the region. Plus, a 10% decrease in precip is not what I would call significant in this case (I am not sure we could even measure precip to 10% in the region). Now, this model result is presented by these same authors as pretty much fact in the abstract and throughout the paper. This seems to be a repeated offense by modelers in general. So, this leads me in the future to not cut modelers much slack as I have in the past. It would help if discussions are placed in proper context.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 30989, 2011.