We thank the two anonymous referees for positive and constructive comments. Below are our responses to Referee #1 and Referee #2 and a list of voluntary changes.

REFEREE #1

This paper presents airborne Sun photometer measurements acquired from the NASA P-3 aircraft over Alaska, Canada, and California during the ARCTAS mission. The paper focuses on the Angström exponents derived from this instrument and the use of these measurements to investigate aerosol type and infer FMF fraction. Comparisons with airborne in situ measurements are also presented. The AOD measurements are also used to investigate horizontal variability of AOD and how this varies with location.

The paper provides analyses of a new airborne Sun photometer dataset collected over Alaska and Canada where such measurements have been sparse. The paper uses techniques published earlier and applies these techniques to this new dataset. The paper is generally well written. Most of the figures are satisfactory, but there a few noted below that are much too small and must be clarified to understand what is plotted.

My recommendation is to publish after the authors satisfactorily address the comments listed below. Most of these comments are minor. There are two particular areas that should be addressed. The first is the impact of cloud contamination and cloud screening should be discussed in much more detail. This is barely mentioned in the paper. Given the prevalence of ice particles over Alaska during April, there is a good likelihood that these ice particles will interfere with the AOD measurements. Also, with the very high AODs associated with the smoke over Canada, it is not clear how cloud interference can be completely removed given the large spatial and temporal variability of AOD which would mask the cloud variability. The second is the retrievals of fine mode fraction (FMF) from the AOD spectra. The authors use the method of O'Neill to derive this. This is fine, but there is no mention of how this compares with previous studies (which included some of the authors) in using methods described by Anderson et al. (2005) and Redemann et al. (2009). The differences between these methods should be described in this paper.

1. (page 1, line 5) This lists the 499 nm Angström exponent. Typically the Angström exponent refers to a pair of wavelengths or a range of wavelengths. The range of wavelengths should be indicated here.

Authors: The text now reads "The Angstrom exponent… derived at 499 nm from a second-order polynomial fit to the AOD spectra measured with the 14-channel Ames Airborne Tracking Sunphotometer (AATS-14) over 354-2139 nm".

2. (page 1, line 5) The 499 nm Angström exponent is listed as 1.4+/-0.3 but the value between 2-4 km is listed as 1.6-1.8? Why not be consistent in indicating these values? Why not indicate that the value between 2-4 km is 1.7+/- 0.1?

Authors: Modified.

3. (page 1, line 7) Presumably "this" refers to the Angström exponents in the altitude regions mentioned in the previous sentence. However, it is not at all clear how these values indicate that the aerosols in these layers are from anthropogenic emissions and biomass burning. This seems to imply that pollution and biomass burning aerosols can have only these values of Angström exponent and this is not conclusively demonstrated.

It may be true that the AMS and black carbon measurements indicate this; if so, the wording should be changed to indicate that these in situ measurements are the prime reason for making the statement about the type of aerosols in these altitude ranges.

Authors: The text now reads "Arctic haze observed in <2km and 2-4 km over Alaska in April 2008 originated mainly from anthropogenic emission and biomass burning, respectively, according to aerosol mass spectrometry and black carbon incandescence measurements. The Angstrom exponent for these air masses is 1.4±0.3 and 1.7±0.1, respectively, when derived at 499 nm from a second-order polynomial fit to the AOD spectra measured with the 14-channel Ames Airborne Tracking Sunphotometer (AATS-14) over 354-2139 nm."

4. (page 1, line 8) Should be AOD spectra.

Authors: "AOD" inserted, as suggested.

5. (page 1, line 15-17) This sentence describing comparison between AERONET and P3 AOD measurements should indicate the wavelength (500 nm?) corresponding to the 0.02 difference and the direction of the difference (which is higher?).

Authors: To reflect the fact that the AATS measurements were sometimes higher and sometimes lower than the AERONET, ± has been inserted. Also "between 340-1640 nm".

6. (page 1, line 13-15). Likewise, when indicate the rms difference in FMF, should indicate which measurement is higher or lower.

Authors: "(in both directions)" has been inserted.

7. (page 5, line 10-11) There is not enough information that describes how cloud screening was done. This is of particular concern for the cases in Alaska during April 2009 when there were many times when ice particles were present. These ice particles were apparent in other ground based and airborne datasets, are especially apparent in lidar data. For the Sun photometer measurements, setting a criteria to screen out all the ice would likely result in removal of also much aerosol; likewise, relaxing the criteria to allow more aerosol measurements would also likely allow ice to be present. The authors need to discuss in much more detail how the cloud screening was done and provide some discussion about the potential presence of ice. The impact of ice would likely be much larger than the sources of error listed on page 6.

Authors: The text below has been inserted.

As Redemann et al. (2009) pointed out, the AATS cloud screening method does not solely relie on the spatial varibility of its transmission measurements. Instead, as long as the AATS instrument maintains tracking of the solar disk through or near a cloud, the Angstrom exponent of the optical depth retrievals is a second test of the effectiveness of the cloud screening method. In this test, the variability of the Angstrom exponent is used to decide whether a feature that shows large spatial variability is likely a cloud or an aerosol plume. If, for example, a feature shows large transmission variability and shows a decrease in Angstrom exponent, we conclude that the feature is likely a cloud and the feature is flagged accordingly. If, on the other hand, a feature shows large transmission variability but no change in Angstrom exponent when compared to adjacent measurements, we conclude that the feature is an aerosol plume. We concede that our method may unintentionally filter out some cases of heavy smoke during the Canada phase of ARCTAS. We concede further that our method may contain remnants of cirrus cloud contamination if the optical depth of such cirrus clouds is small enough so as to not affect the Angstrom exponent calculations of the combined aerosol-cirrus optical depth significantly. In one case during ARCTAS-spring, namely on April 9, 2008 at 21:15UT, near 74^{o}N and 139^{o}E, we encountered a feature that showed relatively large variability in transmission, but relatively slow changes in Angstrom exponent. Given the conditions of the spring phase, it is likely that this feature is a spatially highly homogeneous cirrus cloud, with spatial homogeneity unmatched at lower latitudes. The total optical depth of this feature is of the order of 0.2 – 0.3 and given its isolated nature it has no bearing on the further analysis in this paper. During no other ARCTAS-spring flight did we find features of similar high variability in transmission, but low variability in calculated Angstrom exponent. Therefore, we estimate such cirrus contaminations to be possible at levels of 10% of AOD, generally equivalent to optical depths of 0.01-0.02 or less.

8. (page 10, last paragraph) What is the correlation between Angstrom exponent and organic mass fraction? Likewise between Angström exponent and pollution? The paper currently does not provide enough information to show that there is a high correlation. The paper leads one to believe that smoke was present above 2 km and pollution below 2 km; however, there were occasions (ex. April 19) when smoke was observed by surface instruments at the DOE ARM NSA site and so smoke is not necessarily confined to 2-4 km.

Authors: Our revised text emphasizes that "Our 15-day observation in this area does not provide any proof that the vertical structure persisted throughout the spring."

We do not present data from our transit (to California) flight on April 19.

We hesitate to apply statistical treatments to our airborne data of limited duration. In particular, Angstrom exponent is noisy when the absolute value of extinction coefficient is low. Excluding the background cases from regression calculation is inevitably a subjective exercise, not to mention it further reduces the size of samples. Just to respond to the referee's questions, the correrlation between the organic mass fraction and Angstrom exponent, and that between SP2 BC mass and Angstrom exponent, are both lower than 0.1.

9. Figure 3a, 3b are way too small. I could not read the legends in these figures (even with my reading glasses.) Consequently, I can not determine what is actually plotted in these figures.

Authors: Fonts in Figure 3 have been enlarged.

10. (page 11, line 24) replace us with NASA P-3.

Authors: Text has been modified as suggested.

11. (page 12, line 9) When AOD at 499 nm changed by 2.6, was this due to vertical variability or horizontal variability of the smoke?

Authors: "The low AOD to the east of 108.48 ^{o}W throughout the column (see the projection to the bottom plane of the figure) testifies to the variability in the horizontal direction, not just in the vertical." has been inserted.

12. (page 12, line 23-24) What was the source of the dust? What other information or data are present to corroborate this inference of dust?

Authors: We have modified the text: "They were low presumably because of either dust or ice particles. These two are difficult to separate, but we estimate the contribution of ice particles to be 0.01-0.02 at most (see Sect. 2.1 and references for our cloud screening method) " We do not have solid evidence as to which of dust or ice particles existed and, if it is dust, where it came from.

13. (page 13, line 8-9) This analysis does not account for cloud or ice contamination.

Authors: The following text has been inserted. "Cloud contamination (including cirrus) is unlikely as we screened data based on both spatial variability and Angstrom exponent (Sect. 2.1)."

14. (page 14, line 5) What is the horizontal distance corresponding to 5 seconds?

Authors: "This time period corresponds to 0.6 km or less in horizontal distance." This sentence has been inserted to the manuscript.

15. (page 14, line 18) What were the values of particle hygroscopicity?

Authors: f(RH) was near 50. This is for the data point indicated with long vertical error bars in Figure 5. This profile was flown near clouds and associated with ambient RH >97% and a calculated f(RH) of ~50. This information is now given in the text.

16. (page 14, Figure 14b) Why not plot both as either AOD or extinction?

Authors: One instrument measured AOD and another two together measured extinction. This is noted in Section 3.1.3 where Figure 4 is mentioned for the first time.

17. (page 16, line 24-25) What was the top altitude of the AOD profile?

Authors: "(7400 m)" has been inserted.

18. (Page 19, line 13-14) The analysis uses O'Neill's method to derive FMF from spectral AOD. It would be good if this paper had also used or at least commented on the methods of Anderson et al. (2005) and Redemann et al. (2009) in the use of AOD spectra to infer FMF. These authors had developed empirical relationships to infer FMF using AATS-14 data; it would be good to know how well these relationships would work in these studies.

Authors: The suggested analysis has been made. The second last paragraph now says, "Previous studies in East Asia (Anderson et al., 2005) and Mexico (Redemann et al., 2009b) fit a second-order function of in situ extinction Angstroom exponent to in situ derived SMF, in an attempt to translate column AOD into SMF. The functions, if applied to the ARCTAS column-integral results presented here, underestimate SMF by ~0.1. This is shown in online supplementary Figure S2."

19. (Figure 8) Both Fig. 8a and 8b show outliers. What do these represent? How were they deemed outliers?

Authors: The filled markers indicate the vertical profiles that see agreement within 3%+0.02 between the layer AODs (Figure 5). Other profiles are marked with empty circles. These have been clarified in the text and the captions for Figures 7 and 8.

20. (Page 23, lines 11-21) This is also relevant to determine whether a proposed satellite sensor needs to have very high resolution or whether lower resolution would be sufficient.

Authors: This part of text has been removed, after the referee #2's suggestion to write a separate paper on horizontal variability.

References

Anderson, T. L., Wu, Y., Chu, D. A., Schmid, B., Redemann, J., and Dubovik, O.: Testing the MODIS satellite retrieval of aerosol fine-mode fraction, J. Geophys. Res., 110, D18204, doi:10.1029/2005JD005978, 2005.

Redemann, J., Zhang, Q., Livingston, J., Russell, P., Shinozuka, Y., Clarke, A., Johnson, R., and Levy, R.: Testing aerosol properties in MODIS Collection 4 and 5 using airborne sunphotometer observations in INTEX-B/MILAGRO, Atmos. Chem. Phys., 9, 8159-8172, 2009b.

REFEREE #2

Interactive comment on "Airborne observation of aerosol optical depth during ARCTAS: vertical profiles, inter-comparison, fine-mode fraction and horizontal variability" by Y. Shinozuka et al.
Anonymous Referee #2
Received and published: 14 November 2010
Review of
Airborne observation of aerosol optical depth during ARCTAS: vertical profiles, intercomparison, fine-mode fraction and horizontal variability
Lead Author: Y. Shinozuka
Summary
This is an interesting paper in that illustrates the sources of variability of observed AOD. By combining information from an in-situ platform for characterization of microphysical properties, an onboard and a ground based sunphotometer, the authors had most of the tools needed to assess different aspects of passive aerosol remote sensing. Specifically, this works compares fine-mode fraction retrievals with the same information from in-situ measurements, assess the reason why there are differences between in-situ and retrieved AOD and reports average optical properties of aerosols found in Arctic hazes, Canada fires and Monterrey area aerosols.
Overall there is good and useful information and it makes a good contribution to the field of aerosol remote sensing and linkage with observed in-situ properties.
I have no major objections for this paper and I think it should be published. I have, however, several comments and clarifications that I think they should be added to the text since there are sections that I found a bit confusing to read.
Specific/technical comments.
One general major concern is that figures were too cluttered and they way ACP formats the draft papers is such the figures are very small and I was not able to follow the text because I could not identified the features in the figure (this is based on reading a printed version of the paper).
The following comments are based on the version of the paper available online at : http://www.atmos-chem-phys-discuss.net/10/18315/2010/acpd-10-18315-2010-print.pdf
Abstract
The abstract should include an introductory paragraph or a sentence stating the aerosol or region of observations " spring and summer Arctic hazes, biomass burning in Canada and continental/pollution aerosols near Monterey".

Authors: The second sentence now has "Arctic haze observed in <2km and 2-4 km over Alaska in April 2008…." The 5th sentence now has "…smoke plumes from recent biomass burning in central Canada in June and July 2008…."

Line 25: the AOD variability should be reported over similar spatial ranges. Line 22-24 talks about high aerosol variability over a 20 km range in Canada fires whereas for Alaska fires the variability is reported over 6km range. So in order to compare aerosol

in the different areas I think the spatial ranges should be the same.

Authors: The part of paper relevant to horizontal variability has been removed.

Introduction
page 18318, line 7-10: There are a few studies that appear to be missing and were amongst the first to compare in-situ integrated aerosol properties and remote sensing: Clarke et al., 1996, Remer et el , 1996; Gasso and Hegg, 2002 .

Authors: Gassó, S., and Hegg 2003; has been added to the introduction, because this article uses AATS-14 data.

page 18319:, line 11: the authors should consider or compare with the results of Gasso and ONeill study where they compared the insitu SMF and AATS retrieved FMF.

Authors: Gassó and O'Neill (2006) added to the introduction.

18322, line 6: what is a non-zero field of view? please clarify.

Authors: "(±1.85^{o} of the axis)" has been inserted. The entire sentence has been revised. "Through its non-zero (±1.85^{o} of the axis) field of view, the AATS-14 receives some diffuse light in addition to the direct solar beam."

18326, Section 3: before starting with this section, there should be a brief description/ discussion of the aerosol types typical of the 3 regions studied. This is just to put into context the subsequent results and discussions.

Authors: The text now has "Here is an overview of our AOD observations during ARCTAS. We mainly observed Asian outflow over Alaska, combination of biomass burning and marine aerosols over California and smoke from local forest fires over central Canada."

Section 3.1.1: two points :
1) this section is the only one that mentions anything about clouds and possible contamination. The particular situation of cirrus mentioned in this section is just briefly mentioned and it appears the authors did not make an effort to discriminate for it. Can you expand more on why cirrus could not be properly screened?
2) another possible aerosol type present in this environment are those aerosols generated at the open leads and polyanas by organic precursors such as bacteria, organic gases and organic gels. Another possibility are very fine aerosols from nucleation in the clean atmosphere. Both aerosols are probably present; they are probably at very low concentration although not clear how low and not known optical signature. However, and for example, some episodes of high Angstrom coefficient, low extinction could be related to such aerosols.

Authors: We have added description and discussion on our cloud screening method in Section 2.1, in addition to reference to our previous papers with more details. In Sect. 3.1.1 we mention the possible presence of ice particles and refer the reader to Sect. 2.1.

We have added a comment on open leads and nucleation, referring to previous studies. Other potential sources of aerosols include nucleation at cloud tops followed by subsidence (Clarke et al., 1998; Garrett et al. 2002) and open leads (Ferek et al., 1995). However, unlike anthropogenic pollution, these sources account for only a minor contribution to the total aerosol in winter and spring (Garrett et al. 2004 and references therein).

18328, line 13. I've never heard of a flight "suffering from dirt" , Please use a more appropriate language.

Authors: Modified to "because of dirt ".

18329, line 5: replace "affected" with "was located" or "touched"

Authors: Replaced with "touched".

18332 lines 0-25 : this is an interesting discussion but it lacks any consideration to cloud contamination . Please expand.

Authors: The possible cloud contamination is mentioned immediately after this discussion. "There are, however, caveats associated with this variability parameter. It does not capture the variability in aerosol loading under clouds, for which AATS data are either masked or, much less likely (Sect. 2.1), contaminated." After this we briefly describe an example case with possible cloud interference.

Section 3.3 : Add an introductory sentence indicating what aerosol data was considered in this section: all Arctic, Canada and Monterey? or a subset of them? The idea is just to indicate what aerosol types are included in the analysis.

Authors: An introductory sentence has been placed at the beginning of Sect. 3.3. "The general agreement between the airborne instruments and the AERONET data (Sect. 3.2) makes it easier to relate our aircraft experiment to the continuous observations from the extensive ground network. As an example of such a link, the wavelength dependence and fine-mode fraction are each compared between the multiple observations in this section. As in the previous section, the comparison is made for the 55 vertical profiles selected from all phases of the campaign."

18336, line 25. Not clear why the "discrepancies are largely attributable to horizontal structure" statement? what is the evidence to back this up or is it an expectation of the authors? Please clarify.

Authors: To clarify, the text now says "This is consistent with the fact that the relative difference between the AATS and in situ layer AODs are more or less equal across all wavelengths (i.e., data points for the three wavelengths lie on a line nearly parallel to the 1:1 line for many outlier cases in Figure 5)."

18337 line 5-8. Similar as previous comment. The statement does not make clear the origin of the "expectation" , a reader expectation can be a very different than the writer's. Please, clarify this statement.

Authors: ", the latter of which represents a spatial domain wider than the aircraft's and requires separation of surface reflectance." has been added.

18337 line 16. Add (SMF) next to Submicron fraction.

Authors: (SMF) added.

18338 line 10-15. The authors should consider the fact that the aerosols observed do not have much of a signal in the 2.1um channel since there wasn't much dust in these flights. Compare w/Gasso and O'Neill 2006 study which had quite a bit of dust in the ACE-Asia mission.

Authors: The 1019-nm AOD is now shown instead of the 2.1 um AOD. Data with 1019-nm AOD under 0.04 are masked with grey color.

Also, comparison with Gasso and O'Neill (2006) has been made. " The in situ SMF is within ±16% of the AATS FMF for two thirds of the 19 profiles with 1019nm layer AOD >0.04, a criterion employed by Gasso and O'Neill (2006). This margin is only slightly wider than 5-15% Gasso and O'Neill (2006) find for a fraction of ACE-Asia with high (>0.5) FMF. The authors point out that in ACE-Asia the differences tend to decrease as FMF approaches 1. This trend is not evident in our ARCTAS data."

18339, line 11-12 : Not clear what a "segment of flight" is? are you referring to a constant altitude transect , an spiral/straight ascent/descent, all of them? Please clarify. On a similar note, what are the "potential number of data points"? are they all the points in a segment?

Authors: This part of text has been removed, after the referee #2's suggestion to write a separate paper on horizontal variability.

Section 3.4.1 and 3.4.2: I found these sections a bit obscure and difficult to follow. Particularly, section 3.4.2 tries to explain with words a concept that is relatively new and I think warrants more space . Although I find the approach outlined to study horizontal variability impact as correct and sensible, I think there are a number of points missing in this discussion which is the connection with actual aerosol dynamics such as wind direction (or air mass motion) and direction of aircraft motion (perpendicular or parallel to wind), measurements of state of mixing of aerosol layer and impact of cloud contamination. Because of this, I think these two sections with the additional discussion is worth a small, separate publication probably. In summary, my suggestion is that authors expand this section to make it clearer (probably an additional figure would help) or remove it all together for use in separate paper.

Authors: Of two options the referee suggested, we elect to remove this section all together for a separate paper.

18343, line 7: where it says "(dotted curves)" , what figure you are referring to?

Authors: This part of text has been removed, after the referee #2's suggestion to write a separate paper on horizontal variability.

References

Remer, L. A., Gassó, S. Hegg, D., Kaufman , Y.,Holben, B., 1997: Urban/industrial aerosol: Ground-based sun/sky radiometer and airborne in situ measurements. J. Geophys. Res., 102, 16849-16859

Gassó, S., and N. O'Neill, 2006: Comparisons of Remote Sensing Retrievals and In-Situ Measurements of Aerosol Fine Mode Fraction during ACE-Asia. Geophys. Res. Lett., 33, L05807, doi: 1029/2005GL024926.

Clarke, A. D., J. N. Porter, F. P. J. Valero, and P. Pilewskie (1996),Vertical profiles, aerosol microphysics, and optical closure during the Atlantic Stratocumulus Transition Experiment: Measured and modeled column optical properties,J. Geophys. Res., 101(D2), 4443–4453, doi:10.1029/95JD03140.

Gassó, S., and D. A. Hegg, 2003: On the retrieval of columnar aerosol mass and CCN concentration by MODIS. J. Geophys. Res., 108 (D1), 4010, doi: 10.1029/2002JD002382.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 18315, 2010.

Voluntary changes

Title. “horizontal variabilily” has been removed.

Introduction. The paragraph beginning with “Fourth”, relevant to horizontal variability, has been removed.

Sect. 2.2 The nephelometer measurement average time has been modified to 5 seconds. The nephelometer residence time during ARCTAS was recently identified to be 5 seconds, rather than 10.

The absorption correction scheme has been updated this year by Virkkula (2010). This is now noted in Sect. 2.2: “We correct our data for the scattering artifact as well as calibration error after Virkkula (2010), an erratum for Virkkula et al. (2005). This correction, a function of the ratio of the scattering coefficient to the extinction coefficient (i.e., single scattering albedo, SSA) and the wavelength, reduces 530-nm absorption nearly as much as does the classic correction scheme widely used for a single-wavelength prototype of the PSAP (Bond et al., 1999). We assume the uncertainty for absorption coefficient is 20%.“ Note that because absorption is a minor (<10% for most data) part of extinction and because the updated absorption is 20% higher than the previous values, this update minimally affects the in-situ derived AODs. The numbers and figures have been updated with the new absorption data.

Sect. 3.1.1. Reference has been updated for Stone et al. [2010].