Main comments:
1. On lines 591-597, you use BC as a reference to define BrC. The implication of BC as a surrogate is that there is no LAC resulting from secondary organic aerosol (SOA). How does the non-BC refactory carbon mass compare with the OM from the AMS for the cases you use in Figure 7? Unless all SOA is non-absorbing, then the AMS OM may be a better surrogate for non-BC absorbing carbonaceous aerosol. Please discuss this.

There seems to be some confusion here about how we have calculated BrC and its relation to SOA. In the ARCTAS case, refractory aerosol mass is large compared to other campaigns. This appears to be due to the tail of the dust distributions extending to submicrometer sizes. Thus we cannot create robust vertical profiles of submicrometer refractory mass by screening out the dusty data. Instead, we use established ratios of BC to refractory mass from other campaigns (when dust was much less prevalent), and to separate polluted cases from BB cases, in order to estimate what the ratio could be over the Arctic. As discussed, the ARCTAS ratios are close to but not identical to the other investigations so we are confident that the extrapolation is sound.

Organic aerosol may be emitted as primary particulate, or form later as secondary aerosol. Both primary and secondary organic aerosol may be absorbing. In our analysis we assume that the refractory high temperature organics (>420oC) are the non-BC species contributing most to light absorption. The AMS filament aboard the P-3B operates at 600oC thus there are some organics detected by the AMS which may be 'refractory' according to the size distribution measurements. Therefore it isn't surprising that AMS organics do, in general, trend with refractory mass measured by the size distributions. However, these relations vary considerably, vary with AMS calibrations, and even among airmasses with similar sources, e.g. BB. This is likely due to the fact that the mass of organics that are refractory between 420 and 600oC are only a fraction of the total organics, primary or secondary, absorbing or not, found in each sample.

Finally, the AMS teams report the concentration of all volatile organics (~<600oC). Since we know that some, if not most, of these volatile organics are non-absorbing, we can expect, a-priori, that using AMS organic mass would imply a much lower MAE's. In other words we are effectively diluting the regression with what we know to be non-absorbing mass. This type of analysis was investigated and, as predicted the slopes are low and the R^2 statistics are very poor. Conversely, using our assumption, that no volatile organics (<420oC) are absorbing, means that our MAE's may be somewhat higher than the true value.

Thus using AMS organics we tend to suppress the importance of BrC absorption, whereas our method tends to emphasize its importance. Still BrC absorption remains a smaller fraction of the total absorption compared to absorption by BC. Since our values compare favourably with an independent estimate (Yang et al.) we are confident that this analysis is the best estimate possible of the true value.

2. There is a lot of discussion of dust and its absorption properties in this paper, but I can not find anywhere where dust is defined using anything but simply particle size (submicron vs supermicron). How do we know your “dust” is really dust? And specifically, the ubiquity of the dust mentioned on lines 636-637 is not evident from this paper. Is dust a general feature of the Western Arctic atmosphere or was 2008 anomolous? On line 658, indicate in the conclusions the method(s) used to identify

Asian dust.
For some analysis (e.g. dust MAE) the non-dust cases could be eliminated based on absorption angstrom exponent. However, the vertical profiles of dust mass originally were calculated using size distributions and therefore did not distinguish between other potential types of coarse mode particle. This has been amended by separating the supermicrometer particle number into seasalt and dust particle types using the PALMS instrument onboard the NOAA WP-3D. The supermicrometer volume is then recomputed and appropriate densities applied to convert volume to mass. The text also includes discussion of this procedure.

3. Conclusions and Figure 13 – As you mention at the end of the conclusions, the results shown in this figure are not necessarily representative of the Western Arctic in general. That also needs to be indicated in the figure caption and some of the wording in the earlier part of the conclusions needs to be modified. - Line 637 – ": : :absorption by dust accounted for at most: : :" - Line 647 – "accounted: : :" - Line 651 – You need to express the comparison of the various properties measured on the different aircraft in a more quantitative manner. - Lines 665-667 – "aerosol absorption WAS dominated : : : burning during April 2008."

4. There is no mention of the effects of particle sizing on absorption and scattering. Is this not important?
Sizing is important, but the size distribution and PSAP instruments have insufficient precision to clearly resolve these types of differences without more detailed analysis. This is beyond the scope of this paper.

5. Section 2.0 – The instrumentation discussion is not very informative. Referring to "teams" is insufficient. At very least the main instruments used to derive the results in this paper need to be briefly described along with their calibrations and uncertainties. At a few points in the paper, we are given hints about these things. The main instruments (the SP2s, the PSAPs AND the methods of separation of particles between submicron and supermicron sizes) need to be discussed in section 2.
The 'teams' mentioned here are leaders in their respective fields and their work is widely cited. Discussions of the particulars for each of the instruments, e.g. calibrations, their uncertainties etc. can be found in each of the publications cited. To include this information for each of the instruments would add unnecessary length to this summary manuscript. Where relevant, limitations and uncertainties are discussed when the results are being analyzed. We have restated the PSAP noise limit (0.5 Mm-1 for 240-300 second sample) in Section 2.

Minor comments:

6. Line 50 – by all accounts, the Arctic has begun to experience a profound transformation.
Amended: The cryosphere, perhaps more than any other domain, is experiencing a profound, perhaps millennia-long [*Solomon et al.*, 2009], transformation that is in part a result of anthropogenic induced climate change [*ACIA*, 2004; *IPCC*, 2007].``

7. Line 64 - Please elaborate on what you mean by "accumulation". Where does the aerosol come into and leave the Arctic and in what manner does it accumulate? I can not find where the Shaw reference discusses this.

Indeed, this was the wrong Shaw reference. It has been corrected to:
Shaw, G. E. (1995), The Arctic Haze Phenomenon, *Bulletin of the American Meteorological Society, 76*(12), 2403-2414.

8. Line 144-145 and line 176– Should have a reference for the Alert measurements: either Sharma, S., E. Andrews, L. A. Barrie, J. A. Ogren and D. Lavoué, Variations and Sources of the equivalent black carbon in the high Arctic revealed by long-term Observations at Alert and Barrow: 1989-2003, J. Geophys. Res., Vol. 111, D14208, Doi:1029/2005JD006581, 2006 –OR- Gong, S. L., T. L. Zhao, S. Sharma, D. Toom-Sauntry, D. Lavoué, X. B. Zhang, W. R. Leaitch, and L. A. Barrie, Identification of trends and interannual variability of sulfate and black carbon in the Canadian High Arctic: 1981–2007, J. Geophys. Res., 115, D07305, doi:10.1029/2009JD012943, 2010.
Added both as appropriate.

9. Lines 176-178 – There is no dispute of this statement. However, you must recognize that the aerosol up to 8 km over Alert may have different source regions than that over Barrow. In particular, did the biomass burning plume extend over Alert? Ground measurements may not be so good for representing impacts, but they do reflect emissions trends and in such remote regions they must reflect somewhat the character of the aerosol in the lower troposphere.
Agreed, the data from IPY suggest relative contributions from each of the major source regions (East Asia, Eurasia and N. America) are different at different locations.
Reworded to " Thus while surface based observations provide time series needed to document trends in emissions and their transport to the Arctic, these measurements cannot, in general, be extrapolated to derive column burdens of BC."

10. Figure 3 - We might expect relatively more CO and less BC in BB plumes as suggested by the DC8 and WP3D observations, but based on the AMS plot the P3B observations seem to be opposite. Can you explain?

Not sure I agree that P3B observation seem opposite. The BB plumes in Figure 3 are those coloured red using either ACN or log10(AMS Org:SO4). Subtracting a background CO value of 100 ppbv, the DC-8 and WP-3D slopes for ~BB plumes are:

500/(200-100) = 5

to

1500/(400-100) = 5

The only cluster of strong BB plumes for the P-3B is at 1000/(300-100) = 5. This seems consistent unless I have misunderstood the reviewers comment. If the reviewer is referring to the cluster of DC-8 and P-3B data with BC ~ 500 ng sm-3 and CO ~300 pbv, this cluster of data could be explained by removal of primary particulate near the source, with subsequent condensation of sulfates etc. during long-range transport. It is helpful to note that some of the data in this cluster were obtained during a DC-8/P-3B comparison flight, so the data are consistent between the aircraft in this rather dense urban/industrial plume.

11. Line 232 – “it” rather than “is”.

12. Line 240 - due to instrument malfunctions, zeroes or something else?

13. Line 314 – Reference for intensive aerosol properties?
Added Anderson et al., 1999 and Haywood and Shine, 1995

14. Line 346 – discuss PSAP detection limit in section 2.
Amended

15. Line 348 – You should discuss your FFscat threshold of 0.6 in terms of Figure 10. From Figure 10, it looks like the mean contributions from coarse particles are about 25%. How important is this?
Have now updated Figure 10 to include the mean and median profiles of FMFscat for each of these campaigns and added the appropriate discussion.

16. Lines 372-382 - Can there not be significant brown carbon that will volatilize below 420oC? e.g. Andreae and Gelencser, ACP, 2006.
Added this reference and clarified discussion

17. Lines 390-397 – Your screening does not completely remove coarse dust either.
Also discussed

18. One lines 443-445, you say that Kondo e tal (2009) attributed a factor of 1.8 to filter matrix effects. One lines 461-464, you state that Kondo et al (2009) found offsetting effects resulting in no enhancement. Please clarify this discussion.

19. Lines 491-492 – Reference for this ubiquity?
This is an observation based on ARCTAS/ARCPAC

20. Lines 492-495 – “after multiplying the integrated supermicrometer volume size distributions by...”
amended

21. Lines 498-500 - In Figure 9, the legend says P-3B not WP-3D.
22. Line 500 – “These data are: : :”
Sentence required some clarification, both P-3B (red) and WP-3D (blue) data are presented in the Fig. 9 panel. Sentence reworded to: " The effect of this plume on the mean value of dust mass at that altitude for the WP-3D is shown in figure 9 (blue) as compared to the mean P-3B data (red). "

23. Line 502 – Perhaps panel rather than column, here and elsewhere. Column implies text.
searched and replaced

24. Lines 508-511 - In fig 9, why do you have mass for the P-3B and for the WP-3D

but extinction for the DC8? This discussion needs to be clearer. Also, the inlet to the aircraft is characterized for coarse particles, but with what efficiencies are the coarse particles getting into and being detected by the nephs from the manifold?

We include extinction for the DC-8 and P-3B, mass for the P-3B and WP-3D, the measurements available in the database. Efficiencies for the DC-8 and P-3B include the inlet and plumbing to the instruments.

25. Line 522 – FT is undefined.
amended

26. Lines 527 and 528 – intex-Na?

27. Line 552 – LAC is undefined.
amended

28. Lines 553-560 – The paper is mostly about measurements from the PSAPs. The noise levels need to be discussed in section 2.0.
restated noise levels from Anderson et al., 2004 and McNaughton et al., 2009 in Section 2.

29. Lines 588-589 – "the mean and median MASS concentration: : :". How do we know this is all dust? And how do we know this is all Asian dust?
Updated figure 12 and PALMS analysis resolves this important issue.

30. Line 591 – "non-BC refractory mass" - do you mean BrC rather than "(BrC+BC)"?
Yes

31. Line 608 – 'Natural' dust?
amended to: 'mineral dust'

32. Lines 607-617 – These results would be clearer if they were presented in a table.

Two tables now included summarizing optics and aerosol mass.