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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 Prof. B. Stevens for his review, and especially for being open. Indeed, his remarks proved very helpful in making the paper clearer.

Reply to Major Comments

The most prominent comment was causality - do the aerosols affect meteorology, i.e. temperature profile, or vice-versa? We start by noting that apart from appreciating the Amazon as an important and fragile place, there are a few good reasons to study the effect of smoke on clouds and the atmospheric temperature profile over the Amazon:

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1. During the dry season the ITCZ moves north and the study area is under an anticyclonic flow and a subsidence zone with very little meteorology variance. It was studied and demonstrated in several studies (e.g. Nobre et al., 1998) and the variance in the spatial temperatures was examined in this study as well.
2. The fires during the Amazonian dry season are anthropogenic and their location depends on the farmers, the farm location and on the government enforcement. Therefore the source distribution does not depend on the regional meteorology. The smoke will be advected by the prevailing wind, which is mostly easterly in the east and central Amazon turning northerly in the western part of the basin when flow is blocked by the Andes.

Specifically, if the meteorology was affecting aerosols (the null hypothesis), then performing the same analysis - a scatter plot of the temperature versus AOD - with much less absorbing aerosols should give the same results as shown in the paper. We chose to analyze the same area, but during the months June - July (the beginning of the dry season). During these months the meteorology is stable and similar to the months August - September; however, since the biomass-burning starts only in August (Fig. R1), the aerosols are mostly biogenic, which absorb solar radiation much less (Schafer et al., 2008).

Figure R2 shows the scatter-plot of the AIRS temperature profile vs. AOD for the months June - July. There is no heating at 850 hPa (red curve), compared to the months August-September analyzed in the paper (see Fig. 3, original manuscript), in contrast with the null hypothesis. The cooling near the ground (1000 hPa, blue curve) is of the same order of magnitude as in August-September. We attribute the cooling near the surface to increase cloud fraction due to aerosol microphysical effects on clouds, as explained in the text and by the references cited therein. Apparently the pre-biomass burning aerosols are as effective in changing cloud microphysics as are the smoke particles from later in the season, even if they are less light absorbing.

Again, we cannot completely rule out the possibility that the cooling near the ground in June-July doesn't have meteorological component. Increased AOD could be due to more stable meteorological conditions that would favor the formation and concentration of particles. Using the tools exploited in this work, it is hard to unravel causality in the temperature trend near the surface. However, there is strong evidence that the heating at 850 hPa is primarily due to aerosol absorption.

Freitas et al. (2005) claimed from modeling results that: "...the presence of the smoke in the atmosphere results in a strong radiative forcing as these particles are very efficient solar radiation scatterers and absorbers. The atmosphere responds to this forcing through a cooling of the low levels and a heating of the upper levels of the PBL [planetary boundary layer]. The net effect is an increase in the atmospheric thermodynamical stabilization". This claim is in agreement with our claims based on observations.

In light of this discussion, we made the following changes to the manuscript (note that Figs. R1 and R2 correspond to Figs. 6 and 7 in the revised manuscript).

In the introduction section, we changed the paragraph on page 12009, lines 19-22:
"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 2005-2008. Apart from appreciating the Amazon as an important and fragile place, there are a few good reasons to study the effect of smoke on clouds and the atmospheric temperature profile over the Amazon:
1. During the dry season the ITCZ moves north and the study area is under an anticyclonic flow and a subsidence zone with very little meteorology variance (e.g. Nobre et al., 1998).
2. The fires during the Amazonian dry season are anthropogenic and their location depends on the farmers, the farm location and on the government enforcement. Therefore the source distribution does not depend on the regional meteorology. The smoke will be advected by the prevailing wind, which is mostly easterly in the east and central

Amazon turning northerly in the western part of the basin when flow is blocked by the Andes."

In the discussion section, we added the following paragraphs (page 12016 between lines 11 and 12):
"Could the correlation between the atmospheric stability and AOD be due to a third agent, namely meteorology? The fires during the Amazonian dry season are anthropogenic and their location depends on the farmers, the farm location and on the government enforcement. Therefore the source distribution does not depend on the regional meteorology. Nevertheless, the null hypothesis should be that atmospheric stability favors the formation of aerosols, which results in the correlation seen in Fig. 3. Under the null hypothesis, performing the same analysis - a scatter plot of the temperature versus AOD - with a much less absorbing aerosols, should give the same results as seen in Fig. 3. During the months June - July (the beginning of the dry season), the meteorology is stable and similar to the months August-September, however since the biomass-burning starts only on August (Fig. 6), the aerosols are mostly biogenic, which absorb solar radiation much less (Schafer et al., 2008). Figure 7 shows the same scatter plot, but for the months June - July. During these months, the cooling near the surface (1000 hPa) during June - July is similar to the cooling during August - September. However, there is no heating at 850 hPa, compared to the months August - September, which is in contrast with the null hypothesis.
Although we cannot completely rule out the possibility that the cooling near the ground in June-July doesn't have meteorological component. Increased AOD could be due to more stable meteorological conditions that would favor the formation and concentration of particles. Using the tools exploited in this work, it is hard to unravel causality in the temperature trend near the surface. However, there is strong evidence that the heating at 850 hPa is primarily due to aerosol absorption.
Freitas et al. (2005) claimed from modeling results that: "...the presence of the smoke in the atmosphere results in a strong radiative forcing as these particles are very effi-

cient solar radiation scatterers and absorbers. The atmosphere responds to this forcing through a cooling of the low levels and a heating of the upper levels of the PBL [planetary boundary layer]. The net effect is an increase in the atmospheric thermodynamical stabilization". This claim is in agreement with our observationally-based arguments."

Finally, we have changed the following in the conclusion remark (page 12016, lines 19-25):
"However, it must be acknowledged that the microphysical effect is not completely eliminated, and some of the trends shown in the restricted subsets can be due to the aerosol effect on clouds. Moreover, in addition to the coupling between clouds, aerosol and radiation demonstrated here, the relationships between atmospheric temperature profile and AOD also include components that link atmospheric temperature responses to surface and biospheric processes, and to large-scale meteorology. We show that aerosols can have a significant contribution to these processes which is superimposed on the meteorological effects, and could be in opposite direction. The analysis presented here makes the argument, at the very least, plausible. Illustration of the more complicated picture requires tools beyond those employed in this study."

Reply to Minor Comments

1. Page 12008, Lines 1-6: 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. Answer: We changed the abstract beginning (lines 2-6, page 12008) to: "Aerosols suspended in the atmosphere interact with the solar radiation and thus change the radiation energy fluxes in the atmospheric column. In this paper..."
2. Page 12008, Line 24: Strictly speaking I would not call these feedbacks as none of the processes mention has 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" . . .

Answer: We changed the referencing to these processes. Lines 21-24 (page 12008) now read:

“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).”

3. Page 12008, Line 26: Are these really the right references to establish that the aerosol interacts with radiation?

   Answer: These references were not meant to establish the fact that aerosols interact with radiation, but to describe the results from this interaction. We moved the references to the end of the sentence to make it clearer.

4. Page 12009, Line 10: 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.

   Answer: In the Amazon basin, about half of the moisture comes from evapotranspiration by the vegetation. It was suggested by Andreae et al. (2002) that in response to high AOD values, the vegetation closes its stomata, thus reduces the moisture fluxes. Therefore, moisture flux is not expected to increase significantly with increasing AOD values. However, we agree that we are dealing with a complex system encompassing plant canopies, surface fluxes, aerosols and clouds, and that we can only offer plausible explanations at this point, much of

which is speculative. We changed the end of the paragraph (page 12009, lines 7-10) to:
"(iii) reduce fluxes of moisture from the surface (evaporation and evapotranspiration) to the atmosphere (Koren et al., 2004; Feingold et al., 2005). The reduction of moisture fluxes is also due to the closer of vegetation's stomata, in response to high smoke loading, as suggested by Andreae et al. (2002). Process (iii) is of great importance in the Amazonian atmosphere, where ~50% of the available atmospheric moisture comes from evapotranspiration of the canopy (Salati, 1987)."

5. Page 12009, Line 13: 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.
   Answer: We agree that these processes are still speculative; therefore we changed lines 11-12 (page 12009) to:
   "The two pathway conceptual model was suggested by Koren et al. (2008) who showed the relationship between cloud properties and aerosol loading."

6. Page 12009, 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.
   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 InfraRed Sounder (AIRS), on Aqua (Aumann et al., 2003). AIRS is a high spectral

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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."

7. Page 12010: 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 sounding 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)
   Answer: The soundings are launched at 8am and 8pm, local time (correspond to 12 and 0 UTC), while Aqua overpass is at 13:30 local time. This renders

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the soundings' temperatures less relevant to our study. However, we use the sounding data to translate pressure into height because it seems to us more realistic than the hydrostatic equation.

8. Page 12011: 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 length-scale of the measurement? You see what exactly you are plotting is not clear to me.
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:
$\Delta T\left(loaction, day, press.\right) \equiv T\left(location, day, press.\right) - < T >_{area}\left(day, press.\right)$
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."

9. Page 12011, 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.
Answer: We have taken the reference out.

10. Page 12011, Line 25: Outside of the surface layer an adiabatically unstable profile appear rather unusual 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.
Answer: Indeed, a temperature difference of ~18℃ between 1000 hPa and 850 hPa (see Fig. 3, solid magenta arrow, original manuscript) seems too large. This could be due to "surface contamination", meaning that the AIRS retrieval is affected from the surface temperature. However, the "surface contamination" is not expected to be AOD-dependent (although there is no direct evidence for this), so at most the 1000 hPa curve will be shifted downwards but the trend will stay more-or-less the same. We do note that an instantaneous measurement of the temperature at 13:30 could be unstable. In addition, the lower atmosphere in the Amazon is known to be slightly unstable, especially at noon. We added the following paragraph in the text in page 12012, between lines 3 and 4:
"We note that a temperature difference of ~18℃ between 1000 hPa and 850 hPa seems too large, since convection is expected to be an efficient agent in restoring neutral stability. This descripancy could be due to "surface contamination", meaning that the AIRS retrieval is affected from the surface temperature. However, the "surface contamination" is not expected to be AOD-dependent (although there is no direct evidence for this), so at most the 1000 hPa curve will be shifted downwards but the trend will stay more-or-less the same. We do note that an instantaneous measurement of the temperature at 13:30 could be unstable. In addition, the lower atmosphere in the Amazon is known to be slightly unstable, especially at noon."

11. Page 12012, Line 6: Although this may have been shown by Koren et al., 2008,

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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.
Answer: We changed the sentence to be less conclusive, it now reads (page 12012, line 6):
"Koren et al. (2008) suggested that the cloud cover correlates logarithmically with the aerosols optical depth (AOD)."

12. Page 12014: Did it not occur to the authors that meteorology might play a role?
Answer: We thank Prof. Stevens for drawing out attention to this issue. We added a discussion about the role meteorology might play. The addition is cited in the reply to the major comments.

13. Page 12014: 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.
Answer: This is an interesting direction that we did not consider; we thank Prof. Stevens for bringing it up. As Prof. Stevens says, the effect of enhanced absorption from the multiple scattering of the clouds depends on the cloud optical properties and relative heights of clouds and smoke layers. At 850 hPa, there is no difference in temperature increase with increasing AOD for the two cloud cover categories, implying that the chance of absorption is the same with and without clouds. We changed the paragraph to (page 12014, line 16 to page 12015, line 1. Note that this paragraph contains changes due to other comments, including comments from other referees):
"The temperature rise at 850 hPa is primarily a result of the absorption of solar radiation by the biomass burning aerosols at this level. The magnitude of increase of ~1 °C is consistent with expectations of 1-2 °C formed from radiative transfer modeling (Yu et al., 2002; Koren et al., 2004) and there is a steady rise in temperature as AOD increases. Do clouds interfere with this absorption? On partly

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cloudy days, the absorption might be enhanced due to increased scattering from clouds (termed 3-D effect, see Wen et al., 2006). On the other hand, clouds could also shed the aerosols below them, thus diminishing the absorption. From Fig. 6, there seems to be no difference in temperature increase at 850 hPa with increasing AOD for the two cloud cover categories, implying that the chance of absorption is the same with and without clouds. However, due to the large variablity of the data (as depicted by the error bars), we cannot determine the role clouds play in the absorption."

14. Page 12015, 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.)
Answer: This comment is 2-fold: the saturation of the aerosol effect, especially through the microphysical pathway, and the use of aerosols as a tracer for airmass, which also include again the role meteorology play. The saturation of the aerosol effect was shown previously by both observations (Breon et al., 2002) and modeling (Wang et al., 2005). We included these references and others in the revised manuscript in a new paragraph on page 12015, between lines 10 and 11:
"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)."

The second part of the comment (aerosols as airmass tracer and meteorology), was answered extensively in the reply to the major comments. The main idea being that the meteorology variance is relatively small during these months, due to a large scale subsidence. Also mentioned is the fact that the fires are anthropogenic and their location depends on the farmers, the farm location and on the government enforcement. Therefore the source distribution does not depend on the regional meteorology. An additional analysis of the months June - July was presented, showing that a non-absorbing aerosol does not cause heating at the pressure level 850 hPa, which is in contrast with the meteorology-induced hypothesis. A more detailed discussion, together of the references to the corrections done in the revised manuscript, can be found in the reply to the major comments.

To conclude, we would like to emphasize that we do not claim that aerosols are the only factor determining atmospheric stability or cloudiness. We show that aerosols can have a significant contribution to these processes which is superimposed on the meteorological effects, and could be in opposite direction. The analysis presented here makes the argument, at the very least, plausible.

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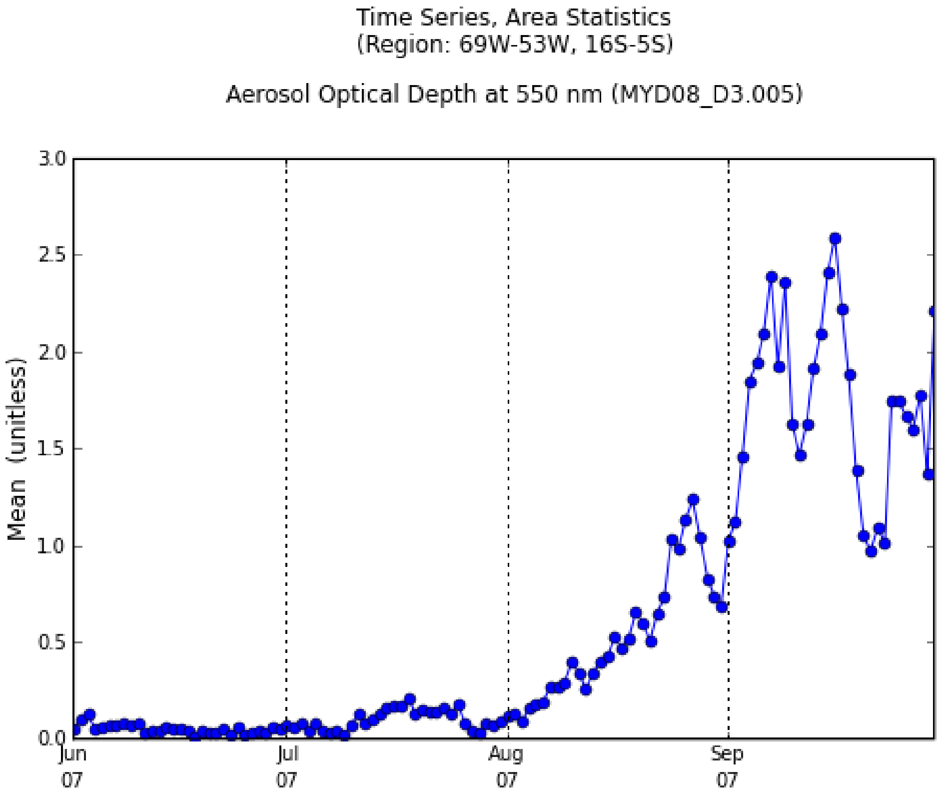

Fig. 1. Mean AOD for the area of interest (Fig. 1, original manuscript) for the months June – September. The burning season clearly starts on August.

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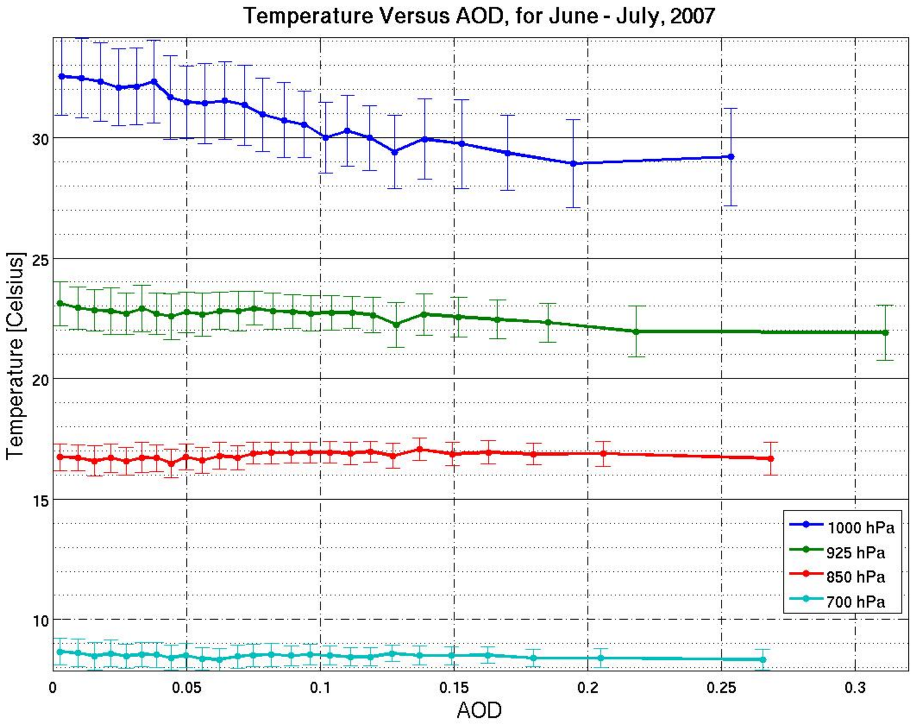

Fig. 2. The same as Fig. 3 (original manuscript), but for the months June – July. Note there is no apparent temperature change at 850 hPa pressure level (red curve).

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