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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Direct measurements of the effect of biomass burning over the Amazon on the atmospheric temperature profile" *by* A. Davidi et al.

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MAJOR COMMENTS:

I apologize, but I really did not like this paper. I hope my reasons are clear, and that my criticisms at least prove constructive.

1. We have known for perhaps half a century that air pollution and atmospheric stability are linked. Stable conditions confine surface pollutants, while conditions of weak stability ventilate the boundary layer and allow the pollution to be dispersed

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in the stronger winds of the free atmosphere. Hence the null hypothesis is that greater stability favors greater AOD. There is also a humidity effect, as shallow boundary layers forming in more stable conditions tend to be more humid, which also affects AOD. The authors need to establish why the conventional train of thought is not relevant in their study. Indeed, the article was remarkable in that it did not seem to occur to the authors that meteorological changes might explain the correlations they are seeing.

2. We understand radiative transfer, and the dry convective (or subcloud) boundary layer. We also know something about the radiative properties of smoke. The plausibility of the authors ideas would be easy to establish if this knowledge were simply used to interpret their data. For reasons pointed out in the minor points below I do not find the data compelling.

3. Too much of the argumentation relies on highly speculative (and I believe) unsubstantiated claims about the dependence of the cloud on the aerosol. I suspect I might not have understood what the authors were arguing because the claims ran so counter to conventional understanding, hence significant effort is required to make these arguments clear and substantial.

I believe the ideas would be best served if the authors used this data as a launching point to look more deeply into the issues. In addition to making better use of auxiliary data, the authors need to explain why their ideas are more compelling than the null hypothesis (which they did not even consider) before this work merits further consideration.

MINOR COMMENTS:

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L1-6: I am of the opinion that an abstract should say what was done, and what was learned, motivation for the work belongs in the introduction.

L24: Strictly speaking I would not call these feedbacks as none of the processes mention have to do with a modification of the input. I realize feedback is sometime used to mean that certain effects are less or more than expected, but I would simply say something along the lines of "engendering a series of processes" ...

L26: Are these really the right references to establish that the aerosol interacts with radiation?

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L10. If the aerosol heats the boundary layer thereby reducing its relative humidity (L5) then it stands to reason that surface evaporation would increase. The role of stability in modifying surface fluxes (flux/gradient relationships) is probably secondary. Moreover any reduction of cloudiness will significantly increase the radiation at the surface, and this, in my estimation would stabilize the whole system. All of which to point out that this chain of reasoning, although certainly plausible, is speculative.

L13: Certainly there is a large body of evidence supporting the idea that the aerosol affects cloud micro-structure and that this changes cloud radiative properties, particularly for clouds of moderate optical thickness. But what happens next is far from being resolved. It seems warranted to make these distinctions.

End of Page: I think the relevant point to establish is not the quality of the AIRS temperature retrievals in general, but rather their quality in the boundary layer. Certainly I know for water vapor the retrievals are not credible here. For temperature I don't know how well they do. The authors should convince the reader that they are useful.

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There are twice a day soundings at SBAT Alta Floresta (Aero) well within the study region. Is there some reason that this data was not used? Later you even use the sound-

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ing data at Manus to translate pressure into height (why not the hydrostatic equation?); I don't understand why you don't use this data for temperature and humidity (see major objections)

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I think I understand what the authors are doing, but to be sure it would help if their terminology was made explicit. Angle brackets are spatial (over the area) and temporal (over a day) averages? How do you get the temporal signal? The correlations then treat all the 1x1 deg boxes independently? Is this warranted? What is the auto-correlation lengthscale of the measurement. You see what exactly you are plotting is not clear to me.

Line 24: Although I like Rogers and Yau as much as anyone, my guess is that you don't need a reference for the dry adiabatic lapse rate.

Line 25: Outside of the surface layer an adiabatically unstable profile appear srather unsuual as convection is extremely efficient at restoring fluids to a state of neutral (in this case) dry stability. This suggests that the AIRS data has problems, and perhaps is seeing the surface temperatures rather than the air temperature at 1000 hPa

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Line 6: Although this may have been shown by Koren et al., 2008, I don't believe anyone seriously believes that the aerosol is the primary control on cloudiness. My guess is that this was not what the authors wanted to say, so please try to clarify.

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Did it not occur to the authors that meteorology might play a role?

On partly cloudy days, wouldn't the multiple scattering of the clouds increase the chance of absorption? I guess this depends on how deep the clouds are in the smoke layer.

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I find the arguments about the microphysical pathways thoroughly unconvincing. Especially so because the supporting studies are also correlative. We all know that the aerosol is a great tracer of airmass history, and that clear (smoke free) days likely reflect different meteorological conditions (although if hot-spots/fires have very short timescales I am willing to be convinced otherwise.. but I must be convinced.)

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 12007, 2009.

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Interactive Comment