Atmos. Chem. Phys. Discuss., 10, C12994–C13007, 2011
www.atmos-chem-phys-discuss.net/10/C12994/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of water vapor/aerosol loading trends and land cover on aerosol microphysical and radiative effects on clouds during the Amazon biomass burning season" *by* J. E. Ten Hoeve et al.

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Received and published: 31 January 2011

— 1. The paper deserves a better title. In particular, "trends" should be avoided here and THROUGHOUT the paper. People usually use "trend" to describe variation over long time spans e.g., 20+ years. You are discussing day-to-day variations of water vapor and AOD over the dry-to-wet transition season only. Does a title like "Aerosol microphysical and radiative effects on warm clouds during the Amazon biomass burning season as observed by MODIS: roles of water vapor and land cover" better describe what you are presenting?

We agree. We have changed the manuscript title to "Microphysical and radiative effects of aerosols on warm clouds during the Amazon biomass burning season as observed by MODIS: impacts of water vapor and land cover." Our only changes to your suggested title are that we replaced the word "roles" with "impacts" and we changed "Aerosol microphysical and radiative effects. . ." to "Microphysical and radiative effects of aerosols. . ."

We have also removed the term "trend" in all but a few instances in the paper as you suggested, replacing the term with other terms such as "variations," "patterns," or "relationships."

— 2. Abstract p.24920, line 2: better remove "high resolution" because MODIS has "Moderate" resolution just as its name suggests.

Changed as suggested.

— 3. p.24920, L9: change "Previous studies" to "Many previous studies".

Changed as suggested.

— 4. p.24920, L22-24: ": : :.then a linear relationship between the indirect effect and increasing AOD, assumed in a majority if global climate models: : :..". First, "the indirect effect" is ambiguous here. What are you referring to? Second, I am not sure if a majority of global climate models just simply assume "a linear relationship" between something and AOD.

We have modified the text to clarify that the indirect effect refers to the 1st indirect effect of aerosols on clouds (Twomey Effect).

We have also rephrased the sentence in question, "If (1) is a contributing mechanism, as we suspect, then an empirically-derived increasing function between cloud drop number and aerosol concentration, assumed in a majority of global climate models, is inaccurate since these models do not include treatment of aerosol absorption in and around clouds."

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The majority of global climate models parameterize the cloud drop number concentration from the aerosol number concentration (Pringle et al., 2009 and references therein). Other models employ more information including aerosol size distributions, composition, or vertical updraft velocity (Ming et al. 2006), but all of these models use at least partially empirical parameterizations to determine cloud drop concentration.

Ming, Y., V. Ramaswamy, L. J. Donner, and V. T. J. Phillips, A new parameterization of cloud droplet activation applicable to general circulation models, J. Atmos. Sci., 63, 1348-1536, 2006

Pringle, K. J., K. S. Carslaw, D. V. Spracklen, G. M. Mann, and M. P. Chipperfield, The relationship between aerosol and cloud drop number concentrations in a global aerosol microphysics model, Atmos. Chem. Phys., 9, 4131-4144, 2009

— 5. Introduction p.24921, L7: change “large uncertainties in our climate system” to “large uncertainties in the understanding of Earth’s climate system”.

Changed as suggested.

— 6. p.24921, L13: there is a modeling study that discusses possible roles of aerosol absorption on the South America dry-to-wet circulation transition: Zhang et al. (2009): Impact of biomass burning aerosol on the monsoon circulation transition over Amazonia. Geophys. Res. Lett., 36, L10814, doi:10.1029/2009GL037180.

We have now cited this reference in the introduction.

— 7. p.24921, L24: Andreae et al. (2004) argue that smoke aerosol slows the hydrology cycle through aerosol microphysical effects. I don’t think the paper provides any direct evidence that “smoke cools the surface, increases the static stability: : :.., suppresses surface heat and moisture fluxes”. So this is not an appropriate reference for here.

We agree. We have removed the Andreae et al. (2004) reference from the sentence.

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— 8. p.24923, L22-27: The atmospheric boundary layer height in the region is generally 1-2 km (lower over forest and high over pasture). It is reasonable to say "the majority of biomass burning smoke exists at or below the cloud layer at an approximate altitude of 3 km", as Davidi's case studies are corroborated by some CALIOP and GLAS analyses over longer periods. So you should not say the smoke is "generally mixed with the boundary layer".

We have removed the sentence in question and have changed the proceeding sentence as well. The text now reads, "Small cumulus clouds often form at the top of the atmospheric boundary layer, which is constrained by a subsidence inversion. The majority of biomass burning smoke exists at or below the cloud layer at an approximate altitude of 3 km (Davidi et al. 2009), although occasionally smoke plumes are pumped to higher altitudes through deeper cumulus convection."

— 9. p. 24924, L1-3: I would suggest to change the sentence to: "In some studies of aerosol-cloud interactions, variations of cloud properties are assumed to be weakly dependent on meteorology due to the stationary high pressure systems during the biomass burning season".

As you suggested, we have changed this sentence to "In some studies of aerosol-cloud interactions, variations of cloud properties are assumed to be weakly dependent on meteorology due to the stationary high pressure systems present during the South American biomass burning season."

— 10. p.24924, L13: "longer timescales", longer than what?

Longer timescales refer to longer seasonal timescales. In an effort to shorten the introduction however (Comment 15), we have removed this sentence since it was not integral to the discussion.

— 11. p.24925, L1-3, "smaller" and "finer" than what?

We have clarified this statement by removing the term "smaller scale" since temper-

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ature/pressure gradients resulting from land surface heterogeneities occur for both small scale deforestation (several kilometers) as well as regional-scale deforestation (hundreds of kilometers). We have changed this statement to "Regional-scale deforestation has been shown to result in mesoscale circulations that arise from land surface heterogeneities (Segal et al., 1988; Wang et al., 2000, 2009; Roy et al., 2002)."

— 12. p.24925, L4-7 and L17: you describe the lower branch of mesoscale circulations (a flow from forest to pasture) introduced by heterogeneity of land cover properties as "land breeze". But I think the otherwise. This kind of flow is analogous to the sea breeze and people call it "inland sea breeze".

We agree that conceptually the resulting breeze is more similar to a sea breeze than a land breeze, but we also feel that including the term "sea" may cause confusion. As a result, we changed the term "land breeze" to "inland breeze," as also used in other studies (e.g. Mahrt et al., 1993)

Mahrt, L., J. Sun, D. Vickers, J. I. MacPherson, J. R. Pederson, and R. L. Desjardins, Observations of fluxes and inland breezes over a heterogenous surface, J. Atmos. Sci., 51(17), 1993.

— 13. No comment listed.

— 14. p.24925, L16-17: I don't believe Koren et al. (2004) show evidence that "cloud fields generally form by noon and dissipate overnight throughout the Amazon Basin". There should be more appropriate references, such as Silva Dias, M. A., Rutledge, S., Kabat, P., Silva Dias, P. L., Nobre, C., Fisch, G., et al. (2002): Clouds and rain processes in a biosphere-atmosphere interaction context in the Amazon Region. Journal of Geophysical Research, 107, 8072. doi:10.1029/2001JD000335 and references therein.

Koren et al. (2004) states that "The scattered cumulus clouds (also called boundary layer clouds) emerge regularly in the morning over the eastern shore. By local noon

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they cover large parts of the Amazon Basin." Silva Dias et al. (2002) also confirms a mid-afternoon peak in convection, however, these studies largely focus on the wet season and not the dry season. The study by Negri et al. (2004) shows the diurnal cycle of cloudiness for the dry season over our study region using GOES IR retrievals (Negri et al., 2004, Fig. 8), showing that clouds form around 12 PM LT and dissipate after about 8 PM LT. We have included this reference and moved this discussion farther up in the introduction.

Negri, A. J., R. F. Adler, L. Xu, and J. Surratt, The impact of Amazonian deforestation on dry season rainfall, J. Climate, 17, 2004.

— 15. I think some efforts can be made to tight up the Introduction.

We have tightened up the introduction, as you suggested. Specifically, we removed some of the discussion regarding the effects of deforestation on regional hydrometeorology, and have shortened the discussion of meteorological variability in aerosol-cloud interaction studies. We have also removed some of the extensive discussion of the South Atlantic Subtropical High and have also shortened and combined the discussion of the temporal and spatial structure of the cumulus clouds present during our study. Overall, we have reduced the introduction by roughly 190 words.

— 16. Data and methods. This section can be tighten up too by simplifying description of the data.

We have shortened the description of the data, as you suggested. We have cut down the data description in the Data and Methods section by roughly 200 words by removing detailed information about these products not integral to their usage in the paper, and also by removing some unneeded references.

— 17. p.24926, L7-10: You already stated why you choose Aqua over Terra. So you may not need to say why you choose MODIS over MISR (because MISR doesn't fly on Aqua). Also you don't mention MODIS's much better coverage.

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We decided to remove this statement as you suggested. MODIS's coverage is briefly mentioned on P24926, L4, but not in the context of MODIS vs. MISR.

— 18. p.24926, L11, "Data from these products, which are provided at resolutions between 1-km and 5-km: : :" "these" is a little ambiguous. It appears in the context that "these products" include aerosol, cloud, water vapor, and temperature. But MODIS aerosol product is at a resolution of 10 km, not 1km or 5km.

To clarify, we have changed this statement to, "Data from the cloud, water vapor, and temperature products, which are provided at resolutions between 1-km and 5-km, are averaged to a resolution of 10-km x 10-km in order to conform to the Level 2 aerosol data." We have also moved this statement further down in the text when shortening the Data and Methods section (Comment 16).

— 19. p.24926, L20-22: "yield" should be "yields". It is better to cite the most recent MODIS over-land aerosol validation paper: Levy et al., Global evaluation of the Collection 5 MODIS dark-target aerosol products over land. Atmos. Chem. Phys., Vol 10, pp 10399-10420, doi:10.5194/acp-10-10399-2010, 2010.

Changed as suggested. We have also replaced the Levy et al. (2007) reference with the Levy et al. (2010) reference.

— 20. p.24928, L19: ": : :.increases with time: : :..", at what time scale?

To clarify the time scale, we have changed this sentence to, "The percentage of deforested land increases from year to year in our fixed study region due to ongoing forest conversion activities."

— 21. p,24929, L7: Are 700 hpa wind vectors representative of weather pattern? Need a reference here.

We have changed the text to state, "The anti-cyclonic high pressure circulation is found to be most discernable at 700 hPa in the NCEP/NCAR Reanalysis data, however, lower level (e.g. 850, 925 hPa) wind vectors may also be used since high pressure extends

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throughout the troposphere."

Koren et al. (2004) performed a similar stratification of days over the Amazon and used wind field maps at 925 mb, 850 mb, and 700 mb, but we used only 700 mb data since the anti-cyclonic circulation of the South Atlantic Subtropical High was more discernable on most days in the 700 mb NCEP data compared with the lower-level NCEP data. However, since the South Atlantic Subtropical High is a warm-core high pressure system, the high pressure circulation extends throughout the lower- to mid-troposphere. As a result, the large scale circulation is often very similar between 700 mb and lower heights (e.g. 850, 925 mb), and so we would have selected most of the same days based on 850/925 mb data compared to 700 mb data.

— 22. p.24929, L29: change "between MODIS and AERONET retrievals" to "between MODIS retrievals and AERONET measurements".

Changed as suggested. We have also made a similar change in the caption of Figure 2.

— 23. Results It may be better to change 3.1 title to "Effect of water vapor variability on aerosol-cloud interactions".

Changed as suggested.

— 24. p.24930, L21-24: It may be not that straightforward to extend the conclusions over the tropical Atlantic to Amazon basin.

We have rewritten this statement to make it more conservative, "Koren et al. (2010) found that MODIS aerosol-cloud relationships over the tropical Atlantic were likely affiliated with physical mechanisms and not retrieval artifacts, suggesting that the relationships found in our study may also be physically-based."

The Koren et al. (2010) study has provided evidence contradicting the hypothesis that remotely-sensed relationships between cloud properties and aerosol loading are largely due to satellite retrieval artifacts. We do not believe that the results found in

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Koren et al. (2010) over the Atlantic Ocean will directly extend to our region over the Amazon Basin; however the Koren et al. (2010) study helps support the idea that aerosol-cloud relationships observed by satellite are likely to be physically-based and not completely satellite artifact.

— 25. p.24931, L9-14: I don't quite understand these statements. It appears to me that these statements contradict with those in line 26-29. Or maybe I missed something here.

To clarify this association between cloud fraction, CWV, and AOD, we have changed the statement in L9-11 to, "Thus, if we plot CWV and cloud fraction directly against AOD, separated into the two halves of the season, we would expect the signs of the regression slopes to be the same for CWV and cloud fraction in each half season assuming CWV has a controlling influence on cloud fraction."

We are stating in L9-11 that if CWV was the dominant influence on cloud fraction (i.e. no influence of aerosols), then the signs of the regression slopes of cloud fraction versus AOD and CWV versus AOD should be the same. However, in L26-29, we explain that in the second half season in Fig. 3a/b (red line) the correlation between cloud fraction and AOD is unlike the correlation between CWV and AOD, pointing to aerosols as a modifier of cloud properties.

— 26. p.24932, L15: what do you mean "source variations"?

Source variations refer to within-season variability in biomass burning aerosol emissions. We have removed this term to avoid confusion.

— 27. p.24932, L27-29: ": : :..because CWV should not be affected by a warming atmospheric column". Can the warmer atmosphere promote more evapotranspiration and hence CWV?

Enhanced incoming solar radiation and thus a warmer atmosphere could promote more evapotranspiration and thus higher CWV, however, Fig. 4a and Fig. 4c show that

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restricting the analysis to cloud fractions below 0.5 compared with all cloud fractions does not appreciably change the plots of CWV versus AOD. Therefore, it is unlikely that higher surface solar radiation due to lower cloud fraction will change CWV appreciably.

Even though this effect is unlikely, we have changed L27-29 to be more cautious, "Even though lower cloud fractions may lead to increased solar radiation at the surface, and thus more evapotranspiration which could increase CWV, Fig. 4c shows little difference in both the shape of the graph and the magnitude of the change of CWV with AOD when compared with Fig. 4a. This suggests that the lower cloud fraction does not have an appreciable effect on the relationship between CWV and AOD."

— 28. p.p.24933, L26: "five percentile groupings". Is it enough to just say "five groupings"?

Changed as suggested.

— 29. p.24934, L4-7: It is not surprising to me that "CWV only varies marginally between AOD bins" because you have grouped CWV into 5 bins. I think you may not need to show Figure 5a anyway.

We have better motivated the importance of the figure in the discussion on P24933. We have added, "Figure 5a illustrates that while the range of CWV represented by each grouping is 0.65 cm on average, CWV only varies marginally between AOD bins, with a maximum difference of 0.10 cm between any two bins in any grouping. This nominal variation in CWV within each grouping is likely to have only a minimal effect on cloud properties." We believe the plot also helps understanding of the stratification process, and therefore we feel it should remain in the manuscript.

— 30. p.24934, L8 to p.24935, L6: It is interesting that you show difference in the cloud fraction-AOD relationship between this study and Koren et al. (2004, 2008). Later on you attribute part of reasons for the difference to the North-South gradient of aerosol absorption. Do you think the variation of CWV may have "contaminated" those previous

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analyses?

We have added a statement after L8 detailing this as a possibility, “Yet, our results indicate that variation in water vapor loading may have inadvertently affected previous analyses of aerosol-cloud interactions over the Amazon.”

Because our study region and study period is different than the study region/period in Koren et al. (2004, 2008), we cannot say for certain that variation in CWV likely contaminated these previous analyses. However, we do show that aerosol-cloud correlations in our study region (which is encompassed by those regions used in the aforementioned studies) can be affected by variation in water vapor, suggesting this is a possibility.

— 31. p.24935, L7-8: “If the aerosol is affecting cloud microphysics, the signal should be apparent in the cloud optical depth (COD) as well in the cloud fraction”. I am not quite sure about this assertion or hypothesis. There may be some processes (e.g., entrainment, aerosol absorption, etc) that compete with aerosol microphysical effect and the observations may not show signals that are consistent with the Twomey effect.

We agree. We have changed this statement to make it more general and cautious. We replaced this statement with, “Aerosol microphysical and radiative effects may also be observed in the cloud optical depth (COD) in addition to the cloud fraction.”

— 32. p.24937, L2: what do you mean by saying “non-polluted values of COD”?

Non-polluted values of COD refer to COD that has not been modified by biomass burning aerosol. The Wilcox et al. (2009) study only looked at biomass burning aerosols above clouds (microphysical interaction of aerosols and stratocumulus clouds were prevented by an inversion layer above the clouds). As a result, we have removed the term “non-polluted” since we have already stated in the text that the study analyzes aerosols above clouds.

— 33. Section 3.2 How about changing title to “Effect of land-cover on aerosol-cloud

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interactions"?

Changed as suggested.

— 34. p.24938, L5-6: You should also mention albedo change that modifies the net surface solar radiation.

We have added this effect to the statement in the text, "Land use changes that convert forest to pasture and cropland modify net radiation at the surface through albedo changes and by altering surface heat and water vapor fluxes. These changes can in turn affect cloud development."

— 35. p.24938, L11: you should consider moving the last sentence of this paragraph (p.24939, L3-4) to here, immediately before "Warm cloud: : :". You did a good job in explaining the forest-pasture contrasts and citing literature. Because this is not your main focus, probably you could consider to tight it up by rephrasing.

We have moved the last sentence up in the paragraph, as you suggested. We have also tightened up the discussion of forest-pasture contrasts since, as your suggestion stated, this is not the main focus of the section.

— 36. p.24940, Figure 8b and corresponding text: It is interesting that CWV difference between forest and pasture doesn't seem to change with increasing AOD. This observation evidence seems to corroborate with results of several model simulations, such as Matsui et al., 2008; Zhang et al., 2008, and Yu et al., 2002. These modeling studies show that absorbing aerosol reduces sensible heat flux much more than evaportranspiration over land. So the local source of CWV would not differ significantly between forest and pasture. Although Zhang et al. (2009) suggest that absorbing aerosol can change the monsoon circulations that would influence large-scale moisture transport and CWV, it is not expected that the resulting CWV change would depend on land cover.

We have now referenced the Matsui et al. (2008) study as well as the Yu et al. (2002)

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study in the text of the paper. We have also included a sentence describing their model results to help support our observational analysis.

— 37. p.24942, L20: what is NE, WE?

This is a typo. We have changed it to NS, WE for North/South and West/East.

— 38. p.24953, Fig.1: What is the white dashed line across the map? "Yellow pixels represent: : :" it doesn't appear "yellow" in my screen.

We have changed the word from yellow to light green to be more accurate. The white dashed line is the result of piecing together MODIS granules that have been reprojected from an integerized sinusoidal projection (the provided projection on the LP DAAC website) to a geographic projection using the MODIS Reprojection Tool. It does not have any physical meaning; and therefore we have removed the white line from the figure.

— 39. p.24955, Fig3: Because (a) and (c) look so much alike (so are (b) and (d)) in terms of variations, probably you only need (c) and (d) and say something in text about (a) and (b).

We have removed figures (a) and (b) and have only retained figures (c) and (d) in Figure 3, according to your suggestion. We have also amended the discussion in the text to briefly mention removed figures (a) and (b). In addition, we have adjusted Supplementary Material S.1 accordingly.

— 40. p.24959, Fig.7 caption: What do you mean "Clouds that extend throughout the column my be misclassified as heavy aerosol plumes"? Some aerosol features are actually clouds? 5th percentile and 50th percentile are not clear to me. Did you sort the cloud top pressure from low to high or from high to low?

Firstly, we have changed this figure according to a comment by Reviewer 3. Occasionally, thick aerosol layers may be characterized as cloud according to the CALIPSO user guide; however, we use only quality-controlled data to minimize the contamina-

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tion. In the previous version of the figure, we use all data that were not considered “bad” quality. However, in the new version of the figure, we only use data with a “medium” confidence or higher. Because we are now using higher quality data, it is less likely that misclassification will occur, which we now state in the figure caption.

The 5th percentile refers to the highest 5% of warm clouds and the 50th percentile refers to the median warm cloud top height, so we sorted from the highest altitude clouds to the lowest altitude clouds. We have clarified this process with additional text in the figure caption as well.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 24919, 2010.

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Interactive Comment