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 $0.5° \times 0.5°$ 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 will add the RH timeseries and associated discussion, and cite the Miglietta study in future versions of the manuscript.

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 orginiated”
9. page 5780, line 12: “but above (between 600 and 2000 m) the wind had roated”
10. page 5781, line 12: “föhn flow [...]” replace by “föhn onset occured before 00 UTC on 5 January”
11. page 5781, line 23: “At 09:00 UTC (Fig. 7a) three main jet formed, which extended

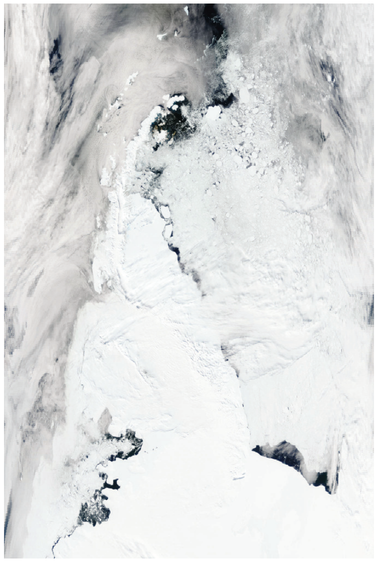
eastwards"
12. page 5782, line 21: "evolved **such** that"
13. page 5784, line 24: "this is likely due to"
14. page 5784, line 25: "compared to 12:00 UTC"
15. page 5785, lines 7-12: Split this sentence it is fairly long and therefore difficult to understand.
16. page 5785, line 20: Hardly visible in 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 refering to the air content pattern, but it could be misinterpreted to refer to the snow melt pattern.
37. page 5807, line 1: "spatial patter**n**"
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.



(a) 1-4-3 (visible) image

(b) 3-6-7 image

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

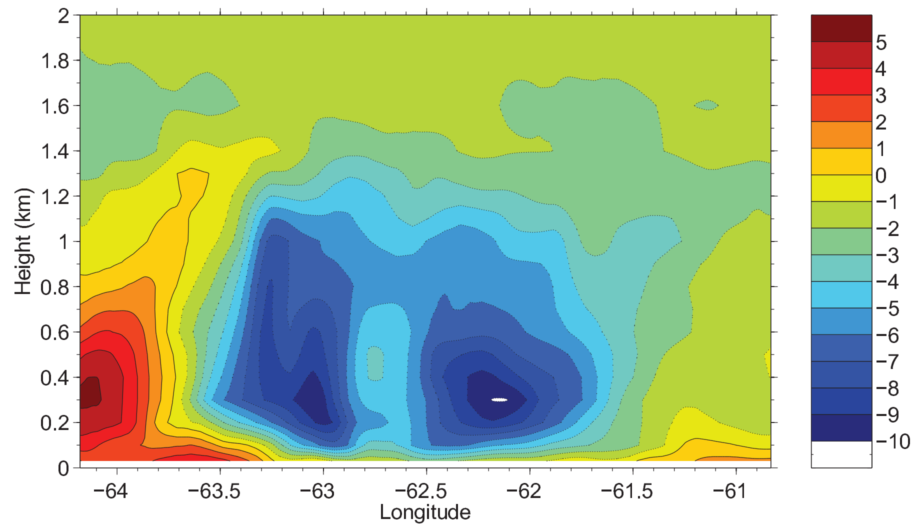


Fig. 15. Vertical cross section through the straight black line in Fig. 8c for 6th Jan at 15 UTC. The colours show the component horizontal wind velocity in a direction perpendicular to the line. Positive values indicate the component directed out of the page in an approximately northerly direction.

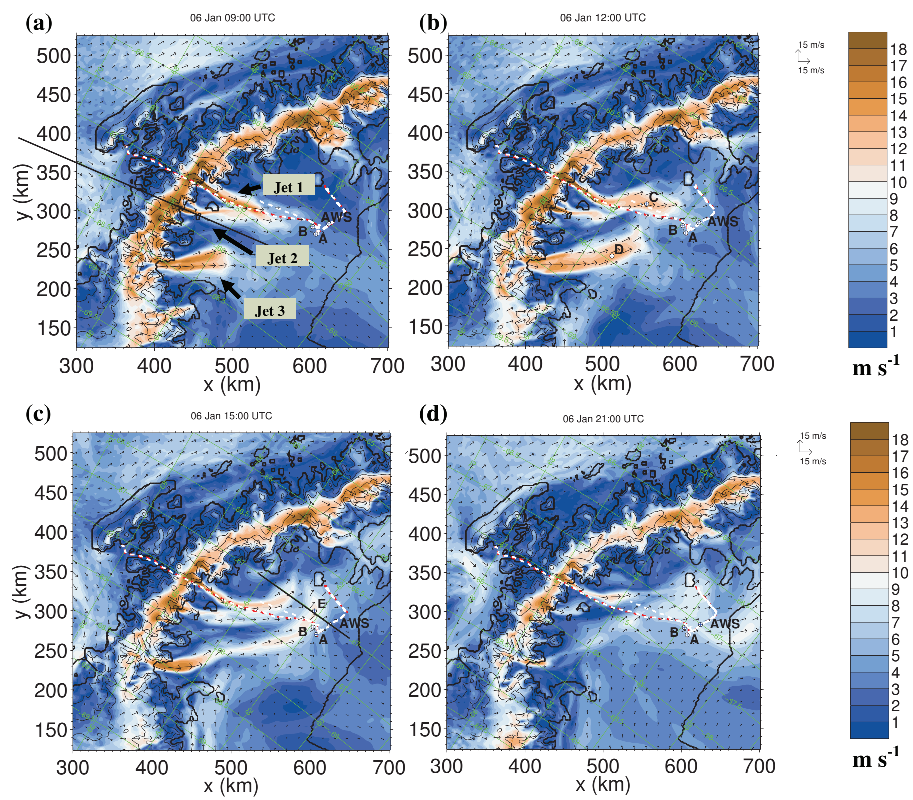

(a)
06 Jan 09:00 UTC
(b)
06 Jan 12:00 UTC
(c)
06 Jan 15:00 UTC
(d)
06 Jan 21:00 UTC
Jet 1
Jet 2
Jet 3
AWS
A
B
C
D
E
x (km)
y (km)
15 m/s
m s⁻¹

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 17 were taken, and the line in (c) is that for the cross section in Fig 15.