We would like to thank the anonymous reviewers for the overall positive and insightful comments on the manuscript. The original comments are in italics and the response to each comment is directly below the comment. We will submit a revised version of the manuscript and figures with the changes outlined below.

Response to review 1:

Page 24807, Line 15 and Line 20. need "e.g." before Boucher et al.,

We have included "e.g." as requested.

Page 24808, Lines 5. This description is a bit simplistic with regards measurement of free tropospheric air at mountain sites. For example, synoptic weather types have influence, as described in Collaud Coen et al. (2011). http://www.atmos-chemphys.net/11/5931/2011/acp-11-5931-2011.pdf

We have altered to text to read "One important characteristic about measurements at high-elevation mountain surface sites is that there are periods which they can be used to investigate and understand FT aerosols.", and we have added the following sentence, "Synoptic meteorology, including advection and subsidence, influence the particles observed at mountain sites (Collaud Coen et al., 2011); however, one may expect chemical transport models to resolve these processes if synoptic meteorology is well-represented."

Page 24809, Line 19. Lat and Long repeated here.

We have removed the elevation, latitude and longitude from the sentence due to duplication.

Page 24810, Line 2. "Whistler Peak often resides in the lower FT" requires a citation or analysis. Please see comment above and Collaud Coen et al. (2011) analysis as an example of the complexities of defining free troposphere and boundary layer influence at mountain sites.

We now cite Gallagher et al. (2011) to support this statement. We agree that there are many complexities involved with the definition of tropospheric or boundary layer influence. As stated above, we have included the following text to the introduction to acknowledge these concerns: "Synoptic meteorology, including advection and subsidence, influence the particles observed at mountain sites (Collaud Coen et al., 2011); however, one may expect chemical transport models to resolve these processes if synoptic meteorology is well-represented."

Page 24811, Line 25. Again, further analysis or references are needed to justify statement that Whistler Peak is only influenced by the boundary layer in the summer.

This sentence has been replaced in response to a comment from the other reviewer, and we no longer mention this as it now reads, "While GEOS-Chem does have vertical mixing for the resolved BL and synoptic/convective mixing between the BL and FT, it does not resolve sub-grid vertical transport due to topographic and upslope flows."

Page 24814. In reference to the threshold temperature, did you consider calculating potential temperature? It may be a more robust measurement of B.L. influence. I am concerned this method is a *simplistic fit to this specific dataset and may not apply to other years considering interannual variability. Also, this may not simply be tuned to other sites (due to complexity or lack of upslope flow). As shown in Table 3, a wide range of temperature provides very similar results (Rˆ2 and m). You may also want to calculate water vapor (using temp and RH data) and use this a proxy.*

We considered using potential temperature as a proxy for boundary layer influence, but given the small range in pressure, potential temperature will scale nearly linearly with temperature and thus proxies using temperature and potential temperature will be nearly identical. We agree that there may be interannual variability in the analysis, therefore we have added the following comment to the manuscript: "As temperature is a simple proxy for boundary-layer influence, interannual variability in synoptic conditions at Whistler Peak beyond the measurement period in this study may lead to variability in an ideal threshold temperature between years." As for using water vapor as a proxy, Gallagher et al. (2011) found that CN was a more robust indicator of boundary layer influence at Whistler. We have the statement, "Though some of the studies discussed above used water vapor, we used CN because Gallagher (2010) found that CN was a more robust indicator of BL influence at Whistler."

Page 24816. The reasoning for considering CN to be an indicator of B.L. requires further description, given the frequency of new particle formation observed at Whistler.

We have added the following sentences, "Also, Whistler frequently observes new-particle formation events. Gallagher et al., (2011) estimated that the new-particle formation at Whistler was generally correlated with upslope flow and BL air. However, it is likely that not all new-particle formation events are associated with BL air and thus would contribute error to using CN as a classification of BL air."

Page 24821, line 15. Yes, temperature was a better proxy than "others proxies used previously" in your study. But the only other one tried was CN. Please see the other suggestions above for other proxies.

We have changed the sentence to say, "We found that using the measured temperature at Whistler Peak as a proxy for upslope flow, we could improve our agreement with measurements, and that temperature was a better proxy than a CN proxy that had been previously used as a proxy for BL air at Whistler (Gallagher et al., 2011), although it is possible that better proxies exist."

Page 24822, line 22. These conclusions regarding the impact of BVOC on SOA production at Whistler should be more carefully stated given that only 2 days of backtrajectories are provided within this paper.

The text has been changed to the following: "Based on this back trajectory analysis, it is hypothesized that this possible source of SOA could be a large source of condensable material, which could increase particle growth and hence increase N80."