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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Downslope föhn winds over the Antarctic Peninsula and their effect on the Larsen Ice Shelves" *by* D. P. Grosvenor et al.

Anonymous Referee #2

Received and published: 4 April 2014

General:

This manuscript presents a detailed observational and model analysis of a foehn event in the Antarctic Peninsula and over the Larsen C Ice Shelf in the austral summer of 2006. The study presents a mesoscale model experiment which is compared to simultaneous airborne and ground meteorological observations.

I believe this detailed study is a worthwhile addition to the current body of knowledge on foehn winds in the Antarctic Peninsula. The methods, results, and discussion are very detailed (which on the one hand is a merit but on the other hand makes it a bit tedious to read), and the mesoscale modelling adds a valuable perspective to the aircraft observations.

However, I do have a few recommendations for improvement of this manuscript. Once taken care of, I believe that this paper is suitable for publication in ACP.

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1. I would like to see the argument for the timing mismatch between the model and the observations stronger and more coherently presented in the manuscript. Have the authors considered to use the high-altitude aircraft data during the outward and return flights to nail down this timing mismatch? It should be possible to see the turning of the winds occurring earlier in the model than in reality. Moreover, this allows a comparison between the nudged upper levels and observations, which is a more direct connection between the reanalysis forcing and the observational data. Also, is there additional data (from Rothera?) available that could serve to make the case of the authors stronger?

2. The quality of the surface energy budget analysis is somewhat hampered by deficiencies in the WRF surface scheme. The authors mention the unrealistic values for the longwave emissivity and the shortwave albedo of snow. In addition, the particular model treatment of the turbulent fluxes (especially with the lowest model layer at $\sim$27 m above the surface) may also explain why the modelled amplitude of the turbulent fluxes are smaller in magnitude than the fluxes presented in King et al. and Munneke et al. What are the roughness lengths for momentum, heat, and moisture in the model? Also, it is conceivable that the energy balance fluxes (most notably the ground heat flux) is influenced by the initialization of the snow. Is the snow represented by a single layer? Or multiple layers? How is the snow initialized at the start of the run? Could the ground heat flux be influenced by the setup of the snow model part?

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Minor issues:

p 5772 - I find the abstract rather long in its present form. Can the authors have a critical look at it and see which information may not be so crucial for the abstract after all?

p 5774 l.8: warming -> rising

p 5774 l.16: gives -> give

p 5780 l. 17: You state that temperatures higher than 0C in the cross-ridge flow would allow for surface melt. This is quite a general statement. It is also possible that there is no surface melt, for example if there is a strong inversion, or a high-albedo surface. Whether the surface is melting depends on the surface energy budget, not only on the temperature in the jet. Conversely, there could also be a melting surface if the air temperature at 250-350 m was below 0C. I suggest rephrasing to something like "The effect of these warm jets on surface melt is investigated in section 4."

p 5780 l.22: 4 -> fourth

p 5784 l.5: should there be a reference to figure 9a here?

p 5784 l.12: figure 7b -> figure 9b (?)

p 5788 l.8: moving eastwards -> eastward movement

p 5788 l.16: movement eastwards -> eastward movement

p 5788 l.17: Peninulsa -> Peninsula

p 5791 l.10: Figure 15a and 15b shows -> Figures 15a and 15b show

p 5791 l.12: windspeed -> wind speed

p 5798 l.1: I find this a somewhat difficult statement. First, a shift of $\sim$9 hours makes that there is a shift of the turbulent fluxes with respect to the radiative fluxes (the latter are bound to the time of the day whereas the former are bound on the wind conditions). Second, whether the modelled effects of the jets on the ice-shelf surface are realistic entirely depends on the surface scheme in the model. Later, the authors acknowledge

that this scheme is not fully suitable to study the surface energy budget.

p 5801 l.1: There are more possible causes than the reduced wind in WRF. It could be related to the surface scheme, and to the coarse representation of the boundary layer in WRF, with the lowest atmospheric level at $\sim$27 m above the surface. Can the authors expand on alternative explanations for the representation of the turbulent fluxes in WRF?

p 5807 l. 1: patter -> pattern

p 5812 l.5-7: This sentence is rather complicated, and not easily understood by non-native speakers. Please simplify your message.

p 5823 fig.7: The labels A, B, C, D are not well visible. Please enhance the contrast between the blue background and the black labels.

p 5835 fig.19: I appreciate the attempt to plot all fluxes on the same vertical axis, but this looks a bit artificial to me. Would it be possible to define an anomaly from the latitudinal mean for each flux? It will lead to almost the same graph but the definition for each line would then be the same. All lines will be averaged around 0 by definition. Possibly, you could add the latitudinal means for the fluxes in the legend or as text in the figure.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 5771, 2014.