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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Characterization of the boundary layer at Dome C (East Antarctica) during the OPALE summer campaign" *by* H. Gallée et al.

Anonymous Referee #1

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General This is a comprehensive description of the application of the MAR model to the OPALE experimental period. The model suffers limitations as do other models in the polar regions of not accurately producing cloud structures (often of mixed phase nature) and the associated surface radiative balance. The authors document this well. Comparisons of wind speed and direction and friction velocity are quite reasonable. The model shows a cold bias in general at nighttime: the temperature in shallow stable layers may be important to the chemistry and a comment on its importance or lack thereof should be made. Only a single 3-day example of model boundary layer depth estimates compared with high resolution sodar data is shown. A critical missing piece in the paper is a detailed comparison between the model and sodar depth measurements for the entire period broken into stable and unstable periods, particularly for the early period when surface snow nitrate and associated fluxes were large. Documenting model performance during the collapse of the daytime convective layer is essential to understanding the ensuing chemistry where past research has indicated the possibility of non-linearity in the HOx-NOx chemical system. I have noted below that in the paper by Frey et al., they eliminate 22% of the NOx flux values (∼five hours per day on average) when the boundary layer depth is less than 10 m: This would eliminate a substantial portion of the evening transition chemistry. I also feel there was inadequate crosslinking to the other papers in this special issue: the authors could easily point out and reference how their model results are used. For example, Frey et al show the only period of NOx profiles on 9 January: the detailed behavior of the boundary layer in this period from the model (and sodar) perspective could be quite valuable. Another curiosity is the burst of NOx around 2300: Is this a boundary layer effect? Similarly, Kukui et al use a 1-D chemistry-transport box model to get the vertical distribution of HONO using the MAR boundary layer depth data: this is an example of the type of use that should be referenced in this paper and how the modeling effort should be an essential part of the OPALE collection of papers.

Specific

33091, lines 1-2: If “observation and modelling of the boundary layer has already been performed” at Dome C there should be references here.

33091, line 1-16: This is all quite general and doesn’t bring out the challenges of modeling the boundary layer at Concordia. A critical feature of the boundary layer at Concordia in the summer is the rapid collapse of a convective BL to a very stable shallow one. In this respect, the authors neglect one the first papers to point this out, namely: King, J. C., S. A. Argentini, and P. S. Anderson (2006), Contrasts between the summertime surface energy balance and boundary layer structure at Dome C and Halley stations, Antarctica, Journal of Geophysical Research-Atmospheres, 111(D2).

33092-93: If “situations with an overcast sky were not considered” give a brief reason

ACPD

14, C11100–C11105, 2015

here. I realize you come back to this later but the question is whether MAR is not useful in interpreting chemical processes under cloudy sky conditions or whether the chemistry analyses were not carried out for cloudy conditions (it seems like the contrast in photochemistry would be important). It would be useful to identify the percentage of time clouds are present during the experimental period (e.g. 10% or 90% would make a big difference.) Another factor with respect to clouds is that they are often associated with periods of the warming of the surface (increased LWD and warm advection): the subsequent boundary layer evolution under clearing skies would be preconditioned by this effect. Was this examined in the model evaluation?

33095, line 12: “similarity”

33095. Section 3: Does Genthon et al 2013 or Gallée and Gorodetskaya (2008) describe MAR in enough detail especially the high resolution aspect in the boundary layer [. . .a long-term simulation of MAR with ECMWF analyses, showing the interest to represent the atmosphere with a fine vertical resolution (Genthon et al., 2013)]. If this is the case, it seems efficient to refer to other summaries of the properties of MAR and only point out the unique properties here that affect boundary layer structure and associated interpretative demands posed by the need to interpret the chemistry in OPALE.

33096, line 10: Given the strong diurnal temperature range, does SISVAT account for subsurface heat storage during the day and conduction back for radiative loss at night? Were there any firn temperature measurements during OPALE that might indicate whether this is important or not?

33096, line 26: Would the orientation of the sastrugi relative to sun orientation also affect the albedo? I think there was a paper by Gerd Wendler in the 1980s on this.

33099, lines 27-28: Note there is a subtle consideration with “winds from the south”: these lie along terrain contours (compare the 120oE meridian with the 3250m contour). Winds from the southwest might be from the “ocean” namely the Ross Sea region although the origin of trajectories are rarely related to local wind directions. Something

ACPD

14, C11100–C11105, 2015

that would greatly add to the analysis would be using the high resolution of MAR to present some trajectory clusters for various key periods during OPALE. Another concern is that the plateau area to the south is often a region of high photochemical production (Slusher et al 2010). Whether this impacts Concordia may be a good question.

33100, lines 20-21: Focusing the discussion on 26-28 December because of intensive observation of chemical species is “interesting.” However, in looking through the other papers submitted to the OPALE special issue I didn’t find this period called out (although there was a lot to look through and I might have missed it.) More interesting meteorology, as far as the behavior of the HOx-NOx system goes, falls in the period 1-18 December with high winds (above the threshold for blowing snow) that precede a dramatic increase in surface nitrate (Berhanu, OPALE special issue) around 4-9 December. A future research question could well be modeling these types of meteorology and chemistry and whether blowing snow is related in increases in surface nitrate. This surface nitrate increase is followed by followed by large increases in atmospheric NOx concentrations (which appear to depend on wind speed) and surface to atmosphere NOx fluxes until 20 December. As snow nitrate and atmospheric concentrations decline could the MAR model be used to quantify the export of NOx, OH and other radicals? Remember there is an “E” in OPALE. Also of interest is 9 January which is described in Frey et al (special issue): in this case the shallow boundary layer modeling is really critical to evaluate to compare with the profile measurements of NOx.

33101, lines 19-22: With respect to Fig. 4b, the authors refer to an underestimation of temperature (cold bias) in the morning (27 and 28 December) although this bias starts in the evening with the collapse of the daytime boundary layer and intensifies as the model wind speed drops during the night. Should not this cold bias influence calculation of the boundary layer depth? Also when the boundary layer is at or below 10m does MOST still work? In Frey et al, they report that when the boundary layer is less than 10m they remove all the NOx flux data from the analysis (the inlet is at 1m which would be 10% of the depth). It would have been useful to have statistics

ACPD
14, C11100–C11105, 2015

from model-sodar comparisons for boundary layer depth for the entire experimental period, by time of day, rather than just one example. Frey et al show a time series of modeled boundary layer depth for the entire experimental period. Unfortunately, shallow boundary layer periods are not resolvable in their figure. However, in Kukui et al., they show a high resolution figure (their Fig. 1) with boundary layer depths that are effectively zero even though u* never goes to zero. Is it possible that the model is better than assumed with Frey et al.'s 10-m cutoff. After comparison with sodar data this would be extremely important to assess in diagnosing surface chemistry after the collapse of the daytime convective boundary layer. This assumes that a sodar minimum range of 2m was used (the sodar's mode 2: Argentini et al. 2013), As Davis et al. 2008 have pointed out the HOx-NOx system can become very non-linear under conditions of both low OH production and shallow boundary layers that allow NOx concentrations to exceed 250 pptv in a non-linear fashion. Of note, Frey et al show values right after 11/12/11of NOx exceeding 2500 pptv.

Figure 3. It would be useful for cross-referencing the chemistry papers to the model results to highlight (say using light gray shading) periods called out in other papers. For example, in Frey et al. 9 January was a special case (their Figure 2) where balloon profiles were made. The authors should probably call out other specific cases discussed in the OPALE papers. In 9-January case, MAR significantly underestimates the 3-m temperature at night but appears to overestimate wind speed if I am interpreting dates correctly (it would be useful in these plots to have a vertical grid lines). In the lower right of the figure, for friction velocity it would be useful to plot the MAR simulation over the BAS observations because the magenta area covers up the comparison with MAR. In this case it would be useful to see whether the friction velocity or the more rapid cooling in MAR is more important to the calculation of the boundary layer depth. In the wind direction plot, it would be useful to have the ordinate divided for the cardinal and ordinal directions (90 and 45 degree intervals).

Figure 6: The black model line should be plotted on top of the blue sodar stars. Can you

ACPD
14, C11100–C11105, 2015

explain why the sodar reveals an earlier peak and fall-off in boundary layer depth than does the model? Is this some combination of radiative balance, wind speed, surface heat flux or something else?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 33089, 2014.

ACPD

14, C11100–C11105, 2015

Interactive Comment