

We thank both referees for giving up their time to review our paper and for providing constructive comments.

Our responses are below the referee comments in blue (bold and underlined).

Referee#1

2 Specific comments

Data and Methods

1. In section 2.1 more details on the flight path and date should be given. These issues are detailed later on in section 3, but for clarity and better reference a full description here would be nicer.

The flight track information from the start of Section 3.2 has been moved to section 2.1. The description of the observations made have been kept in section 3.2 in order to keep the methods and results separate. Section 2.1 now reads:-

2.1 Aircraft observations

Observations were made by an instrumented DHC6 Twin Otter aircraft operated by the British Antarctic Survey. The aircraft instrumentation is described by (King et al., 2008). Briefly, the aircraft recorded basic meteorological variables (pressure, temperature, frost point temperature, wind speed and direction) at flight level. In addition, a remote measurement of surface temperature was available from a downward-pointing infrared thermometer and upwelling and downwelling long- and shortwave radiative fluxes were measured by aircraft-mounted pyrgeometers and solarimeters.

Figure 4 shows the flight track of the aircraft with the aircraft altitude shown in colour. The aircraft took off from Rothera Research Station (see Fig. 4) at 19:20 UTC on 6 January and headed east. It traversed the Antarctic Peninsula ridge at 3000 m in altitude until the aircraft was ~ 170 km downwind of the ridge crest. Then, at 20:15 UTC, the aircraft descended towards the surface of the Larsen C ice shelf over a horizontal distance of ~ 10 km where it performed some low level flight legs, which will be discussed later (Sect. 3.4.3). At 22:00 UTC it made another ascent within ~ 10 km of the descent profile and returned back over the ridge along a similar path. The reader is also referred to King et al. (2008) for further information on this case study.

Section 3.2 reads:-

3.2 Aircraft observations of the föhn jet

The flight track of the aircraft was described in Section 2.1; we now discuss the observations that were made during the flight. During the initial ascent (close to Rothera) the

2. In section 2.2 the parametrization schemes used for the WRF-simulations should be named, as particularly the turbulence and surface flux parametrization may have some impact on the results.

This information has been added to the text:-

2.2 WRF modelling introduction

The model used is a version of the WRF (Weather Research and Forecasting) mesoscale model (Skamarock and Klemp, 2008) that has been specially modified for use in polar regions by researchers at the Bryd Polar Research Center (Hines and Bromwich, 2008; Bromwich et al., 2009) through improvements in the representation of the polar surface; the WRF parameterization options that are now listed were selected according to these studies and the reader is referred there for further details and for justifications for these choices: the Rapid Radiative Transfer Model (RRTM) was selected for longwave radiation and the Goddard scheme for shortwave radiation; the Mellor-Yamada-Janjić TKE scheme was used for the boundary layer option in conjunction with the Janjić Eta scheme for the surface layer (Janjić, 2002), which is based on Monin-Obukhov similarity theory, but with moisture and thermal roughness lengths that scale with those for momentum as a function of the molecular viscosity for momentum and the friction velocity, following Zilitinkevich (1995); for the land surface model, the four-layer unified Noah scheme was selected. As described in Hines and Bromwich (2008), the latter was modified to deal with deep snow packs and the density, heat capacity and heat conductivity of the snow pack are based upon observations of Antarctic snow firn.

In addition it should be detailed which observations were used for nudging, as the time shift between the observations and the simulation is important for the latter discussion.

The nudging was performed using the same ECMWF analysis data that was used for updating of the lateral boundary conditions. This is now stated in the text:-

The model was initialised with and received lateral boundary information from ECMWF analysis data, which for the period in question was available at $0.5^\circ \times 0.5^\circ$ horizontal resolution with 61 vertical levels. The simulation was started at 00:00 UTC on 5 January 2006 and ran until 00:00 UTC on 8 January 2006. It was decided to perform nudging on all model nests so that the model fields of horizontal wind, temperature and vapour mixing ratio are constantly being moved towards the above mentioned ECMWF analysis fields. This was done since otherwise it was found that the fields drifted away

The thermodynamics and meteorology of the foehn flow

This section is really lengthy and the readability could be much improved by shortening and sharpening the argumentation. Particularly in section 3.2 to 3.5 several issues are discussed multiple times. A potential remedy would be merging several sections (some observations like the time shift between observations and simulation are made several times) or reordering some subsection, as particularly the last subsection (3.6.1 and 3.6.2) pertain mostly to the synoptic scale conditions discussed at the very beginning of the section 3. Also the AWS is at the location of the flight leg A-L1 and therefore the two sections discussing both measurements could benefit from combining them. I would suggest first discussing the large-scale flow evolution including the upstream conditions in the model and the observations (currently sections 3.1, 3.6.1 and 3.6.2), then describing the foehn jets and their evolution in the model and the observations and finally concluding the section with a discussion of foehn dynamics (currently section 3.6).

We agree that this section is lengthy, although effort was made to split it up into appropriate sub-sections in order to break it down into more manageable chunks. However, it is true that the message was sometimes hard to discern in the original manuscript. Therefore we have done some rewriting of this section to make those messages clearer and to help the section to flow better. Section 3.5.1 has been combined into Section 3.4 and labelled “Assessment of the model over longer timescales through comparison to the AWS timeseries”. Sections in 3.5.2 have been re-labelled to “Using the model jet evolution to interpret the AWS timeseries” and is now in a section on its own. We feel that the new names better reflect what was contained in them. Section 3.4.3 has been moved to an appendix with only its main conclusions referred to in the main text in this section, somewhat shortening the section and improving the flow of the arguments.

Unfortunately, we feel that some of the re-ordering of the subsections suggested by the reviewer would not be practical. Sections 3.6.1 and 3.6.2 mainly pertain to the flow structure from the vertical cross section (the theory of Smith and Sun, etc.) and so moving them to before the section that describes Smith and Sun is unfeasible.

We have made the argument regarding the time shift between the observations and the simulation less repetitive. On the suggestion of both Referees we have also discussed evidence from the upper level legs of the aircraft – please see the response to Referee #2 regarding this.

We also agree that Section 3.5.2 was a little confusing and this has been re-written in order to be clearer. In response to the suggestions below, the issues discussed in this

section have also been made clearer through the use of vertical cross sections. Please see those responses for further details on this.

For the discussion of flow patterns at higher and lower levels (300m and 10 m) vertical cross sections perpendicular to the jet axis would help to connect the different levels (in addition to the 1D profiles you show for the comparison to aircraft ascent and descent). **Please see the response to the later comment on this for details about how this has been addressed.**

The description of the flight path and the location of the measurements should be moved to the “Data and Methods” section. **This has been done (described earlier).**

Some further comments:

1. **3.2** On page 5780 the potential impact of latent heating on föhn flow is mentioned. Are there any observations that indicate precipitation and / or cloud formation on the windward side of the AP?

We have included images from MODIS that indicate that there was relatively little upwind cloud formation and so little contribution from latent heating in this case. The images also show that the ice shelf was mostly free of clouds. The following has been added and the new figure is appended after the responses:-

Figure 7 shows MODIS images over the peninsula ridge from 6th Jan at 13:00 UTC. Fig. 7b shows that most of the Larsen C Ice Shelf was relatively cloud free since the ice surface shows up as red, whereas cloud shows as white. There is cloud upwind; however, Fig. 7a demonstrates that this is quite thin. A linear band of thicker cloud can be seen orientated along the ridge crest that is associated with the mountain wave, although there is a gap in this cloud just north of Adelaide Island and Rothera. These observations suggest that latent heating through precipitation removal is not a big contributor to the downwind warming in this case.

2. **3.4.1** You state that the modeled jets extend to the measurement location, which contradicts statements later on in the article.

The reference later in the section refers to at 12UTC, whereas the first reference is referring to 15UTC. The sentence has been changed to the following to make this clearer:-

However, since at 12 UTC the modelled jets do not reach as far east as the location where the aircraft observations were taken, this suggests...

3. It is several times stated that the flow at 10 meters is decoupled from the flow at 300 meters and that the first is essentially influenced by the surface pressure

distribution, while the one at 300 meters is less. You should shortly summarize the dynamical reason for this.

Probably a cross-section perpendicular to the jet axis would also help.

We have added the requested vertical cross section (the figure is included after these responses) and have added the discussion of the dynamical reasons:-

In the simulation the circulation patterns start to change after 12:00 UTC, so that by 15:00 UTC the low pressure circulation over the ice shelf is further east and has intensified (Fig. 16b). The model wind direction over the AWS is closer to westerly at this time. Figure 14a and b suggests that the area of higher wind speed over the AWS at 15:00 UTC is due to wind that emanated from locations further north along the Peninsula mountains (jets 1 and/or 2), and travelled approximately towards the northeast. However, the even higher winds associated with jet 3 have not yet reached the AWS region by 15:00 UTC for the height of 10 m (Fig. 14b) like they have at 300 m (see Fig. 8c). This is further demonstrated in Fig. 15, which shows a vertical cross section taken at 15 UTC on 6th Jan along a line passing over the AWS location and orientated west to east, such that it is perpendicular to the axis of the jets at this time

(see Fig. 8c for the location of the line). The north-south horizontal component shown in the plot reveals much lower wind speeds near the surface compared to those in the jets. A reversed wind direction to the west and east of the jets can also be seen. The modelled differences between the 10 m and 300 m winds are also corroborated by the aircraft observations made at constant heights close the surface, which are described in Appendix A.

It is clear that the modelled jets show stronger winds at 300 m than they do at 10 m in the regions just downwind of the ridge where the jets emanate. However, at the location of the AWS this disparity is much greater. We speculate that this is due to the fact that the initial lower wind speeds at 10 m would lead to less Coriolis turning than the stronger wind jets at 300 m. This would mean less northerly progression in the face of the northwesterly winds at the eastern edge of the ice shelf associated with the pressure gradient.

4. The time shift of the model simulation to the real world may be more easily identified by comparing the upper level aircraft data to the model wind field at the same time and elevation. This would also support the argumentation that the time shift is due to the analysis.

Please refer to the response provided to Referee#2 regarding this matter.

5. 3.6.2 It is known that the moisture content has implications for blocking (e. g. Miglietta and Buzzi, 2001). It would be interesting to investigate whether there is a change in the upstream moisture content during 6 January in the model which could lead to a change in the blocking behavior. The rapid change of the wind speed, which is hypothesized to have a major impact is observed at 1 km altitude and therefore still in the blocked air mass (before and after the cessation of the jets).

We have examined timeseries of relative humidity at 1km and 2km for the same location as those in the manuscript. We do indeed see a rapid reduction in RH at the same time as the wind direction change and cessation of the foehn event. However, without some idealized modelling of this case it is probably impossible to say whether the change in RH had any causal effect on the flow, or whether it was a symptom of the meteorology changes. The shift of the wind direction upwind of the mountain towards southerly would also be associated with reduced relative humidity since the air would then be coming from the dry continent rather than the moist ocean regions. Although the same lack of proof of causality can also be said for the wind direction effect. Further work would be required to answer this, which is beyond the scope of our study.

We have added the RH timeseries and some associated discussion, and cite the Miglietta study:-

Figure 19f reveals a rapid reduction in relative humidity (RH) at the same time as the wind direction change and cessation of the föhn event. There are indications that the moisture content of the upwind air has implications for blocking (Miglietta and Buzzi, 2001), which might suggest that the change in RH is playing some role in the föhn cessation. However, without some idealized modelling of this case it is probably impossible to say whether the change in RH had any causal effect on the flow, or whether

it was a symptom of the meteorology changes. The shift of the wind direction upwind of the mountain towards southerly would also be associated with reduced relative humidity since the air would then be coming from the dry continent rather than the moist oceanic regions. Although the same lack of proof of causality can also be said for the wind direction effect. Further work would be required to answer this, which is beyond the scope of our study.

6. The flow behavior here is different from the one described by Orr et al. (2008) for blocked flow. It would be nice to include a paragraph discussing the differences (in upstream conditions) between their case and yours and speculate on the reasons for the different behavior.

We have added a paragraph discussing this in the “Potential temperature cross section and foehn dynamics” section.

Finally, the simulations of flow over the Antarctic Peninsula presented in Orr et al. (2008) showed a case where there was upstream blocking in a similar flow regime to that in our case ($\hat{h}=3.0$ compared to $\hat{h}=3.8$ in our case) and with a similar upstream vertical stratification pattern. However, in Orr et al. (2008) there was no descent of warm, accelerated air on the leeward side down to the surface in contrast to in our case. It is difficult to say for sure why the two outcomes are so different given the complexity of such flows and the incompleteness of the knowledge of them, as well as the possibility of time dependent behaviour. Although, one key difference between the two simulations is that the horizontal resolution used in Orr et al. (2008) was 12 km, compared to the 1.875 km used in our study. This may have led to poorly represented gravity waves in the latter, which in our study had a horizontal wavelength of around 60 km and were shown to have been vital for the lee flow development.

The effects of the föhn jets on surface melting and the surface energy budget of the Larsen Ice Shelves

1. One of the main statements is that reduced cloud cover due to the foehn air drying is one major reason for enhanced melting. However, there is no figure illustrating the dryness of the air. Are there any measurements of cloud cover or relative humidity from the AWS or even a satellite picture to illustrate this? Alternatively also WRF model output could be used to this end.

The dryness of the air observed by the aircraft is shown and discussed in the King et al. (2008) paper and is referenced in the manuscript in section describing the aircraft measurements :-

Warm air temperatures (Fig. 5c) were observed at around the same height as the jet wind speed maximum with a maximum of 4.6 °C at 283 m above the surface. The presence of this warm air caused a strong temperature inversion above the ice surface. The surface itself remained close to 0 °C, as confirmed by the surface infrared aircraft measurements (King et al., 2008). King et al. (2008) also showed that the downwind air had a considerably higher potential and equivalent potential temperature and was drier than that at equivalent altitudes on the upwind side. This indicates either adiabatic warming due to the descent of dry air that either originated from above the mountain, or diabatic warming of air that came from below the mountain on the upwind side and experienced latent heat warming due to ice or liquid formation and drying by precipitation loss.

Also, as described above, a MODIS image has been added, which shows almost cloud-free conditions over the ice shelf. A statement about a lack of cloud cover has been added to the shortwave radiation section:-

deposition to the surface, etc. Very little cloud cover was produced over the ice shelf during the simulation, which is consistent with the aircraft observations and the satellite image shown in Fig. 7.

2. The WRF model estimates for ground heat flux, the sensible and latent heat flux

might be dependent on the chosen parametrization of boundary layer, turbulence and surface processes and the involved assumptions. Could you add a section where you discuss this issue and the quality of the parameterizations over ice / snow covered surfaces?

Please refer to the response to Referree#2 for our response to this.

3 Technical corrections

These have all been attended to.

1. page 5776, line 3: “described by **King et al. (2008)**”
2. page 5776, line 18: Leave out the first part of the sentence (or detail instead which vertical coordinate system is used by the model). In the second part the “increase with height” should be replaced by “decrease with height”, if the vertical resolution is meant.
3. page 5776, line 20: “where it remained **constant** throughout ...” (?)
4. page 5777, line 14: “by circumpolar flow”
5. page 5777, line 15: “(05:00 UTC on 5 January **2006**)”
6. page 5778, line 6: AP should be defined somewhere before
7. page 5778, line 20: “with **this** system” unclear reference
8. page 5780, line 4: “descent of dry air that originated”
9. page 5780, line 12: “but above (between 600 and 2000 m) the wind had roared”
10. page 5781, line 12: “föhn flow [...]” replace by “föhn onset occurred before 00 UTC on 5 January”
11. page 5781, line 23: “At 09:00 UTC (Fig. 7a) three main jet formed, which extended eastwards”
12. page 5782, line 21: “evolved **such** that”
13. page 5784, line 24: “this is likely due to”
14. page 5784, line 25: “compared to 12:00 UTC”
15. page 5785, lines 7-12: Split this sentence it is fairly long and therefore difficult to understand.
16. page 5785, line 20: Hardly visible in Fig. 7d due to the chosen color
17. page 5787, line 4: Add reference to section in the last sentence.
18. page 5788, line 8/9: “The **eastward shift** of the small low pressure system [...] **may be** related to the”
19. page 5789, line 22: “on the other side” unclear reference
20. page 5791, line 10: “vertical cross sections **along** the black line in Fig.7”
21. page 5791, line 12: “horizontal windspeed perpendicular to”
22. page 5791, line 14: “hereafter **be** denoted as”
23. page 5791, line 15: “the **cross section** passes through”
24. page 5792, line 22: “Thus **strong** low level blocking [...] observed **in** the simulation”
25. page 5793, line 4-8: Split up this sentence!
26. page 5793, line 15: Why SS87 for Smith (1989)?
27. page 5796, line 3: “within the region of low U followed”
28. page 5797, line 12: increase in h is almost not visible from the graphic
29. page 5797, line 23: “it was associated with”
30. page 5799, line 25: Reference for “similarly”?
31. page 5802, line 11: remove “which are explained shortly”
32. page 5803, line 9: “**second largest term**”

33. page 5803, line 15: “the ice shelf surface **temperature**”
34. page 5804, line 10: “at the southern **model domain boundary**”
35. page 5805, line 11 f: “this trend **is** / **maybe is** mainly driven [...] which is most likely due to”
36. page 5806, line 23 f: “The pattern is strongly anticorrelated ...” Please reformulate this sentence. You are referring to the air content pattern, but it could be misinterpreted to refer to the snow melt pattern.
37. page 5807, line 1: “spatial pattern”
38. page 5807, line 5: “might **contribute** to the differences”

References

Hines, K. M. and Bromwich, D. H.: Development and testing of Polar Weather Research and Forecasting (WRF) model, Part I: Greenland ice sheet meteorology, *Mon. Weather Rev.*, 136, 1971–1989, doi:<http://dx.doi.org/10.1175/2007MWR2112.110.1175/2007MWR2112.1>, 25 2008.

Janjić, Z. I., 2002: Nonsingular implementation of the Mellor–Yamada level 2.5 scheme in the NCEP Meso model. NCEP Office Note 437, National Centers for Environmental Prediction, 61 pp.

Zilitinkevich, S.S., 1995: Non-local turbulent transport: pollution dispersion aspects of coherent structure of convective flows. In: *Air Pollution III - Volume I. Air Pollution Theory and Simulation* (Eds. H. Power, N. Moussiopoulos and C.A. Brebbia). Computational Mechanics Publications, Southampton Boston, 53-60.

We thank both referees for giving up their time to review our paper and for providing constructive comments.

Our responses are below the referee comments in blue (bold and underlined).

Referee #2

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?

We have examined the aircraft data taken on the high altitude (around 3000m) approach and return legs. Unfortunately, given that there is only a time difference of 2.25 hours between the two legs and the fact that this gives are only two datapoints, it is quite difficult to say for sure whether there is a timing discrepancy between the model and reality. What is really needed is a longer term timeseries, which is difficult to accomplish at high altitude. Besides this, the differences involved are actually quite small – e.g. 9 hours is not a large amount of time relative to the duration of the event and the amount of turning of the upper level winds over the relevant period is fairly slight (around 37 degrees). Despite this, the data suggests that the modelled pressure was lower than that observed and the wind direction directed more towards the south, which is consistent with the timing mismatch suggested in the manuscript. Surface pressure data from Rothera shows a similar overall drop in pressure between the model and reality with similar timing (wind data is highly variable and not likely useful due to terrain effects and low level blocking). However, the nature of the changes are quite different with Rothera showing a constant pressure followed by a sudden drop and the model showing a gradual change, which may be indicative of errors in the large scale meteorological fields. Overall, though, it is hard to provide firm evidence that such a timing mismatch in upper level winds (and in the shifting of the low pressure systems) actually occurred. We have described this evidence in the updated manuscript and made it clear that the idea is likely to remain speculative, but plausible, and that there may be other causes for the mismatch in the timing of the low level winds as diagnosed by the AWS comparison. The revised text reads as follows (highlighted) :-

10 The moving eastwards of the small low pressure system over the ice shelf seen in Fig. 16a and b looks to be related to the movement of the larger low pressure system over the Ronne Ice Shelf (as seen in Fig. 2). It is possible that this system shifted prematurely in the model compared to reality and was responsible for the influx of southerly winds onto the ice shelf giving rise to the earlier change in 10 m wind speeds and direction compared to the AWS. Figure 2b shows that the movement of the low pressure system has resulted in the winds on the west of the Peninsula shifting so that they no longer impact perpendicularly to the ridge. It seems likely that this may have caused the cessation of the föhn jets since föhn flow generally requires winds that are close to perpendicular to the ridge. If the winds shifted early in the model compared to reality then this may have also caused the early cessation of the föhn jets.

20 However, it is difficult to ascertain for sure whether there was a timing discrepancy between the model and reality for these large scale systems. Wind data at upper levels (above the mountain ridge height) would be useful for this since the flow is likely to be less variable and hence more representative of the larger scale situation. Unfortunately, only brief observations at such altitudes are available. For the aircraft observations made above the ice shelf at around 3000 m, the eastward flight leg (the earliest leg at around 20:07 UTC) and the westward leg (22:23 UTC) were only 2.25 hours apart, whereas what is ideally needed is a longer term timeseries. Comparisons with the model at the time of the earlier leg do show that the model pressure was 1.8 hPa

lower than the observed mean over the leg and the wind direction was around 20° too low. These are both consistent with the upper situation changing too early in the model since the modelled pressure was dropping and winds rotating towards the south in the model. However, given the small margins involved it is likely that instrument uncertainties could also account for these differences. Comparisons to the surface pressure timeseries at Rothera (not shown) reveal a similar decrease in pressure between the model and observations after 0 UTC on 6th Jan, with no clear evidence of a timing issue. One difference, though, is that the observed pressure drops in a "step change" manner between 0 UTC on 6th Jan and 0 UTC on 7th Jan with fairly constant pressure in between, whereas the modelled change is more gradual.

Thus there is some evidence that there are discrepancies with the pressure systems and upper level winds of the model compared to reality. This would point towards a lack of accuracy with the large scale analysis that drives the model boundary conditions and upper level nudging, which in turn may affect the föhn winds. However, given the evidence available, this is fairly speculative and it is possible that there were other causes for the timing differences seen in the low level winds between the model and observations. It should also be borne in mind that the change in upper level wind direction over the period during which the jets ceased was quite small; the wind direction was 237° at 6 UTC on 6th Jan and reduced by only 37° by 12 UTC on 7th Jan. Thus the margins of any error in the analysis are likely to be small, although the results here suggest that such small upper wind direction changes may be important for determining whether föhn flow occurs or not. Also, a timing difference of approximately 9 hours is fairly small given the overall timeframe of the existence of the jets.

In summary, there are some differences between the model and the observations, but overall the agreement is good and gives confidence that the modelled jet behaviour was similar to reality in many aspects.

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 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?

The selection of surface layer and land-use scheme is based upon the thorough testing and subsequent modification of the various available WRF schemes in order to determine those that best matched observations over ice covered surfaces, as detailed in Hines and Bromwich (2008).

The surface layer scheme used is that from the Eta model, which is based on Monin-Obukhov similarity theory, but with modifications following (Janjić, 2002). The roughness length for momentum is 10^{-3} m and the moisture and thermal roughness lengths are scaled from this following Zilitinkevitch (1995) as a function of the molecular viscosity for momentum and the friction velocity.

The snow pack is represented using four layers through the use of the Noah land surface model with modifications to deal with deep snow-packs described in Hines and Bromwich (2008). The density, heat capacity and heat conductivity of the snow-pack are based upon observations of Antarctic snow firn. So, the representation in the simulations presented in our paper were probably the best possible for ground heat flux calculations with the WRF setup as it was at the time. Of course there will certainly be scope for improvements, particularly regarding the tailoring of the scheme to the specific region of the simulation. Unfortunately, this is beyond the scope of our study.

The point about the initialization of the subsurface snow temperatures made by the Referee is well taken and is likely to be the largest area of weakness for the representation of ground heat flux calculations. The values provided within the WRF domain setup utility were used, which are based on annual averages. This therefore may introduce some errors in the ground heat flux and melting calculations since the use of seasonally varying subsurface temperatures tailored for Antarctic ice shelves would be more appropriate. Also, there may be some spin-up period for the temperatures of the sub-surface layers associated with the use of this data.

We have added to the discussion on these two issues in the revised text.

RE sensible and latent heat fluxes:-

Föhn jets are also warm (near surface air temperature $> 0^{\circ}\text{C}$) and so caused an increase in the amount of downward sensible heat flux at the surface. However, because the jet air is also dry, surface energy loss due to snow ablation (latent heat fluxes) tends to cancel out a lot of the surface heating effect due to sensible heating. This was the case in the modelling in this study and this is also consistent with the aircraft observations and AWS analysis mentioned above. However, the comparison to those results suggests that the sensible and latent heat fluxes were underestimated in the model, indicating deficiencies in the model representation of these processes and their link to the jets, or of the föhn jets themselves. This is likely to implicate the surface layer scheme parameterization. The selection of the Janjić Eta scheme (see Section 2.2 for details) used in this study was based upon the thorough testing of the various available WRF schemes in order to determine those that best matched observations over ice covered surfaces, as detailed in Hines and Bromwich (2008). However, improved accuracy could likely be obtained through the use of roughness length values and scalings that are tailored to the Larsen C Ice Shelf.

RE ground heat fluxes:-

Whilst the model treatment of the thermal properties of the sub-surface snow pack were specially modified to deal with deep snowpacks, including the use of density, heat capacity and heat conductivity values taken from observations of Antarctic snow firn (Hines and Bromwich, 2008), it is likely that some deficiencies still remain. The values provided within the WRF domain setup utility were used for the initialization of the sub-surface snow temperatures, which are based on annual averages. This therefore may introduce some errors in the ground heat flux and melting calculations since the use of seasonally varying sub-surface temperatures tailored for the Larsen C Ice Shelf would be more appropriate. Also, there may be some spin-up period for the temperatures of the sub-surface layers associated with the use of this data. Therefore, it is recommended that sub-surface temperature data from longer term runs (i.e. with fully spun-up sub-surface temperatures) of this region are used for future studies (e.g. data from the Antarctic Mesoscale Prediction System, known as AMPS, or other polar WRF runs). The provision of sub-surface melt layers may also lead to better model accuracy in melting estimates.

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Minor issues:

These have all been attended to, except where noted below.

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?

The abstract has been shortened a little, although it is difficult to make it too short due to the coverage of a number of topics in the paper.

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."

This sentence has been changed to "Such temperatures could promote melting of the ice surface;" in order to indicate that the melting is not certain.

p 5780 l.22: 4 -> fourth

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

Rather, this should be Fig. 5a since we are referring to the time of 12UTC.

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

Again, this should be Fig. 5b since we are referring to the time of 12UTC. Figure 9 is referred to in the next section.

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 -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.

We acknowledge that the diurnal timing issues are likely to cause different interactions in the model compared to reality and that a lot depends upon the realism of the surface scheme; the paragraph has been changed to:-

The good match between the model and observations presented so far give confidence that the development and evolution of the modeled jets are similar to that of the real jets, which might suggest that the modeled effects of the jets on the ice shelf surface will also be realistic. However, we also acknowledge that the interactions between the jet dynamics and the radiative fluxes will be somewhat different from those in reality due to the timing issues described earlier. Also, the modelled impact of the jets upon the ice surface will be dependent upon the surface scheme of the model, which is discussed later.

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 -27 m above the surface. Can the

authors expand on alternative explanations for the representation of the turbulent fluxes in WRF?

We have added a sentence here to mention that deficiencies in the model parameterization of the surface layer turbulent fluxes may also be to blame and referred to the discussion section for more details (as described above).

p 5807 l. 1: patter -> pattern

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

This has been changed to:-

of Larsen Ice Shelf surface melting. Our results suggest that reduced upwind blocking, due to wind speed increases or stability decreases, might not result in an increased likelihood of föhn events over the Antarctic Peninsula, as suggested in previous studies. Thus, increased westerly wind strength due to climate change may not necessarily

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.

The contrast has been improved.

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.

We agree that this plot is unusual, but feel that what is suggested here would not be that much different from its present form. The mathematical definition would indeed be the same for each line. However, the means for each line would have to be listed in the legend, just as the values at the reference location are now. The disadvantage would be that it would also make the lines that are currently not adjusted harder to interpret.

Additional changes

Below are listed some minor changes that have been performed, but which were not requested by the referees.

- A sentence has been added to the abstract to highlight a key result:-

region has generally not been considered. Our results therefore suggest that reduced upwind blocking, due to wind speed increases or stability decreases, might not result in an increased likelihood of föhn events over the Antarctic Peninsula, as previously suggested.

- References to a recent paper (Elvidge, 2014) on a similar topic have been included, along with some discussion:-

Section 1 (Introduction) :-

of the flows for ice shelf melt rate and stability. However, little is known about these details in the context of the Antarctic Peninsula, except for the very recent results of Elvidge et al. (2014). In the latter some simulations of föhn flow and comparisons to aircraft observations for three different types of flow regime were presented following the OFCAP (Orographic Flows and the Climate of the Antarctic Peninsula) field campaign. These results are discussed some more in Sections 3.6.2 and 4.3.4. Our paper

Section 3.6.2 :-

60 km and were shown to have been vital for the lee flow development. Recent 1.5 km resolution simulations presented in Elvidge et al. (2014) also showed the occurrence of föhn flow in blocked upwind conditions, which also indicates that a high horizontal resolution may be required to resolve such flows.

Section 4.3.4 :-

for melting through these processes. Note that in the recent study of Elvidge et al. (2014) the föhn air during a similar upwind blocking case was actually cooler than the surrounding air since it was associated with gap flow that had descended less than the larger scale flow. It would be interesting to compare these two cases in order to understand these differences, although this is beyond the scope of this study.

Section 5 (Discussions and conclusions)

Recent modelling work (Elvidge et al., 2014) presented simulations of a föhn case during upwind blocking with some similarities to the case presented here. However, there appear to be some key differences since the föhn jets were cooler than the surrounding air, which is the opposite to what was observed here. Understanding these differences would provide some interesting insight into these processes, but is unfortunately beyond the scope of our study. Finally, the likelihood from the results of this paper and from Elvidge et al. (2014) that föhn events can occur in conditions of strong upwind blocking has ramifications for how meteorological data is interpreted in terms

of Larsen Ice Shelf surface melting.

- A new subsection (3.6.2) has been created and some material was moved to there so that the order of the topics was changed slightly to allow a better flow.

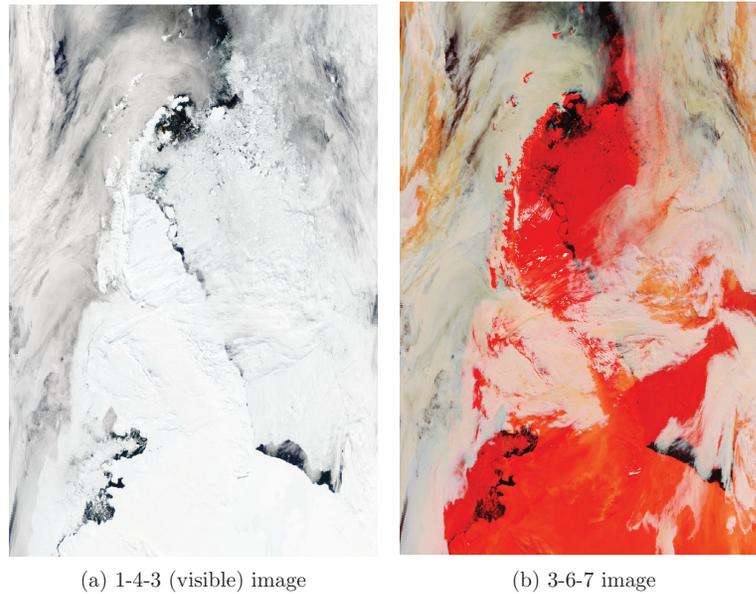


Fig. 7. MODIS images over the Antarctic Peninsula region from 6th Jan at 13:00 UTC. a) shows the visible image (bands 1, 4 and 3 used for red (R), green (G) and blue (B), respectively). b) shows a false colour image using, respectively, bands 3, 6 and 7 for RGB. In b) ice covered land shows up as red, whereas cloud shows up as white. The image is orientated approximately with north at the top and south at the bottom. The outline of the ice shelf, the ice covered land and sea-ice to the east of the ice shelf can be discerned in (a) - see Fig. 4 to aid identification. b) demonstrates that most of the Larsen C Ice Shelf was relatively cloud free. a) shows that the cloud upwind (west) of the ridge is quite thin, whereas much thicker cloud is present along the ridge crest (except in the central portion of the ridge just north of Adelaide Island). Images were taken from http://lance-modis.eosdis.nasa.gov/cgi-bin/imagery/single.cgi?image=crefl1_143.A2006006130000-2006006130459.1km.jpg

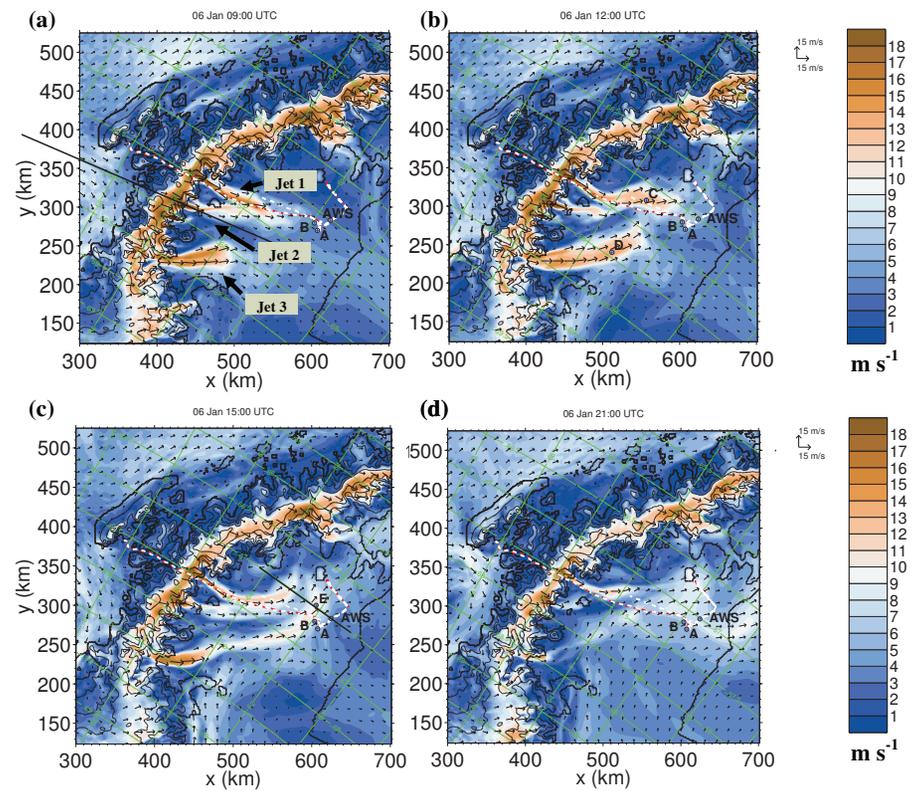


Fig. 8. As for Fig. 6 except in close up view over the ice shelf and at different times on 6 January: 09:00 UTC **(a)**, 12:00 UTC **(b)**, 15:00 UTC **(c)** and 21:00 UTC **(d)**. Also marked are the locations of various other points where the model profiles in Figs. 5 and 10 have been taken. The black straight line in (a) is the line over which the cross sections in Fig 15 were taken, and the line in (c) is that for the cross section in Fig 13.

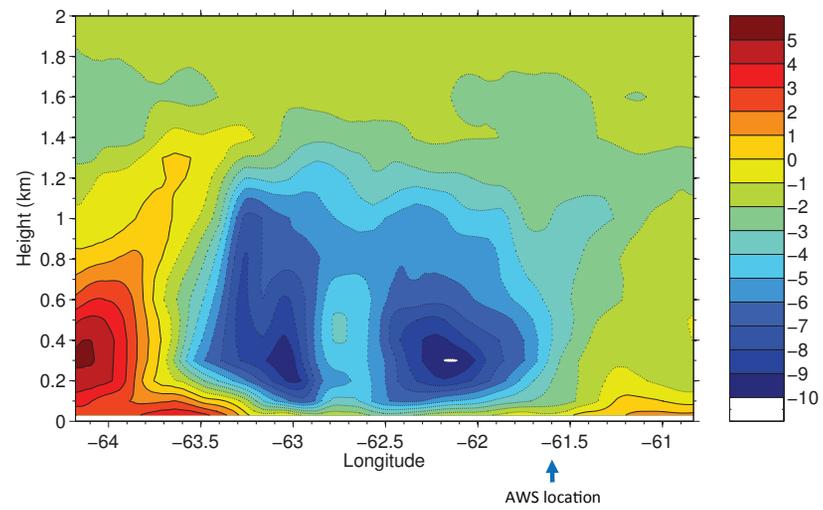


Fig. 13. Vertical cross section through the straight black line in Fig. 8c for 6th Jan at 15 UTC. The colours show the component horizontal wind velocity in a direction perpendicular to the line. Positive values indicate the component directed out of the page in an approximately northerly direction. The location of the AWS is also marked.

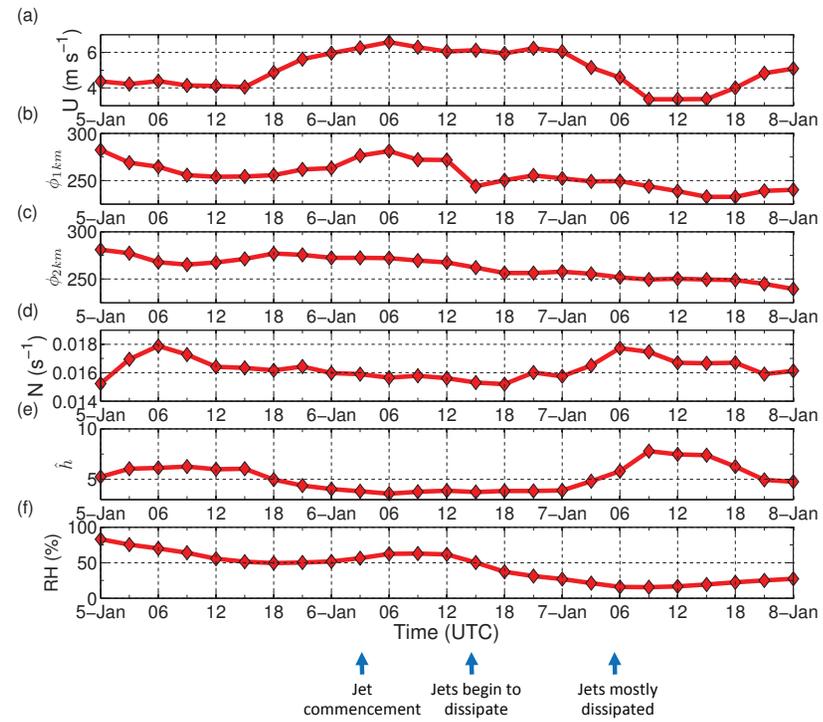


Fig. 17. Timeseries of various quantities taken from the profile at the lefthand edge of the cross section in Fig. 15. **(a)** shows the component horizontal wind for the cross section averaged between heights of 0 and 2 km; **(b)** and **(c)** show the wind direction (ϕ) at heights of 1 and 2 km, respectively; **(d)** shows that Brunt Väisälä frequency; **(e)** shows the non-dimensional mountain height; and **(f)** shows the relative humidity (RH) at a height of 2 km. Also marked are notable times for the development of the near surface jet on Larsen C.