

Interactive comment on “Simulations of a cold-air pool associated with elevated wintertime ozone in the Uintah Basin, Utah” by E. M. Neemann et al.

Anonymous Referee #3

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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

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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.

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?
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

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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??

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.

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.

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.”

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

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

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

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quite deep—often 1.5 km or more over the basin If this was not so in the model runs, this should be noted.

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

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.

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

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.”

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 15953, 2014.

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