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 , Received and published: 11 January 2015

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.

1.1. The following sentence is added in the « discussion and conclusion section » en p.33104, line 3: « *Note that, since underestimation will induce also an error in the modelled temperatures measured temperatures were used when interpreting the chemistry (Preunkert et al., 2014)* »

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.

1.2. The height of the BL is not used as an input variable of the 1D box models used by Legrand et al. (2014), Kukui et al. (2014), and Preunkert et al. (2014). It is a diagnostic variable generated by MAR and illustrating the behavior of simulated turbulence. Rather the turbulent diffusion coefficients generated by MAR are used as the input variable of the 1D box models.

Frey et al. (2014) decide to not use MOST when BL height is lower than 10 m.

The comparison between MAR BL height and sodar measurements helps us in evaluating the model. A comparison between the model and sodar measurements is possible for a few days only during the period of interest, which lasts from 4 December 2011 to 11 January 2012 in Legrand et al. (2014), from 19 December to 9 January in Kukui et al. (2014), from 14 December to 11 January in Preunkert et al. (2014), and from 23 November to 12 January in Frey et al. (2014). Sodar data are available only on 12, 13, 18, 21, 26, 27, 28 December 2011 and on 3, and 4 January 2012. Among those days MAR underestimates DLW radiation significantly on 18 December in the evening, and on 21 December. Although the possibility exists to make a comparison between MAR, sodar measurements and other meteorological measurements on 12 and 13 December 2011, the best period for such a comparison is on 26 – 27 – 28 December 2011, since this period is the longest and it is analyzed by the above-mentioned authors. It will be mentioned on p.33100, line 20.

Simulated (observed) minimum and maximum heights of the BL are 3.4 and 224 m (10 m and 150 m) on 12 December and 3.6 and 251 m on 13 December (5 m and 125 m).

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.

1.3. Unfortunately MAR significantly underestimates DLW radiation on 9 January and the period after that day, so that a comparison of MAR simulation with observations on that day is not relevant. In the same way it is not possible to interpret the burst of NOx around 23h00 in Figure 2 of Frey et al. (2014) with MAR simulation.

The following details about how MAR outputs are used in other OPALE studies are given on p. 33092, line 27:

MAR turbulent vertical diffusion coefficients Kz are used by Preunkert et al. (2014) and the uncertainty of the later on HCHO mixing ratios is discussed. Legrand et al. (2014) also use the same MAR outputs in their 1D box model of HONO mixing ratio. Kukui et al. (2014) performed similar calculations using the same MAR output. On the other hand BL heights are not strictly needed since they are redundant with Kz. Frey et al. (2014) use MAR BL heights to determine when they may apply the Monin–Obukhov similarity theory for calculating the turbulent fluxes of NOx in the SBL.

Cross-linking is also made on p. 33100, line 12.

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.

1.4. This is an introductory sentence. References are given later in the text. Clarification is included in the text on p.33091, line 2.

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).

1.5. The critical feature is the generation of a very stable BL after sunset. Rapid collapse of the convective BL at the end of the day is observed at other latitudes. King et al. (2006) paper was already cited in the companion paper of this issue (Gallée et al. 2014) in order to explain the role of sensible heat fluxes at Dome C which are responsible for a strong diurnal cycle of turbulence. It is now also cited in the present paper on p. 33091 line 20.

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

1.6.1. Sentence starting on p.33092 line 28 is rewritten with more details: Observations during bad weather conditions are often discarded when the air (containing contaminants) comes from the station. Bad weather conditions also often occur simultaneously with a significant advection of chemical species, a situation that was discarded in the studies cited above. Finally clear sky conditions were preferred since the assumption of a similar DSW radiation from sunny day to sunny day may be done. These criteria allow us to avoid most of the situations for which clouds are underestimated leading to an erroneous behavior of the surface energy budget, as explained by Legrand et al. (2014).

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?

1.6.2. The boundary layer evolution under clearing skies was not compared with the observations since the model underestimates cloud cover, so that the timing of clearing skies is not the same in the model and in the observations. Also note from the detailed analysis of the 26 – 28 December period that the response of the model could differ depending on the time of the day at which a covered sky occurs (compare the biases of the model on 26 December and 27 December).

33095, line 12: “similarity”

1.7. Correction is made.

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.

The description of the roughness could have been removed from the description of MAR since observations of roughness length were not done at Dome C during OPALE. Nevertheless a blowing snow event is simulated on 29 December and could help in analyzing the sensitivity of the model. Indeed it is responsible for a change of the roughness length from almost constant values around 0.05 mm before the event to 2 mm after the event at Dome C. No significant sensitivity to this change may be found in the behavior of MAR variables near the surface from a look to Fig. 3. Some information about that point is given on p. 33100 line 6.

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?

1.9. SISVAT is a multi-layer snow model, and each snow layer has its own heat capacity and conduction coefficient. Firn temperature measurements were done during summer 2009 – 2010 (Brun et al. 2011) but not during OPALE.

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.

1.10. Indeed the effect of sastrugi is not included in MAR and this now mentioned on p. 33096, line 26. Influence of sastrugi on snow albedo is mentioned on p. 33097, line 21.

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 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.

1.11. Indeed, transport of chemical species in the BL may not be neglected when the wind comes from the ocean. Oceanic influences are typically arriving (from the 1000 km far away northern coast) at Dome C under northerly wind conditions. In addition chemical measurements were made a few hundred meters southwards from the main Station of Dome C. This is why northerly wind situations were not considered during OPALE.
Concerning potential southerly wind advections with potential enriched photochemical produced species, this was not considered in the actual chemistry manuscripts since they treat actually species with a rather short atmospheric lifetime. This should be the purpose of a future study when examining for example the ozone budget at Dome C. Neglecting situations with advection is mentioned in p.33092 line 28 and following.

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?

This should be the purpose of a future study, for example by activating the transport of tracers and possibly the generic chemical model of MAR.

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.

1.12. The reason for choosing 26 – 28 December is given on p. 33100 line 20.

Blowing snow may have occurred during OPALE but we have no observations of that phenomenon. MAR simulated a blowing snow event on 29 December. Influence of blowing snow could be considered in a future study also taking into account the influence of transport.

As already explained it was not possible to compare MAR to the observations on 9 January.

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 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.

1.13.0. p.33101 line 19 is reworded and a sentence is added about the starting time of the underestimation.

1.13.1. The underestimation of turbulence by the K-e model during night-time is explained in p.33101 lines 28-29. Of course this could lead to an underestimation of the BL height. This detail is added on p.33102, line 4.

1.13.2. The BL height is a diagnostic from the turbulence model of MAR. It is never smaller than the height of the lowest level of the model. Unfortunately we do not have continuous sodar measurements to get a comprehensive comparison between the simulated and observed BL height. The period from 26 to 28 December was also chosen to evaluate MAR since it is the longest period for which we have continuous sodar measurements together with other meteorological observations.

1.13.3. MOST could be responsible for the cold bias but as explained on p. 33101 lines 24 – 25 the downward turbulent heat flux is well simulated. Looking at the experiment with 1 m resolution it is found that the weakening of the turbulent fluxes from 1 to 2 m amounts to

slightly more than 20%, a value that is larger than the usual departure from constancy generally accepted (10%). More generally temperature and wind speed at 2 m in the simulations with 1 m and 2 m resolution near the surface have been compared. It has been found that when clear sky is observed they are not sensitive (differences no larger than 1.5°C to 2°C or 1 m/sec) to the vertical resolution even when in the simulation with 1 m resolution the turbulent fluxes between 1 m and 2 m depart from the constancy by 30%. These additional explanations have been included after p.33101 line 27.
1.13.4. Note also that a cold bias near the surface is simulated since simulated turbulence does not shut down. Rather a decoupling of the lowest layers of the model with the surface would have lead to a warm bias.

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).
1.14. Grey shading is used for cloudy periods (DLW assumed to be higher than 130 W/m2). MAR simulation of friction velocity is plotted over the BAS observations. Ordinate are divided in cardinal and ordinal direction in wind direction plot.
Unfortunately MAR works wrong on 9 January 2012.

Figure 6: The black model line should be plotted on top of the blue sodar stars. Can you 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?
1.15. The earlier peak and fall-off in boundary layer depth is marked on 26 December and is due to the presence of clouds, which are not simukated. This is indicated on p.33103, line 14.

Anonymous Referee #2 . Received and published: 25 January 2015

General assessment

This paper describes the performance of a mesoscale atmosphere model when applied to summertime conditions over Dome C, East Antarctica. In general a good agreement is found for wind speed and wind direction, but important deviations are found in simulated shortwave/longwave radiation components and near-surface temperatures. The paper is reasonably well written, but the English needs improving by the editorial staff.

Here I only provide textual comments when a formulation may cause confusion. The figures are generally of good quality. The added value of the science requires better motivation. All in all the paper requires major revisions, see below.

Major comments

The introduction must be restructured and rewritten so as to include more specific information how mesoscale models like MAR can assist in the interpretation of the chemical composition of the Antarctic boundary layer. The current model does not have a chemical routine, so please explain explicitly how the current results are of value for OPALE. Can the results be used to drive an offline chemistry module? It must also become clear what this study adds to previous knowledge on the ABL structure over Dome C, since quite a number of observational studies have been published on that topic recently.

2.1.1. More information on how mesoscale models can assist in the interpretation of the chemical composition of the Antarctic boundary layer is included in the introduction on p.33092 line 17.

2.1.2. Other papers of the special issue use MAR BL height and eddy diffusivity to drive chemical 1D box models (see Legrand et al., 2014, Kukui et al., 2014 and Preunkert et al., 2014). Frey et al. (2014) uses simulated BL height to decide if the conditions to use the Monin-Obukhov Similarity Theory (MOST) are met. More details is included in the paper about what and how model data are used in other papers on p.33092, line 27.

2.1.3. The purpose of the paper is also to analyze the impact of MAR turbulence on the vertical profile of meteorological variables. Such a work has not yet performed with so much details.

Page 33096: An elaborate description is given on the parameterizations of surface and surface layer processes, e.g. z0 as a function of sastrugi formation and decay and the interaction of blowing snow with the vertical transport of radiation; disappointingly little of the influence of these elaborate parameterizations on the model results is found back in the discussion of the results. How important are these model adjustments for the final results at Dome C? For instance, it would be nice to discuss a time series of z0. Was blowing snow a common occurrence during the campaign? If so, was this simulated by the model? Etc.

2.2. Parameterization of z0 was not modified since the study on blowing snow by Gallée et al. (2013). Observations of blowing snow and roughness length were not done at Dome C during OPALE. Nevertheless the simulation of a blowing snow event on 29 December is responsible for a change of the roughness length from almost constant values of 0.05 mm to 2 mm at Dome C. No significant sensitivity to this change may be found from a look to Fig. 3. Some information about that point is given on p. 33100 line 6.

Same page: how is the calibration (line 27) performed? How did MAR perform in terms of 3 m wind speed before this calibration was performed?
2.3. The calibration of the roughness length is performed from observation made near the coast of Adélie Land (see Gallée et al., 2013). No changes have been made for this study since observations were not available. See also p. 33100 line 6.

Table 1: It is remarkable that both LWd and SWd are underestimated. When cloud cover is underestimated in the model, as is suspected, one would expect SWd to be overestimated. Any thoughts?
2.4. We use the solar routine developed by ECMWF. One could expect that SWd is larger than expected when cloud cover is underestimated but this does not preclude the solar routine to underestimate SWd under clear sky situations.

p. 33090, l. 24: the model used by Van As and others (2006) had very high vertical resolution, in the cm range near the surface; in terms of physics, it was not simpler, just 1D. How important are 3D (advection) effects over Dome C, in other words, what is the added effect of performing 3D simulations?
2.5. Advection effects and changes in the pressure gradient force (PGF) are handled in a more realistic way with a 3D model than with a 1D model, since both processes are highly non linear in the real atmosphere. Furthermore Dome C is surrounded by slopes, so that atmospheric dynamics there are characterized by mass divergence when downslope flows occur (usually during night for clear sky conditions). Mass divergence may be responsible for a thinning of the BL at Dome C. Finally as the aim is to use a 3D model in future studies, we prefer to use it and compare it with the observations rather than developing a new 1D model. The additional knowledge of MAR we gain from this study will help us for future studies including e.g., the transport of chemical species.

Another important difference between Van As and others (2006) and this study is that Kohnen is situated on a ridge with surface slope, generating a mixture of inertial oscillations and katabatic winds, while Dome C has no or very little slope, deleting the impact of katabatic forcing. This is supported by the absence of a nocturnal wind speed maximum. Please add a brief discussion along these lines (difference between climate of the ice shelves, the ice sheet slopes and the interior domes) in the introduction, and how these differences in e.g. daily cycles could impact the chemistry of the boundary later.
2.6. Low level jet may be responsible for a nocturnal wind speed maximum just above the BL at Dome C. This point was mentioned on p.33102 line 27 and detailed in a companion paper by Gallée et al. (2014).
The very low air temperatures at Dome C strongly limits latent heat fluxes at Dome C so that the conditions for developing a well mixed layer during daytime are optimal, in contrast to the situation over the ice shelf, as at Halley, for example. This is mentioned on p. 33091 line 20.
Also the Antarctic plateau is far away from the coast, so that the chemical properties of the air masses coming from the Antarctic interior at Dome C are rather homogeneous. This is mentioned on p.33092, line 28.

p. 33103, l. 3: "... while the pressure gradient force (PGF) still contributes to an increase of the wind speed after that time..." but the supergeostrophic wind speeds in the nocturnal jet are caused by a combination of (frictionless) inertia and the Coriolis effect, and do not require changes in the geostrophic wind speed.
2.7. Wind speed (and not geostrophic wind speed) is mentioned in the sentence. Simulated wind speed is smaller than geostrophic wind speed during daytime and does not become supergeostrophic immediately after turbulence shuts down. Rather it tends to become supergeostrophic after some time and then to come back to the geostrophic equilibrium, causing an inertial wave.

Minor and textual comments

p. 33090, l. 17: preferably use 'evaluation' instead of 'validation' when it concerns models
2.8. OK

p. 33090, l. 20: for -> in
2.9. OK

p. 33090, l. 22: remove 'circulation'
2.10. OK

p. 33090, l. 23: an approach ...done -> a study...performed
2.11. OK

p. 33091, l. 13: able -> enable
2.12. OK

p. 33092, l. 27: "...the low troposphere..." perhaps leave out 'low' for a site > 3000 m asl
2.13. OK

p. 33093, l. 26: the sensors used in the K&Z CNR1 are CG3 pyrgeometers and CM3 pyranometers (I may be wrong, please check). Please state their accuracy; if I remember well, measurement error maybe substantial for these sensors and may explain part of the obs-model bias.
2.14. The reviewer is right, the sensor used is a Kipp & Zonen CNR1 which combines two CM3 pyranometers for downward and upward broadband shortwave radiation flux (spectral range 305–2800 nm) and two CG3 pyrgeometers for downward and upward broadband longwave radiation flux (spectral range 5– 50 µm). The K&Z CM3 pyranometer is a thermopile type pyranometer, covered by a single glass dome, which complies with ISO 9060 second-class specifications (estimated accuracy for daily totals ±10%). The K&Z CG3 pyrgeometer consists of a thermopile sensor covered by a silicon window that is transparent for far-infrared radiation but absorbs solar radiation. The factory-provided estimated accuracy of the K&Z CG3 for daily totals is also ±10%.
Errors which may affect the SHW radiation in Antarctica: 1) Icing of the sensor dome, 2) Rime formation on the sensor Dome, 3) Low sun Angle, 4) Sensor tilt, 5) High surface albedo.
Errors which may affect the LW radiation: 1) Window heating offset, 2) Riming of the upward-facing pyrgeometer window, 3) Riming of the downward-facing pyrgeometer window.

Van den Broeke et al. 2004a [Surface Radiation balance in Antarctica as measured with automatic weather stations. M. Van den Broeke, C. Reijmer, and Roderik van de Wal, Journal of Geophysical Research Vol. 109, D09103 doi:10.1029/2003JD004394, 2004] compared radiation measurements of the K&Z CNR1 with radiation data collected at Neumayer station, a BSRN station (70.7°S, 8.4°W, 50 m asl) for a 10-day period in February 2001. At Neumayer, the radiation instruments (K&Z CM11 for shortwave radiation and Eppley PIR for longwave radiation) are ventilated with slightly heated air to prevent rime formation. The comparison yielded a root mean square difference of 2.7% (4.8 W m−2) for daily mean SHWdown and 1.2% (2.7 W m−2) for daily mean LWdown. This shows that under controlled conditions the K&Z CM3 and CG3 perform much better than the listed specifications. Similar results were found by Van den Broeke et al. 2004b (Assessing and improving the quality of unattended Radiation Observations in Antarctica, M. Van den Broeke, D. Van As, C. Reijmer, and Roderik van de Wal, Journal of Atmospheric and Oceanic Technology, 2004).

p. 33095, l. 7: in the absence of a significant surface slope at Dome C and the fact that it is the highest point of the region, I do not expect drainage flow but rather radially diverging flow away from the dome, see major comment above.

2.15. The simulation is 3D and not 1D. Drainage winds will be simulated everywhere over the domain except probably over the Dome.