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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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We thank the reviewer for his insightful comments, which helped make the paper clearer.

Reply to Major Comments

1. Page 12009: Discussion of AIRS: AIRS is of central importance to the paper, yet little is discussed about it. The authors refer to published literature for evaluations, but I'd like to see a summary of AIRS performance in the region of interest. For example, what is meant by "biases and variations in the temperature profiles

do not correlate significantly with cloud fraction"? (I am referring to quantitative statistical measures).

Answer: We added to this paragraph a discussion about the AIRS temperature retrievals performance in the relevant altitudes, as well as the effect of clouds on the temperature retrievals. We also changed this discussion to be more quantitative. The paragraph (page 12009, line 22 to page 12010, line 4) now reads:

"The atmospheric temperature profile is measured with the Atmospheric Infra-Red Sounder (AIRS), on Aqua (Aumann et al., 2003). AIRS is a high spectral resolution infrared sounder, which is designed to provide atmospheric temperature and water vapor profiles. AIRS temperature retrieval has been validated in various campaigns that include different geophysical conditions: polar, non-polar, day, night, land, and ocean (Olsen et al., 2007, and references therein). In particular, the AIRS temperature profile over the Amazon basin, during September-October, was shown to have an RMS (root mean square) of about 1-2 K at pressure levels above 900 hPa and an RMS of 3-4 K below 900 hPa (de Souza et al., 2005). Another campaign in Natal/Brazil showed that the temperature retrieval RMS was about 1K (de Souza et al., 2006). Clouds were shown to have only a minor effect on both the AIRS temperature profile (Susskind et al., 2006; Tobin et al., 2006) and the surface air temperature (Gao et al., 2008). As far as we know, there has been no publication on the effect of smoke - or aerosols in general - on the temperature retrieval. However, the works done by de Souza et al. (2005 and 2006) in Brazil and Gao et al. (2008) in China may serve as an indication for the performance of AIRS temperature retrievals in hazy conditions. All the above mentioned validations were done with radiosondes; however one must keep in mind that the AIRS footprint is 45 x 45 km at nadir, while radiosondes measure only a point's profile, therefore the above RMS values are upper bounds for the true errors (Tobin et al., 2006). In addition, several studies showed that assimilation of AIRS temperature data can improve forecasting (Reale et al., 2008; Freitas et al., 2007); these may further support the validity of the retrieval."

2. Page 12010, Line 23: “expected to be smooth in our region of interest”: Please explain why (especially since temperature spikes are filtered out).
   Answer: We thank the reviewer for drawing our attention to this. We meant that the AIRS temperature retrieval occasionally produce unphysical retrievals. Pixels which differ from their neighboring pixels by more than 20 ℃ were filtered out. We changed the sentence to (page 12010, lines 22-25):
   “Occasionally AIRS retrievals produce unphysical spikes in the temperature fields of a particular pressure level. We filtered out pixels in which the temperature difference between the pixel in question and its neighbors exceeded 20 ℃. This eliminated about 0.5% of the data.”
   Note that we corrected a mistake we made in the original manuscript - the filtered out pixels were only 0.5% of data (and not 3%).

3. Page 12011: Definition and interpretation of DT. The authors should provide arguments on why the correlation of DT versus AOD defines a functional relationship, and why the similarity between <T>daily–AOD and DT–AOD suggest variances from meteorology is small (and not vice versa). Since the functional relationship was defined as “DT versus AOD”, why did you use T vs. AOD in the end? (Even if the two relationships are similar, it seems better to stay with the former).
   Answer: Indeed, this was not clear in the manuscript. We made this clearer using more exact definitions. We changed the paragraph (page 12011, lines 4-16) to:
   “Since we want to focus on the aerosol regional effect, we first examine the variance in the temperature (T) due to daily meteorological changes, using the following scheme. For a specific day and pressure level, the spatial mean temperature (denoted $< T >_{area}$) was calculated. Then, this mean was subtracted from each temperature in the corresponding day and pressure level. Repeating this procedure for all days and pressure levels results with a ΔT, which is mathematically defined as:
   $\Delta T\,(loaction, day, press.) \equiv T\,(location, day, press.) - < T >_{area}\,(day, press.)$

   Plotting ΔT versus AOD will show a functional relationship between temperature and aerosols, even if there are day-to-day variations in the regional temperature. Figure 2 shows both ΔT and T versus AOD, for pressure level 1000 hPa. The close agreement between ΔT and T is an indication for the stability of the meteorology during the period analyzed in this work. We note that the other pressure levels (i.e., 925, 850, and 700 hPa) show similar agreement. Because no significance differences are observed, further analysis is done with the “absolute” temperature (T), which will be more visually instructive later on.”

4. Page 12013: I was very pleased to see a confirmation of aerosol layers being collocated with the temperature profile change; was this analysis repeated for more cases than those shown in Fig. 5?
   Answer: We looked at 16 images spanning the entire time period (August - September) which showed similar pattern, figure 5 (original manuscript) is a characteristic image of these images. We added the following sentence (page 12013, within line 16):
   “. . . about 3.3 km. These images are characteristic, chosen from 16 images spanning the 2 months period (August - September) analyzed in this work. Examination. . . ”

5. Page 12014, Line 7: “associated with increasing AOD are larger than expected”. Can you be a little more quantitative? What is “expected” and why?
   Answer: We thank the reviewer for drawing our attention to this unclear statement. The expected decrease in surface air temperature due to an increase in AOD is about 1-2 ℃, based on several radiative transfer modeling studies (Yu et al., 2002; Koren et al., 2004). We changed the sentence (page 12014 lines 6-8) to:
   “The cooling due to aerosol extinction of solar radiation in the surface layer is expected to be on the order of 1-2 ℃ as shown by Yu et al., (2002) and Koren et al., (2004) from radiative transfer modeling. Here the observational analysis shows a

cooling of ~4°C, about twice the theoretical values, suggesting that another factor must come into play."

6. Page 12014, Lines 10-15: This is an important and insightful discussion. I would like to see a little more detail (and quantitative arguments presented) in the analysis. Would AOD~0.3 correspond to some set of aerosol/cloud microphysical conditions that explains why the saturation is observed?
Answer: We thank the reviewer for the encouragement. The fact there is a transition in clouds properties at a certain AOD level was shown previously by both observation (Breon et al., 2002) and modeling (Wang, 2005). Jiang and Feingold (2006), another modeling study showed that this transition point occurs when both microphysical and radiative processes are included in the model, but not when the radiative processes are shut off. Koren et al. (2008) developed an analytical model that describes this transition point at AOD~0.3, and supported this model by observations over the Amazon. Another work of interest on this topic is Rosenfeld et al., (2008). A full discussion of this transition is outside the scope of this paper, and will be addressed in the future. We added the above discussion to the revised manuscript in the discussion section (page 12015, between lines 17 and 18):
"The fact there is a transition in clouds properties at a certain AOD level was shown previously by both observation (Breon et al., 2002) and modeling (Wang, 2005). Jiang and Feingold (2006), another modeling study showed that this transition point occurs when both microphysical and radiative processes are included in the model, but not when the radiative processes are shut off. Koren et al. (2008) developed an analytical model that describes this transition point at AOD ~0.25, and supported this model by observations over the Amazon. Another work of interest on this topic is Rosenfeld et al., (2008). A full discussion of this transition is outside the scope of this paper, and will be addressed in the future."

7. Page 12014, Lines 16-18: This discussion seems a bit too vague. Can you elab-

orate (with numbers and appropriate citations) on the "magnitudes are consistent with expectations".
Answer: We thank the reviewer for drawing our attention to this vague statement. The expected increase in temperature at 850 hPa due to an increase in AOD is about 1-2°C, based on several radiative transfer modeling studies (Yu et al., 2002; Koren et al., 2004). We changed the sentence (page 12014 lines 17-18) to:
"The magnitude of increase of 1-2°C is consistent from expectations formed from radiative transfer modeling (Yu et al., 2002; Koren et al., 2004) and there is a steady rise in temperature as AOD increases."

8. Page 12014, Line 25: "cloud cover to narrow range..." how narrow? Is the narrowing enough so that AOD signals dominate? (A scaling argument could show this).
Answer: When we divide the data into low and high cloud cover, making the division at cloud fraction =0.3, we lessen the cloud contribution. With restricted cloud cover ranges, the cooling associated with increased AOD is cut in about a half and the temperature decrease is a linear function of AOD with no saturation point. Repeating the same analysis for different cloud cover cutoff values (i.e. different from 0.3) gave similar results. Ideally we would like to further narrow the cloud cover ranges, but the sample sizes in the narrow ranges became too sparse for statistical analysis. We moved the discussion about this issue to the end of the first paragraph in the discussion section. This paragraph now reads (Note that this paragraph contains changes due to other comments, including comments from other referees):
"Figures 3 and 4 present a compelling association between increasing aerosol optical depth in the Amazon and measurable temperature changes within the lower atmosphere. While the altitudes exhibiting the temperature changes lie within the characteristic smoke layer, as observed by CALIPSO, the temperature

changes cannot be due solely to heating/cooling by the aerosol absorption and scattering. The cooling due to aerosol extinction of solar radiation in the surface layer is expected to be on the order of 1-2℃ as shown by Yu et al., (2002) and Koren et al., (2004) from radiative transfer modeling. Here the observational analysis shows a cooling of ~4℃, about twice the theoretical values, suggesting that another factor must come into play. By controlling for cloud cover (Fig. 4), thus lessening the cloud contribution, we see that the magnitude of the cooling near the surface is cut in about half. In addition, in the high cloud cover case, saturation appears at AOD~0.27 (Fig 4b), which agrees with the saturation of the cloud cover at AOD above ~0.27 (Fig. 4d), while in the low cloud case, the temperature decrease is a linear function of AOD with no saturation point. Repeating the same analysis for different cloud cover cutoff values (i.e., different from 0.3) gave similar results. Ideally we would like to further narrow the cloud cover ranges, but the sample sizes in the narrow ranges became too sparse for statistical analysis."

9. Page 12014, Line 27: Keeping cloud cover is insightful, but does not constrain LWP or COD to a similar degree. Because of this, you can have very different cloud microphysical states, and introduce larger uncertainty in the analysis. Although this is acknowledged by the authors in the conclusion section, would it be useful to repeat the calculation with COD (or LWP) bins instead? Combined with the "cloud cover-bin" analysis, one might be able to say a little more about the "direct" and "indirect" radiative effects on the temperature profile.
   Answer: We agree that constraining LWP or COD would be an interesting exercise, but we feel that it is outside the scope of this paper. Here we focus on the relationship between aerosol and temperature profiles. We introduce the discussion of cloud cover based on the strong results of previously published literature that found relationships between cloud cover and AOD in the Amazon, and because changes in cloud cover provide the primary mechanism for clouds to affect

lower atmosphere temperature.

10. Page 12015, Line 2: I would like to see a few more arguments presented on why AOD~0.3 really corresponds to a transition point. Is this consistent with known theory? (A scaling argument based on cloud droplet number concentration impacts on auto-conversion timescale and/or cloud optical depth could be useful).
    Answer: We replied to this comment by citing several modeling and observational studies (see reply to major comment #6), especially the Jiang and Feingold (2006) and Koren et al., (2008) papers.

Reply to Minor Comments

1. The authors should try to reference more of the published literature, especially in the introduction when discussing aerosol-cloud interactions and absorption (for example, Albrect, 1989).
   Answer: We added the following references in the introduction (page 12008, lines 21-24):
   "The first pathway follows aerosol-induced changes to the cloud condensation nuclei (CCN) and ice nuclei (IN) concentrations and distributions, thus changing the microphysical properties of the cloud and stimulating related processes (Twomey, 1977; Rosenfeld, 2000; Albrecht, 1998; see also a review by Lohmann and Feichter, 2005)."

2. Page 12009, Line 20: "Amazon basin due to the presence". The "due to" seems a bit too absolute. Perhaps "consistent with" or "can be explained by".
   Answer: We accept the suggestion. The sentence was changed to (page 12009, lines 19-21):
   "In this paper we directly measure the change in atmospheric temperature profile over the Amazon basin in the presence of absorbing aerosols emitted from biomass burning during the dry seasons (August and September) of 2005-2007."

3. Page 12009, Line 22-33: This is an important paragraph, but seems to be out of place; I suggest placing it in section 2.
Answer: We thank the reviewer for this comment, however since this paragraph was dramatically changed (see reply to major comment #1), we believe this paragraph should be in the introduction.

4. Page 12010, Line 14-15: replace "and unless otherwise specified, all AOD" with "unless specified, all AOD".
Answer: We thank the reviewer for this correction; it was implemented in the text.

5. Page 12010, Line 23: "we screen out outliers that show sharp changes in temperature relative..." For completeness, describe quantitatively how the sharp temperature gradient filter was applied.
Answer: We thank the reviewer for drawing our attention for this unclear statement. We refer to the occasional unphysical temperature retrieval from AIRS, by which we mean temperature "spikes" of more than 20℃ relative to the neighboring pixels. See reply to major comment #2 for a citation of the correction.

6. Page 12010, Line 26: "to maintain similar temperature variances": This is not clear to me. Do you mean that you ensure sample size is the same so you have variances that can be compared (in a statistical sense)?
Answer: Indeed, we kept the number of data points in each bin the same in all bins. The following sentence was added to clarify this issue (page 12012, between lines 26 and 27):
"This way any change in the variances (and hence in the standard deviation) could not be directly attributed to sample size."
We note that the standard deviation is indeed quite fixed for each pressure level.

7. Page 12011, Line 1: "standard error of the mean": Do you mean standard deviation?
Answer: The error bars are the standard error of the mean, at a confidence level

of 95% (i.e. it is equal to: $2 \cdot \sigma/\sqrt{n}$, where σ is the standard deviation and n is the number of data points in a bin). However, in the revised manuscript we have changed it so that the error bars will indicate the standard deviation (i.e. 66% of the data points in a bin fall within the error bars). We believe this change shows better the spreading of the data.

8. Page 12015, Line 12: "combination of both processes." Which processes?
Answer: We refer to the microphysical and radiative effects. The sentence was changed to:
". . . we see the combination of both the microphysical and radiative effects."

9. Page 12015, Line 15: "surpass moderate levels." Give indicative range.
Answer: We meant AOD values above ~0.3. However, due to the referees comments, this paragraph was rephrased and incorporated into the first paragraph in the discussion section (which is cited in the reply to major comment #8).

10. Page 12015, Line 18: Replace "affected similarly" with "affected similarly by the smoke".
Answer: We thank the reviewer for this correction, however we rephrased the entire paragraph so this correction is not needed (see reply to minor comment #11).

11. Page 12015, Lines 20-21: I had a hard time understanding the point of this sentence. Can it be further elaborated?
Answer: We thank the reviewer for this comment. We rephrased the entire paragraph (page 12015, lines18-21):
"The pressure level 925 hPa can be viewed as a transition altitude. Its specific response would be determined by the vertical distribution of the smoke, which we cannot know at the moment. On the other hand, the 700 hPa level is at the very top of the smoke layer (Fig. 5), where the smoke is concentrated very thinly.

Because the very top of the smoke layer is heated less, this pressure level is expected to be less affected by changes in the smoke loading."

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