Response to Referees for "Simulations of a cold –air pool associated with elevated wintertime ozone in the Uintah Basin, Utah" by E.M. Neemann et al. 2014

Reviewer comments are in italics. Responses are underlined.

Anonymous Referee #1
Received and published: 9 July 2014

General Comments:

This manuscript (i.e., Neemann et al., 2014) investigated a 6-day cold-air pool episode associated with elevated wintertime ozone in Utah using WRF-CMAQ simulations. This work is important to understand the elevated wintertime ozone episodes. The main weakness of this paper is lack of analysis of vertical profiles of chemical species. Thus I recommend revision before it can be accepted for publication.

Author Response: The authors thank Referee 1 for the thorough and insightful review. We have responded to each comment below. We summarize the changes made to the document below:

Specific Comments:

Given the boundary layer vertical structure playing a critical role for ozone accumulation for the selected episode, vertical profiles of chemical species (including O3 precursors and O3) need to be presented. For example, such profiles need to be added into the current Fig. 6.

Author Response: We agree with both referee 1 and 2 that the ozone, VOC, and NOx profiles are very important (and would be useful to show in Fig. 6). However, the work presented here was conducted primarily to improve the meteorological modeling of these CAP episodes in the Uintah Basin, with the preliminary CMAQ O_3 simulations presented to illustrate the impact of the meteorology on the general spatio-temporal ozone evolution. This study is part of a special issue that will include the aforementioned vertical profiles of chemical species in other papers within the special issue, for example, vertical profiles pertaining to the episode we modeled are included by Oltmans et al. (2014). Simulations using the CMAQ and WRF-Chem modeling frameworks to model ozone are an area of active research by many UBWOS scientists. Other scientists will be presenting detailed analyses of the vertical profiles of ozone and various chemical species that those groups collected over both the 2011-2013 and 2013-2014 winters.

Cross section of O3 in Figure 15 might be better to put along with vertical cross section of potential temperature in Figure 12. By doing so, the readers can immediately see the impact of cold pool on O3 accumulation.

Author Response: We liked this idea and aredid this figure with ozone data shown. Unfortunately, the cross-section of ozone at the time of the CAP tilting described in this figure did not effectively illustrate the impact of CAP on ozone accumulation since it was 11 pm MST.

Also the authors might consider adding O3 contours in Figure 10, similar as the current Fig. 16c. probably remove the current black lines in Figure 10 since color shade is enough.

Author Response: Agreed. We have added O_3 contours as well as cloud water and ice content in response to reviewer 3 to Fig. 10 (which is now Fig 9 in the revised manuscript) and removed most of the black lines (left thin ones at 5 K intervals to help highlight the theta contours) as suggested. Doing this allowed presentation of O_3 for the NONE case in addition to the FULL case. We did not compute ozone for the BASE simulation and therefore Fig. 9a does not show ozone. In addition, by compressing the information from Fig 16c into Fig 9b, we have removed Fig. 16c from the paper.

"Liquid-phase low stratus and fog are represented by aqua/green colours (e.g., southern ID and portions of western and central UT) while the yellow/orange colours evident in the basin are typically associated with ice-phase stratus and fog". Such information needs to be given using a color bar in the Figure.

Author Response: We have added a colored-coordinated legend that correspond to pertinent features in Fig 5a and b.

AGL and above sea level are mixed in the text, e.g., "lowest 300m is capped by increasing potential temperature on 4 February below 1800m (Fig. 6a and c). The strong stability extends upwards to 2750m".

Author Response: The text is changed to reflect that all heights are in MSL unless specifically referenced as AGL. AGL references are only used in 4 instances where it is necessary to convey near-surface vertical extent.

"The mixed layer is shallower on the 5th (Fig. 6b) with lower relative humidity within the CAP". I don't read this in Fig. 6. Relative humidity exceeded 100% on both 4th and 5th

Author Response: We agree this text was misleading and have clarified the text to point out the slightly lower RH in the upper portions of the boundary-layer CAP, which indicates the lack of cloud cover on that day: "The observed mixed layer was shallower the following day as a result of thinner surface ice fog (Fig. 4b) with ~10% lower relative humidity within the upper portions of the CAP."

"Data collected from ozonesondes and tethersondes during February 2013show that the vertical extent of maximum ozone concentrations was typically limited to 1700m m.s.l. and below, or in the lowest 200–300m of the boundary layer (Schnell et al.,2014). A gradient in concentrations was noted above this level, with ozone concentrations returning to background 03 levels above 1900m m.s.l. (Karion et al., 2014)." Such vertical structure of O3 needs to be presented along with vertical profiles of meteorological variables.

Author Response As discussed earlier, we agree that vertical structure of ozone is important. However, these profiles during the CAP event modelled in this paper will be presented in other papers within the ACP special issue (e.g. Oltmans et al. 2014), such that this information will be readily accessible for intercomparison with our model results.

Table 2 is confusing. How many simulations are conducted? 3? 4?

Author Response: We agree that Table 2 was confusing and we have modified it in response. Only 3 simulations were conducted and we have simplified Table 2 so that the FULL simulation is no longer duplicated in the Table.

"Most schemes generally allow too much turbulent mixing, which results in boundary layers that are too deep (Holtslag et al., 2013). While the MYJ PBL scheme was ultimately selected for this study, the Asymmetric Convective Model, Grenier-Bretherton-McCaa, and Bretherton-Park PBL schemes were also tested in addition to the Yonsei University (YSU) scheme with and without the Jimenez surface layer formulation and updated stability functions (Jimenez et al., 2012). The MYJ was chosen since it best represented the combination of moisture, stability, and temperature". This gives an impression that all the PBL schemes except MYJ give too much turbulent mixing, which might not be true [Hu et al., 2013].

Author Response: We have modified the text so that it does not give the impression that all PBL schemes except MYJ give too much turbulent mixing. Our understanding is that generally all schemes including MYJ are somewhat overdispersive in stable PBL situations (Baklanov et al. 2011, Holtslag et al, 2014, etc). We have modified the above statement to the following (and also added the useful information found in the Hu et al. 2013 reference):

"PBL schemes have difficulties handling low clouds, vertical temperature profiles, 2 m temperatures, and mixing in stably stratified conditions (Reeves et al., 2011; Shin and Hong, 2011; Zhang et al., 2013). Many schemes typically allow too much turbulent mixing in stable conditions, which results in boundary layers that are too deep (Holtslag et al., 2013). While the MYJ PBL scheme was ultimately selected for this study, the Asymmetric Convective Model, Grenier-Bretherton-McCaa, and Bretherton-Park PBL schemes were also tested in addition to the Yonsei University (YSU) scheme with and without the Jimenez surface layer formulation and updated stability functions (Jimenez et al., 2012). The MYJ was chosen because in simulating this particular CAP it best represented the combination of moisture, stability, and temperature characteristics that were observed in the Uintah Basin for the simulated period. Further testing of PBL schemes performance in the presence of high wind speeds above the stable PBL are also needed (e.g., Hu et al. 2013 found that modifications to the YSU nighttime velocity scale improved the simulations). "

Zhang et al. [2013] is more appropriate for the statement of "PBL schemes have difficulties handling 2m temperatures, and mixing in stably stratified conditions"

Author Response: We have added this reference.

"The warm air aloft (700hPa temperatures between -7 and 0°C) overtopping

very cold low-level air (diurnally ranging between -18 and -5°C) resulted in a strong capping inversion within the basin." This is an important mechanism for the confined cool pool formation [Lu and Zhong, 2014]. It is better to show such information in a figure (i.e., vertical profile of temperature)

Author Response: We have clarified this description and now introduce Fig. 4 earlier (sounding profiles) in our discussion of the cold pool evolution to give a Figure to go with the description.

Figures need improvement, e.g., Figure 2.

Author response We have worked to improve most of the Figures (2 Figures were removed from the paper during the revision process as requested by reviewers). A summary of changes to the Fig. is given below:

All Figures: Tick marks have been changed to 00:00 MST (midnight).

Fig 2. Rescaled so that (a) and (b) were the same size. Added red box in (a) that corresponds to region in (b). Added legend text and scale to (a) and (b)

Fig. 3. Added legend text (surface albedo and snow depth). Increased size of a-c lettering and increased size of image to improve print quality.

Fig. 4. Increased size of font on vertical and horizontal axes.

Fig. 5. Included legend for satellite images as requested.

Figs. 7 and 8. Added legend text (Temperature, Longwave Difference and 2-m Temperature Difference).

Fig. 9. We added 03 contours to Fig 9b and c in the revised manuscript and removed most of the black lines associated with potential temperature (left a few thin ones at 5 K intervals to help highlight the theta contours).

Fig. 11. We added legend text (Wind Speed, etc) and added Another time to show changes in CAP depth on the western slope.

Fig. 13. We added color-scale to Fig. 13a as requested by reviewer 2 to make spatial ozone variations more visually obvious.

Fig. 14. We added legend text (ozone) and compressed the old Fig 16a and b into Fig 14e and f

Author Response: The Hu et al. 2013 and Zhang et al. 2013 papers were added to the reference list

Hu, X. M., P. M. Klein, and M. Xue (2013), Evaluation of the updated YSU planetary

boundary layer scheme within WRF for wind resource and air quality assessments, J Geophys Res-Atmos, 118(18), 10490-10505, doi 10.1002/Jgrd.50823.
Lu, W., and S. Zhong (2014), A numerical study of a persistent cold air pool episode in the Salt Lake Valley, Utah, Journal of Geophysical Research: Atmospheres, 119(4), 2013JD020410, 10.1002/2013JD020410.
Zhang, H. L., Z. X. Pu, and X. B. Zhang (2013), Examination of Errors in Near-Surface Temperature and Wind from WRF Numerical Simulations in Regions of Complex Terrain, Weather Forecast, 28(3), 893-914, Doi 10.1175/Waf-D-12-00109.1.

Author Response: we have also added the following new references regarding UBWOS work that were recently submitted to ACPD Uintah Basin Special Issue:

Lee, L., Wooldridge, P. J., Gilman, J. B., Warneke, C., de Gouw, J., and Cohen, R. C.: Low temperatures enhance organic nitrate formation: evidence from observations in the 2012 Uintah Basin Winter Ozone Study, Atmos. Chem. Phys. Discuss., 14, 17401-17438, doi:10.5194/acpd-14-17401-2014, 2014.

Li, R., Warneke, C., Graus, M., Field, R., Geiger, F., Veres, P. R., Soltis, J., Li, S.-M., Murphy, S. M., Sweeney, C., Pétron, G., Roberts, J. M., and de Gouw, J.: Measurements of hydrogen sulfide (H_2S) using PTR-MS: calibration, humidity dependence, inter-comparison and results from field studies in an oil and gas production region, Atmos. Meas. Tech. Discuss., 7, 6205-6243, doi:10.5194/amtd-7-6205-2014, 2014.

Warneke, C., Veres, P. R., Murphy, S. M., Soltis, J., Field, R. A., Graus, M. G., Koss, A., Li, S.-M., Li, R., Yuan, B., Roberts, J. M., and de Gouw, J. A.: PTR-QMS vs. PTR-TOF comparison in a region with oil and natural gas extraction industry in the Uintah Basin in 2013, Atmos. Meas. Tech. Discuss., 7, 6565-6593, doi:10.5194/amtd-7-6565-2014, 2014.

Oltmans, S. J., Karion, A., Schnell, R. C., Pétron, G., Sweeney, C., Wolter, S., Neff, D., Montzka, S. A., Miller, B. R., Helmig, D., Johnson, B. J., and Hueber, J.: A high ozone episode in winter 2013 in the Uinta Basin oil and gas region characterized by aircraft measurements, Atmos. Chem. Phys. Discuss., 14, 20117-20157, doi:10.5194/acpd-14-20117-2014, 2014.

Ahmadov, R., McKeen, S., Trainer, M., Banta, R., Brewer, A., Brown, S., Edwards, P. M., de Gouw, J. A., Frost, G. J., Gilman, J., Helmig, D., Johnson, B., Karion, A., Koss, A., Langford, A., Lerner, B., Olson, J., Oltmans, S., Peischl, J., Pétron, G., Pichugina, Y., Roberts, J. M., Ryerson, T., Schnell, R., Senff, C., Sweeney, C., Thompson, C., Veres, P., Warneke, C., Wild, R., Williams, E. J., Yuan, B., and Zamora, R.: Understanding high wintertime ozone pollution events in an oil and natural gas producing region of the western US, Atmos. Chem. Phys. Discuss., 14, 20295-20343, doi:10.5194/acpd-14-20295-2014, 2014.

Response to Referees for "Simulations of a cold –air pool associated with elevated wintertime ozone in the Uintah Basin, Utah" by E.M. Neemann et al. 2014

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Anonymous Referee #2
Received and published: 14 July 2014

General Comments

The authors present a comprehensive case study that illustrates the importance of boundary layer dynamics and snow cover to the occurrence of the unusually high ozone concentrations observed occasionally in the fracking area in the Uintah Basin, Utah. Observations are compared to WRF and CMAQ model runs, and several deficiencies in the models are identified. The manuscript is comprehensive and only minor modifications are required.

Author Response: The authors thank referee 2 for the helpful review. We have attempted to address all of the referee's concerns below.

Specific Comments
1. Please stick with MST throughout the manuscript; at the moment both UTC and MST are used.

Author Response: As suggested, we have changed all times within the paper text and figures to MST

2. Stick with either a.g.l. or m.s.l. throughout; using them interchangeably is confusing. I would recommend a.g.l.

Author Response: As suggested, we we have modified the text to reflect all heights are in MSL unless specifically referenced as AGL. AGL references are only used in 4 instances where it is necessary to convey near-surface vertical extent. Because locations are at various heights above sea level, we are unable to use a.g.l. in many situations.

3. page 15956, Line 3: "... of the atmosphere (Fig. 1)". Eliminate "A schematic of this...". There are a few other places where figures can be introduced more succinctly (e.g. p.15962, Figs. 6 and 7); there is no need to duplicate any of the information that is in the caption.

Author Response: As suggested, we have changed the text in introduction of Figs. 1, 6, and 7 to be more succinct and to remove duplicative information.

We entirely removed the sentence: 'A schematic of this typical set-up is shown in Fig. 1).

We changed this sentence “Since the UDAQ inventory and CMAQ model are available at a resolution of 4 km, that model was forced with WRF data from the 4 km nest shown in Fig. 2a” to this: “Since the UDAQ inventory and CMAQ model are available at a resolution of 4 km, that model was forced with WRF data from the 4 km nest (Fig. 2a)”.

We have removed this sentence: “Vertical profiles of potential temperature, relative humidity, and wind at Roosevelt from rawinsondes released at midday (18:00 UTC) on 4 and 5 February 2013 are shown in Fig. 6.”

We have removed this sentence: “Figure 6a presents the time evolution of surface ozone at selected locations in the basin.”

4. Sixteen is a large number of figures... I think the manuscript can do without Fig. 3 (it’s all explained in the text), and without Fig. 5. If you want to keep Fig. 5 in, please add a color scale, and use MST in the caption.

Author Response: As suggested, we have removed Fig 3 and have compressed Fig. 16 (which duplicated some of Fig 9) into Fig. 14. Thus, the total Fig. number has been reduced from 16 to 14. We have also added a colored-coordinated legend that correspond to pertinent features in Fig 5a and b.

5. Section 3.5.1: the tilting/sloshing of the CAP is very interesting... are there any surface observations (ideally near Starvation Reservoir) to corroborate this with real data?

Author Response: Yes, surface observations of the intrusion and retreat of warmer westerly flow exists at the Starvation Reservoir and other weather stations available at mesowest.utah.edu. We hope to discuss these behaviors in more detail in future work but in the interest of space do not discuss them in this paper. We now do include in Fig. 11 two snapshots of the CAP structure illustrating the tilting/sloshing that was previously a ‘not shown.’

6. Fig. 6 can be improved by including ozonesonde profiles (if available around Feb. 4 or 5), or at least ozone profiles from CMAQ. If showing CMAQ O3 profiles, you could also include VOC and NOx profiles.

Author Response: We agree with both referee 1 and 2 that ozone, VOC, and NOx profiles would be useful to show in Fig. 6. However, our work in the larger UBOS study is focused on accurately modeling the meteorological aspects of these wintertime inversions, and other scientists will be presenting detailed analyses of the vertical profiles of ozone and various chemical species that those research groups collected in recent winters . Therefore, it would be inappropriate for us to present these data here.

7. The time scale in Fig. 7, 10 and 16 is unclear. Does the tick denote the middle (noon) of the day? MST? That appears more likely than the tick denoting midnight, given the diurnal O3 peak just after the tick. This would be rather unusual and a tick at 0:00 MST (midnight) would be preferred. Whatever you choose should be stated *explicitly in the captions.*

Author Response: Thank you for the clarification. All Figures: Tick marks have been changed to 00:00 MST (midnight) and the figure captions now explicitly state this.

8. Instead of (or in addition to) the ozone mixing ratios being given in the little boxes in Fig. 14(a), coloring the line or boxes and providing a color code would clarify the ozone distribution on the transect.

Author Response: We have modified Fig. 14 (now Fig. 13) to include a color scale for ozone to clarify the ozone distribution on the transect.

9. I would recommend rephrasing your conclusion that CMAQ does an "adequate" job near the sources; that is a judgment call with which some may disagree. A safer statement would be to give a percent difference between model and reality.

Author Response: We agree with the referee statement and have modified our conclusions and also clarified the sentence in response to reviewer 3. We no longer give a 'judgment call' between model and reality, but simply state the fact that the model and observations tend to be closer near the source regions than away from those regions

New text: "CMAQ model-derived estimates of ozone concentrations agree better with observations (1) during the daytime than during the nighttime and (2) near the highly dense precursor emission sources located in the southeast quadrant of the basin (Fig. 14)."

Response to Referees for "Simulations of a cold –air pool associated with elevated wintertime ozone in the Uintah Basin, Utah" by E.M. Neemann et al. 2014

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Anonymous Referee #3

Received and published: 24 July 2014

Comments: This paper is a model sensitivity study testing the effects of model snow cover and model ice microphysics on the formation and structure of a persistent cold-air pool in a wide mountain basin. The paper is clearly written and contains novel material worthy of publication. I recommend publication after major revision to clarify what this paper shows. An underlying question for any such numerical study is, can mesoscale NWP models reproduce realistic shallow cold pools at all, and if they do, is it 'for the right reasons,' given the well known limitations of these models–several of which are mentioned in this paper, such as excessive diffusion, warm biases at the surface, unrealistically warm, deep daytime boundary layers, etc. In the model runs presented here a cold pool is formed when fog is present, and the cold pool more resembles the measurements used for comparison, when the ice microphysics are modified to allow persistent ice fog. A reader may take from this that this study shows that ice fog is a necessary condition for strong cold pool–and thus high O3–formation in nature. I do not believe this is true, nor that these 3 model runs demonstrate this. I am recommending that the authors carefully reword their findings and conclusions to be clear about what is vs. what is not demonstrated as to the role of the ice fog.
A considerable amount of profile and other measured data is available in the central and eastern portions of the basin, which was largely not used to verify the model results, such as wind, temperature, and some chemical-species profiles. Since this is a model sensitivity study rather than a verification study, the limited measurements in the northwestern sector of the basin were presumably to establish that the model was giving the right ballpark. But a statement that there were other measurements available that were not used, and why they weren't, is still needed.

Author Response: We thank reviewer 3 for the thoughtful review, and we have clarified the intent of the study in terms of "*findings and conclusions to be clear about what is vs. what is not demonstrated as to the role of the ice fog*". We also elaborate on the role of clouds and the ability of the WRF model to capture the clouds. We also briefly discuss the reasons we did not do a more extensive validation. Further elaboration on these points is found below in the responses to the major comments.

Comment: A reader may take from this that this study shows that ice fog is a necessary condition for strong cold pool–and thus high O3–formation in nature.

Author Response: We have clarified the text in the paper to ensure that the reader is not unintentionally misled to believe that ice fog is a necessary condition for a strong cold pool. We agree completely with the reviewer--there have been several very strong cold pools during

UBWOS with very high ozone concentrations in which no ice fog was present. The intent of this paper regarding clouds is to point out that:

1. In this particular cold air pool episode (Feb 1-10 2013), ice fog played a small but notable role in CAP evolution in terms of modulating the incoming solar radiation and depth of boundary layer
2. Getting the WRF model to reproduce ice fog instead of liquid stratus improves model results

As such, we have made several modifications listed below to the text to make sure this is clear.

1. Abstract. We listed snow variations first followed by discussion of clouds since snow impacts are larger than cloud impacts
2. Section 3.3: We expanded the discussion of clouds in simulated vertical profiles and added cloud profiles to Fig. 9
3. Summary: We reworked the first two ‘key findings’ bullets listed below to make sure the points in the paper are clear that presence of snow has a large impact and clouds a lesser impact.

   1. The WRF mesoscale meteorological model was able to simulate ***a strong wintertime capping temperature inversion*** above the boundary-layer within the Uintah Basin ***irrespective of surface or cloud characteristics.***
   2. The CAP characteristics below ~ 2100 m (stable layer intensity, vertical structure, and boundary-layer flows) are heavily influenced by the presence of snow cover and are modulated to ***a lesser extent by the numerical treatment of cloud microphysics***.

4. We also added this statement to the discussion:
   “For example, in this study we did not investigate the impact of having no clouds on the CAP evolution, which may have resulted in greater radiational surface cooling, and even shallower mixed-layer heights than simulated with ice fog present.”

A considerable amount of profile and other measured data is available in the central and eastern portions of the basin, which was largely not used to verify the model results, such as wind, temperature, and some chemical-species profiles. Since this is a model sensitivity study rather than a verification study, the limited measurements in the northwestern sector of the basin were presumably to establish that the model was giving the right ballpark. But a statement that there were other measurements available that were not used, and why they weren’t, is still needed.

Author Response: With regards to the additional data available in the basin, we believe the data we used was sufficient to determine whether or not the model was grossly capturing the relevant features of the CAP. The goal of this limited study was to obtain a sufficient numerical representation of the CAP dynamics within the entire basin, and Roosevelt provided a more central location for a targeted validation. Second, the localized nature of the drainage flows observed at Horsepool (Robert Banta, personnel communication) made the wind observations at that location potentially less representative of the overall CAP flows than at Roosevelt. Finally, daily profiles of the synoptic flow above the CAP were only available from the Roosevelt Soundings. We agree, however, that benefits will come from further analysis of observational data and we have added the following text to the paper:

Section 2.1:
"Additional profiles of wind, temperature, and chemical species in the boundary-layer available in the east-central portion of the basin as part of UBWOS are being examined by other researchers (e.g., Oltmans et al. 2014). "

Summary and Discussion:
". Fourth, additional data (particularly with regard to chemical species) exists in the east-central part of the basin (e.g. at Horsepool and Ouray) that could be used to evaluate meteorological and chemical model performance."

Major comments:

1. Fog was not persistent in the basin during the 2013 experiment. It had a diurnal cycle, forming in the early-morning hours after midnight (sometimes even after sunrise). The fog dissipated in late morning or early afternoon, as noted in the paper. It was not observed to be prevalent during the early period of formation of the cold pool, therefore probably not a primary driver of cold-pool formation in this basin. Did the fog in model runs exhibit a diurnal cycle?

Author Response: Unfortunately, as discussed in the paper and mentioned by reviewer 3, all numerical models struggle with handling the generally thin, variable cloud layers that occur at different levels within cold air pools, and improving low cloud treatment in NWP remains an active area of research. The observed diurnal cycle of fog occurrence in the basin was not captured well by the WRF model. None of the simulations produced extensive fog at the onset of the CAP, thus reinforcing reviewer 3's statement that the fog was not a driver of cold pool formation. There was a modeled diurnal cycle, however, on most days in the simulations such that that cloud water was maximized at night in the BASE simulation and cloud ice was maximized at night in the FULL simulation (new Fig. 9). However, the FULL and BASE simulations kept clouds on Feb 2nd, 5th and 6th when they should not have. In this study we documented the overall improvement in CAP depth and temperature by changing from liquid to ice clouds (which were observed). We have added cloud water and cloud ice to Fig. 9 and an associated discussion to further illustrate the differences between the BASE, FULL, and NONE to describe the simulated cloud occurrence, level, thickness, etc. We have also clarified the text (to make sure it is clear that we do not believe the fog to be a driver of cold pool formation, only one of the many factors that needs to be considered in CAP evolution (see earlier discussion points 1-4). Our focus is on the cloud impact on 'modulating' the cold pool depth, incoming solar and outgoing longwave radiation, and possibly snow albedo and ultimately ozone concentrations to some extent. The simple modification we have introduced (snow albedo, vegetation effective depth, and cloud ice sedimentation rates) resulted in a simulation that was *improved* but by no means perfect.

Added/modified text 3.3 which describes diurnal cycle of modeled fog:

“Comparing the temporal evolution of the potential temperature, cloud water and ice profiles at Horsepool between the BASE and FULL simulations further illustrates the impact of cloud type on CAP thermodynamics (Fig. 9). The 1–3 ◦C colder surface temperatures noted in the FULL compared to the BASE simulation are associated with extensive ice fog that occurred in the FULL simulation between the surface and the bottom of the capping inversion (Fig. 9a and b). The base of the capping inversion (approximately represented by the ~280 K potential temperature isotherm in Fig. 9a-b and the ~290 K isotherm in Fig 9c) associated with the top of the stratus clouds in BASE (Fig. 9a) averages 100–200 m higher than the top of the ice fog simulated in FULL (Fig. 9b). The ice and liquid clouds simulated in FULL and BASE also have a diurnal cycle, with higher liquid and ice cloud amounts during the night than during the day, but the simulated cloud occurrences are overestimated in both simulations compared to ceilometer observations, particularly on the 5th and 6th of February (Figs 6b and 9b), However, the surface-based depth of the ice fog in FULL is more realistic than the deeper and elevated stratus cloud seen in BASE and the simulated vertical temperature profile in FULL also more closely matches available observations (e.g., Fig. 4a and b). “

2. On those nights when fog was present for several hours before sunrise, significant ice on the trees, fences, and other surfaces (the authors note the presence of hoar frost) was often evident. In the presence of the fog, we interpreted this as riming, which would indicate the presence of supercooled water in the fog - not completely iced out. The ice-RH soundings of some of the model runs (in Fig.6c,d) indicate supersaturation with respect to ice, but the atmospheric sounding does not seem to. The comment here is that the issue of whether the real fog contained all ice or supercooled liquid water is not settled, and this will have an impact on the radiative properties of the fog/cloud and their potential for cooling the near-surface air. The satellite product, which indicated an ice cloud, may have been responding to the tops of the cloud/fog??

Author Response:

We are fairly confident that the fog observed in the UB during 1-6 February was primarily ice fog for the following reasons:

1. The range of -10 to -15 °C is typically given as the temperature at which ice fog begins to play an important role (Gultepe et al. 2014), while between 0 °C and -10 °C super-cooled liquid droplets are generally observed. The surface temperatures within this UBOS CAP were generally well below -15 °C at night (as cold as -20 °C) and only briefly warmed up above -10 C during the afternoon for a few hours on some days. In addition, recent studies have shown that “*Ice fog can be significantly enhanced by higher aerosol concentrations* and increased vapor from anthropogenic sources.” (Schmitt et al. 2013). We hypothesize that the pollutants found within the Uintah Basin also likely enhanced the ice fog formation during this event.

2. Visually, the thin, sometimes almost transparent fogs (with the sun showing through at times) we observed at our observations site in Roosevelt fitted with observations of ice fogs (see photos on next page), with lots of delicate crystals that had fallen and collected on surfaces. We believe that in a ‘riming’ situation there would be more of a ‘plastering’ effect on objects versus the delicate crystals observed. In summary, we believe what occurred in much of the fog in the UB during this CAP was most likely ‘ice-fog induced frost’ which looks similar to riming but with a more crystalline make-up. Compare our photos from those of ice-fog induced frost from Gultepe et al. 2014. We believe the ice fog and light fresh snowfall falling out of these fogs is one of the reasons for the enhanced albedo we observed at Horsepool during this CAP.

3. We observed snow crystals falling out of the fog on most mornings and depositing on surfaces, windshield, etc, which meant that at least some ice crystals were being formed within the fog and falling out.

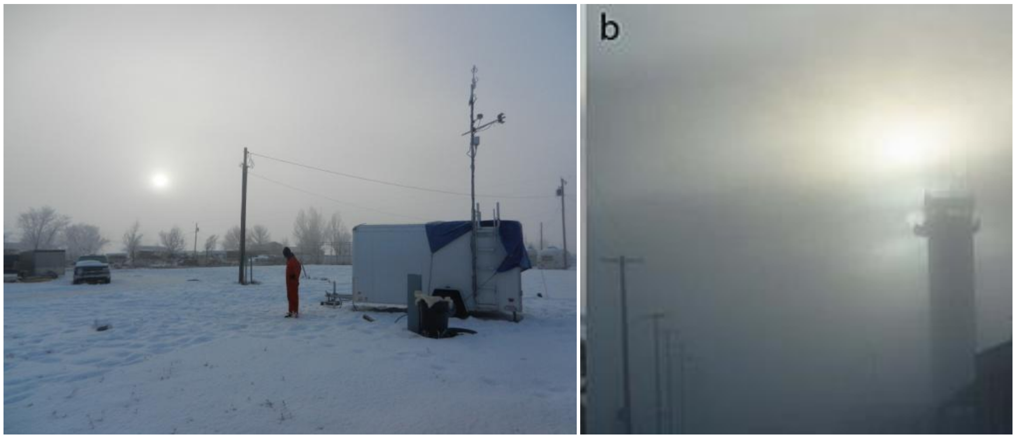
4. The daily soundings were launched near midday, when the fogs were typically tenuous and breaking up. Consequently, the soundings likely do not capture the supersaturation that was present several hours earlier.

5. While these ice fogs are most common in extremely cold locales like interior Alaska, there is a history of them occurring in the Western US during CAPs such as those in the Uintah Basin (e.g., ‘pogonip’ Native American word for ice fog http://www.carsonweather.com/modules/myalbum/photo.php?lid=184)

6. Since the fog layers were typically quite shallow (~100 -200 m deep) in Roosevelt, and with the coldest temperatures observed near the surface, the likelihood of ice fog should increase near the surface. Consequently, the satellite product should provide another verification that the clouds had significant ice content during this CAP.

It is also possible that supercooled water was also present during this CAP, particularly during the daytime hours before the fog dissipated, but we believe that for much of the lifecycle the fog was likely primarily composed of mostly ice.

Ice-fog induced frost UBOS 2 Feb 2013 (left) and from Gultepe et al. 2014 (right)



Photos of ice-fog induced UBOS 2 Feb 2013 (left) and from Schmidt et al. 2013 (right)

3. On p.-70, line 5, the authors refer to, "cloud water often present in the NONE simulation (not shown)." I believe this is the only mention of cloud/fog formation in the no-snow case. When did this form and what were its properties? During the previous year's campaign, the no-snow case in the basin was observed for the entire UBWOS-2012 experiment, and I believe it is well documented that little if any low cloud or fog was observed during that period, certainly none during the day. If the model was producing low cloud/fog in absence of snow cover, this would undermine the credibility of the results in an absolute sense, and make it more important to be clear about this being a model sensitivity study.

Author Response: The overall cloud amount and occurrence was somewhat reduced in the NONE experiment compared to FULL and BASE, but some thin stratus clouds were still present much of the simulation. As seen in Fig. 9c, the 2nd through 5th had partial cloud coverage. A general observation we and others who study CAPs is that they often struggle with low clouds, and it varies from CAP to CAP. Sometimes a CAP should have clouds and the model does not produce any, and other times it produces too many clouds. We hope to learn more about the model biases and how to fix them in future studies. We agree with the fact that very little low cloudiness was observed during 2012 winter. However, we have observed low clouds in the basin at other times when there was no snow on the ground, and although it is likely less common, there are situations where a strong capping inversion and clouds could exist over the basin without snow cover. We have added the following two sections of text to the paper to address this comment:

"...the sensitivity of the CAP to ice-phase microphysics is minimized in the NONE simulation since the boundary layer over the bare ground/vegetation is too warm (i.e., higher than −12 °C) to nucleate cloud ice. The resulting liquid-phase stratus in the NONE simulation leads to increased longwave radiation at the surface. Finally, the cloud thickness and occurrence in the warmer NONE boundary-layer is reduced compared to FULL, resulting in greater incoming short-wave radiation (Fig. 9c).

"The modelled afternoon mixed layer depths of 400-800 m in the NONE simulation were somewhat less than those observed in the basin during snow-free conditions the previous winter (Lyman and Shorthill 2013), and clouds formed within the NONE simulations whereas clouds were only infrequently observed in the Uintah Basin in winter 2011-2012 when snow cover was absent. However, a number of factors contribute to mixed layer depth and cloud occurrence, including the strength of the synoptic-scale capping inversion and boundary-layer relative humidity, so evaluating overall model simulation performance during snow-free conditions within the basin would require simulating periods when snow was not present, such as during the 2011-2012 winter. "

4. No runs were reported on with snow and without fog. This is why this study does not show that fog is necessary for cold pool formation. I don't believe that authors make this claim, but I think it is important to be clear about this point, since some readers may see this as a "take-away" point, as discussed above.

Author Response: This is an excellent point, and we have added the following sentence to the paper that refers to Neemann (2014). If model is configured in such a way that no clouds are allowed to form, then the resulting CAP is several °C colder than the ice fog case.

"For example, if no clouds are allowed to form during the CAP lifecycle, this results in shallower mixed-layer heights and a colder CAP by several °C than that simulated with ice fog present (Neemann 2014)."

Minor suggestions:

5. p.-66, line 23: Fig. 12 has no temporal information – the previous sentence refers to "large changes in depth within just a few hours," and Fig 12 is supposed to illustrate "this type of behavior."

Author Response: We have included a later time within the new Fig. 11b to show the sloshing behavior of the cold pool on the western side of the basin.

6. p. -67 line 6: "(not shown)" . why is this cross section not shown? It should be added to Fig. 12.

Author Response: Unfortunately, the cross-section of ozone at the time of the CAP tilting described in this figure did not effectively illustrate the impact of CAP on ozone accumulation. This is because it is nighttime and the CMAQ model significantly underestimated the nighttime ozone levels.

7. p. -68, first paragraph: Without further description and motivation, this paragraph doesn't make much sense.

Author Response: We have expanded the discussion in this paragraph to give more background information and to further hypothesize on some of the physical factors playing a role in the flow patterns seen. The following text has been added:

"Thermally-driven daytime upvalley/upslope and nighttime downvalley/downslope flows were observed within the basin by Lyman et al. (2013), while cross-basin elevated easterly flows 100-300 m a.g.l. (possibly associated with basin-scale thermal gradients) were observed in rawinsonde soundings between 1-6 February 2013 at Roosevelt (Fig. 4). Within the model simulations, it appears that both additive and destructive interactions between the cross-basin elevated easterly flows and near-surface daytime upvalley/upslope and nighttime downvalley/ downslope flows are occurring (Fig. 12). While basin-scale thermal gradients likely drive the elevated easterly flow, those gradients are at times in concert with and at other times interfering with more localized thermal gradients within drainages and along slopes. We hypothesize that the basin-scale thermal gradients are associated with either (1) elevated heating on the western slope of the basin, or (2) interactions between the westerly downslope flow, the cold air pool, and the mountain slope. "

8. p. -68, line 22: "removal of snow cover only affects the near surface " The previous-year's experience of UBWOS-2012 indicates that the afternoon mixed layer became quite deep–often 1.5 km or more over the basin If this was not so in the model runs, this should be noted.

Author Response: Agreed. We have added the following text to the paper:
"The modelled afternoon mixed layer depths of 400-800 m in the NONE simulation were somewhat less than those observed in the basin during snow-free conditions the previous winter (Lyman and Shorthill 2013), and clouds formed within the NONE simulations whereas clouds were only infrequently observed in the Uintah Basin in winter 2011-2012 when snow cover was absent. However, a number of factors contribute to mixed layer depth and cloud occurrence, including the strength of the synoptic-scale capping inversion and boundary-layer relative humidity, so evaluating overall model simulation performance during snow-free conditions within the basin would require simulating periods when snow was not present, such as during the 2011-2012 winter. "

9. p. -69, para 4.1: where did the mobile data come from? Attribution is needed here.

Author Response: We have attributed the data to the University of Utah in the figure caption.

10. p. -71, "CAP depth" discussion: the discussion seems to be about the mixed layer (or aerosol layer) depth, which is not the same as the CAP depth.

Author Response: We agree that it is difficult to describe the mixed layer and CAP depth distinctly and that we should be careful in interchanging the use of CAP and mixed-layer. The aerosol layer depth from the ceilometers often had pollution embedded in stable layers above the surface mixed layer, so it is unclear to us whether there might be ozone also within the CAP but above the mixed layer. For this paper, we have rewritten this discussion in an attempt to clarify and interchanged CAP with mixed-layer in most cases as this definition relates more closely to the ozone concentrations as we have seen in presentations from vertical profiles of O3.

11. p. -72, line 11-15: confusing – please rewrite.

Author Response: In response to this and reviewer 2 comments, we have rewritten/shortened this statement to the following:

"CMAQ model-derived estimates of ozone concentrations agree better with observations (1) during the daytime than during the nighttime and (2) near the highly dense precursor emission sources located in the southeast quadrant of the basin (Fig. 14)."

12. p. -72, lines 16-19: "two ways" – these concepts are not original to this paper, but have been noted as important factors since the first paper in this subject. This study provides support for those ideas, but I would not call them a "key finding of this study."

Author Response: We agree that this statement appeared to discount that these factors are already known, which was not our intention. We have re-worded that point to the following:

"The numerical simulations presented herein provide additional support to previous observational evidence that snow cover affects ozone concentrations by (1) cooling the near-surface layer and thereby strengthening the CAP and increasing stability further aloft, and (2) increasing the surface albedo and subsequent photolysis rates, contributing to rapid ozone production."

13: p. -74, line 10: "This study highlights the need for improvements in the representation of snow variables" Does it? The study shows that the presence of snow vs. no snow is important, but doesn't really show that a more sophisticated treatment of the snow would make any difference. Could be restated, "it would be interesting to see if" or similar.

Author Response: Good point. We did not present a strong argument in this paper about the impacts of changing the albedo/vegetation cover on the CAP simulation. However, the comparison of the snow vs. no snow case points out how important snow is, and in other simulations we did find some improvements when including a more realistic albedo value. For this paper, we have reworded this section to the following:

"The impacts of modifying the albedo from the default NAM initialization to that observed (an increase of 0.17, see Section 2.2) on the CAP meteorology were relatively small (not shown). However, larger impacts resulting from modest changes in snow albedo are likely to be observed within photochemical models (e.g., Ahmadov et al. 2014). Because of the spatio-temporal variability of snow depth and albedo within the Uintah Basin during winter seasons, the need for more sophisticated representation of snow variables in meteorological and air quality models in this region is apparent, and worthy of future research to better quantify the impact of these improvements on ozone simulations. Proper treatment of both the spatial extent of snow cover as well as the snow surface using a snow physics model driven by local atmospheric and chemical properties (e.g., the three-layer snow model within Noah Multi-Parameterization land surface model; Niu et al., 2011) may be needed to obtain a sufficiently accurate evolution of the snowpack and surface albedo. "