Response to review of "Controlled meteorological (CMET) balloon profiling of the Arctic atmospheric boundary layer around Spitsbergen compared to a mesoscale model" by T. J. Roberts et al.

We thank the two anonymous reviewers and GJ Steeneveld for their useful comments that have greatly helped to improve the manuscript. We provide our responses below.

Comment from Anonymous Referee #2

The paper compares model simulations carried out using a mesoscale meteorological model, with resolution down to 1 km, with observations from controlled meteorological balloons. This comparison bears on the polar atmospheric boundary layer, the balloons being laucnhed from Svalbard. The outcome demonstrates that the model fails to reproduce many characteristics of the observaed boundary layer. The balloon technology used is new and makes it possible to obtain numerous vertical soundings along a flight. This study is of interest both because it is a demonstration of the usefulness of these controlled meteorological balloons and because it shows the deificiencies of themesoscale model at these high latitudes and in the presence of complex terrain and of fractional sea-ice. The paper is clearly written, the conclusions are well supported. I recommend publication after a minor revision.

We thank the reviewer for their comments highlighting the interest in CMET balloons as a new technology to especially probe remote regions. As a response to other reviewer comments we have decided to focus on CMET observations compared to ECMWF model output in the updated ms.

Major points:
1. CMET balloons are a new technology and this paper is an important demonstration of the possibilities that these balloons offer for investigating the Boundary Layer. As they are new, it would be useful to give some more description of the balloons, their design and principle, and the implementation.At present, there are a few sentences at the top of p27543. We understand or imagine what the balloons may be. It would be best to give more details (principle, autonomy, timescales for a vertical sounding, ascent/descent rates, range of altitudes that can be sampled...). Of course, this is certainly described in Voss et al, 2013; but a few sentences in the present paper would make it more self-sufficient...

We agree that more information on the CMET's should be provided, especially as the information in Voss et al. (2013) may not be available to all readers and because further insights about the CMETs have since been gained since that publications. We now provide a dedicated section in Methods to describing the CMET technology. Example of Text added to Methods:

"Controlled Meteorological (CMET) balloons can fly for multiple days in the troposphere with altitude controlled via satellite link (Voss et al., 2012). Altitude control is achieved by the dual balloon design (high-pressure inner and low pressure outer balloon) between which helium is transferred by a miniature pump-valve system. Commands sent through an Iridium satellite link can set target altitude (0–3500 m), control band (~50 – 500 m with the higher band using less power) vertical velocity (~0.5 - 1.5 m/s), termination countdown timer, and numerous other operational parameters. For this study, a new capacity was added to perform automated soundings between two specified pressure altitudes.
The 215-gram CMET payload (excluding balloon envelopes) includes the control electronics, GPS receiver, satellite modem, pump-valve system, lithium polymer battery, photovoltaic

panel, aspirated T-RH sensor, and a vacuum-insulated pouch for the payload. The payload temperature is maintained within acceptable operating limits (typically +20 ˚C above ambient) even at altitudes of several kilometres in the Arctic.
An aviation-grade pressure sensor (Freescale MPXH6115A) coupled to a 16-bit analog-to-digital converter (Analog Devices AD7795) provides altitude information to the balloon's control algorithm every 10 seconds during flight. As part of data post-processing, this pressure-derived altitude is further verified using the in-flight GPS altitude (Inventek ISM300X). GPS latitude and longitude provide the in-flight CMET coordinates and are also further analysed post-flight to determine wind speeds in eastward (U) and northward (V) directions.
Temperature is measured using a thermistor (General Electric MC65F103A) in a 10k-Ohm divider circuit coupled to the aforementioned analog-to-digital converter. A capacitance humidity sensor (G-TUCN.34 from UPSI, covering 2 to 98 % RH range over -40° C to + 85° C) generates a signal which is a function of the ambient relative humidity (RH) with respect to water. Relative humidity was converted to specific humidity for comparison to the ECMWF model output. "

2. The relative performances of the three ABL schemes used are not sufficiently described; the main conclusion insists that the three are fairly close together, and far from the observations, indicating that there is work yet to be done in understanding and modeling the polar ABL. Fine. Nonetheless, in the frame of the present study, were there some aspects which seemed better described with one scheme rather than the others?

Following the comment of reviewer 3 we have updated the CMET-model comparison to focus on the ECMWF data rather than WRF. As suggested by that reviewer, we will consider to make a follow-up improved WRF study in future with more detailed sensitivity studies that will take into consideration the reviewer's question about performance of the different boundary layer schemes.

Minor points: p27540, line 11: useful to add preicison on finest resolution: 'nested grids down to 1 km'
We agree but this text has now changed given the adjusted ms scope.

p27541, line 9: remove comma: 'processes, is' -> 'pocesses is' (commas around 'however' could also be removed)
Corrected.

p27542, lines 9-20: make a table perhaps (type of instrument, number of observations, publication..)? This sentence is not readable.
Our updated introduction text has a renewed focus therefore we no longer list so many studies in one sentence - we agree it needed improving. Any future study focused on WRF modelling will clearly outline and tabulate as appropriate lists of previous studies.

p27543, line 2: commanded -> commandp27543, line 17: 'nunatak' will not be understood by many readers I expect; it may be justified to leave as is (and motivated readers will learn a new word...) or to change to something like "topographically induced convection", although less precise...
We choose to keep nunatak but look for ways to briefly explain it in an accurate manner.

p27546, line 22: 'Gulf Stream' -> North Atlantic Drift rather...
Yes we correct our terminology.

p27547, line 3: 'cumulus convection was neglected' -> 'the cumulus convection scheme was unused'

This text is no longer used.

p27548, line 2: become -> became line 3: ’given occurence of’ -> ’due to the presence of’?
Corrected

p27566, figure 5: fonts are too small
All fonts in updated figures are checked to be large enough size.

p27568, figure 7: bottom right panel: for the direction, could the authors use or set up a color table that is periodic (ie the color for 360 should be the same as that for 0, eg by setting up the colormap twice, head to tail (there would be an inconvenient: a 180 degree ambiguity as each color would correspond to 2 angles) or by creating a periodic color table (eg blue to green to yellow to red to purple to blue)
Corrected. This is possible using one of the existing Matlab colormaps, “hsv” that passes through a kind of rainbow spectrum, but where minimum and maximum of the colormap range are both red colour.

p27570, figure 9: color map should be the same for all three panels in each column (see column 1, middel panel)
We agree color maps should be the same for easy comparison. This issue is checked and corrected in all new figures in the ms where similar multiple panels are presented.

p27570-1, figure 9, 11: it is somewhat misleading to show observations from the whole flight and a cross section at only one given time on the same plot. Perhaps the observations should be restricted to those within +/- 4 hours of the cross section
We agree. Updated figures in the revised ms show model-observation comparison in two ways. Firstly, with model data at a fixed time along a fixed latitude/longitude. Here CMET data is now only shown for time +/- 4 hours and the actual CMET location at the model time is clearly marked to make it clear where the comparison is strictly valid. This type of presentation is useful to place the observations in context of the atmospheric conditions (predicted by the model) spatially in front and behind the balloon at a certain time.
Secondly, the model data has been more deeply processed and analysed to produce 4D interpolated data along the precise CMET flight (interpolated in terms of latitude, longitude, pressure and time. This allows the CMET and model data to be more fully directly compared.

Examples of these two kinds of plots are shown below:

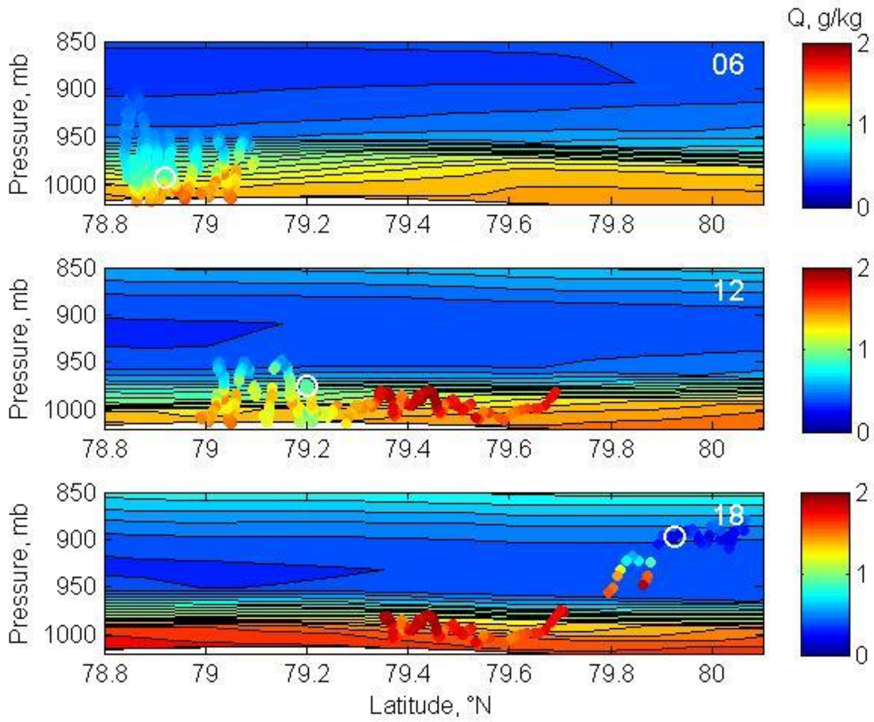

Figure A. Humidity observed along the CMET flight 5 is compared to ECMWF model output as a function of pressure, for model output at 06, 12 and 18 h UTC on 11th May along a N-S line at 11 ˚E longitude. CMET balloon location at the three model times is indicated by a white circle, with the CMET flight humidity observations shown for model time ± 4 hr.

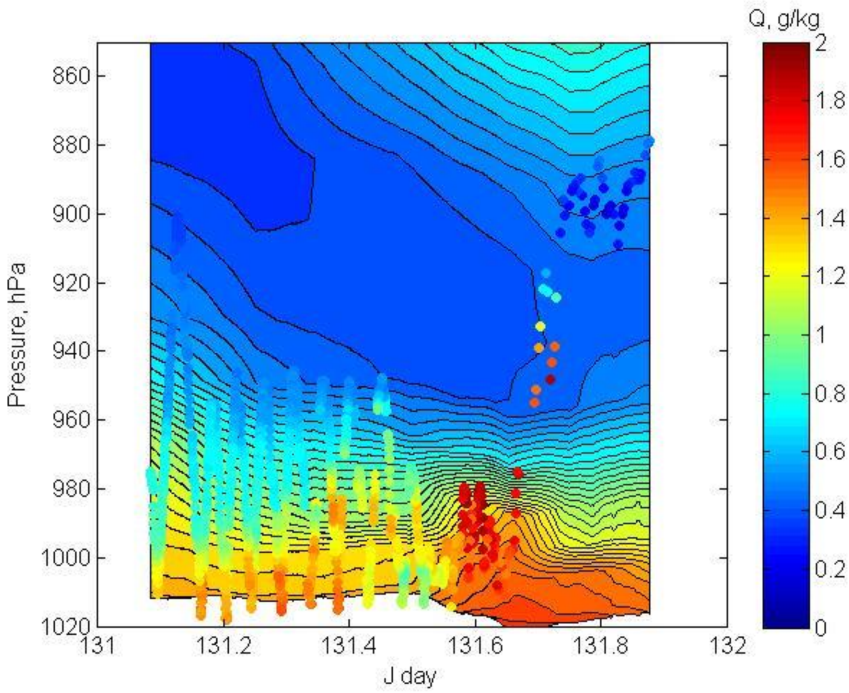

Figure B. Humidity observed along the CMET flight 5 is compared to ECMWF model data as a function of pressure. Model data has been 4D-interpolated to the in-flight CMET locations (latitude, longitude, pressure and time) for the period 02-22 h on 11th May.

p27573, figure 12 is difficult to read. This is perhaps an attempt to show too much information on one figure. The trajectories launched from a given height (black lines) seem fairly regular. Perhaps the authors could obtain a figure that is easier to read by showing only the balloon track and the final positions?

In our original ms the description and presentation of this figure was not clear. This figure 12 presents a wind field analysis based on one CMET flight only (flight 5, with continuous automated soundings). The 3D trajectories presented in Figure 12 (at specific heights) are calculated based on winds determined from the CMET balloon during the in-flight soundings. Wind vectors at the specific heights are analysed from each of the soundings and placed end-to-end to produce a wind-field that predicts the overall air-mass trajectories at each specified height. Note that the analysis to produce this figure includes an assumption of (horizontal) spatial homogeneity. In the revised ms we will improve the text for clarity and also present 2D figures alongside the 3D plot to more clearly illustrate these wind-field trajectories from different view points.

Comment from GJ Steeneveld

This is an interesting study that documents the WRF model performance against a new measurement technique for the challenging Arctic region.

Just two remarks:

In the study you use the WRF 3.3.1 version. It is known that WRF versions older than the release 3.4.1 is suffering from a bug in the YSU scheme concerning the stable boundary layer. It appeared that the stability was not correctly activated. This results in too deep stable boundary layers, with low levels jets that are too much diluted (thick and low wind speeds).

These have been documented in:

Sterk, H. A. M., G. J. Steeneveld, and A. A. M. Holtslag (2013), The role of snowsurface coupling, radiation, and turbulent mixing in modeling a stable boundary layer over Arctic sea ice, J. Geophys. Res. Atmos., 118, 1199–1217, doi:10.1002/jgrd.50158
Sterk, H.A.M., G. J. Steeneveld, T. Vihma, P. S. Anderson, F. C. Bosveld, A. A. M. Holtslag, Clear-sky stable boundary layers with low winds over snow-covered surfaces. Part 1: WRF model evaluation, Quarterly Journal of the Royal Meteorological Society, 2015, 141, 691, 2165.
Xiao-Ming Hu, Petra M. Klein, Ming Xue, Evaluation of the updated YSU planetary boundary layer scheme within WRF for wind resource and air quality assessments, Journal of Geophysical Research: Atmospheres, 2013, 118, 18, 10,490.

Although I believe the model biases that are shown also are the result of other aspects of the modelling effort, perhaps it is worth checking.

-A second question is related to the land/snow-atmosphere coupling. The representation of the complex process of how to represent the heat and moisture transport from the subsurface and the land surface to the atmosphere is crucial. Do the model results remain the same in case another land-surface scheme is used?

We thank GJ Steeneveld for these insights to the WRF model and for the references provided. We agree this poses a limitation to the WRF modelling of our study in investigation of the YSU BL scheme. In future work it would be of interest to further investigate land-snow-atmosphere and also sea-ice-snow-atmosphere coupling through WRF model sensitivity studies.

However, as suggested by reviewer 3 we now re-focus this study on CMET comparison to ECMWF data, with a view to making further regional WRF modelling in future for a more detailed high resolution comparison and WRF model sensitivity study. For this intended follow-up study the reviewer's comments and suggestions are very valuable.

Comment from Anonymous Referee #3

This paper presents the use of a new balloon technology to obtain numerous vertical soundings during a flight, an extremely useful tool for improving knowledge of weather with links to to climate in the Arctic region. Then, the balloon data is compared with the regional weather model WRF run during the same period. The strength of this paper is the measurements, there are some concerns about the WRF modeling that make it difficult to draw conclusions about the model performance as already pointed out by the short comment by GJ Steeneveld. This is a really interesting and exciting measurement technique and upon addressing the major and minor comments, the paper should be accepted as it's well within the scope of ACP.

We thank the reviewer for their comments, who highlights that the strength of the study is in the CMET measurements and raises some issues with the WRF modelling.

Major comments:

- Recently a large problem in the surface skin temperature for the Noah Land Surface Model (LSM) over snow/ice was discovered and corrected in the most recent version of WRF (see comments for most recent WRF release 3.7.1 found online http://www2.mmm.ucar.edu/wrf/users/wrfv3.7/updates-3.7.1.html). This issue combined with the issue pointed for the YSU boundary layer scheme (noted in the short comment by GJ Steeneveld) make it clear that the model should be re-run and compared with the CMET data using the most recent WRF version, where these bugs have been corrected. In addition, the authors note they did not use fractional sea ice, which is currently commonly used for runs in the Arctic region. Finally, the authors didn't use any type of restart or nudging for the outer domain, which is also commonly used to ensure large scale meteorological features don't diverge from ECMWF.
- Rather than fixing all of these issues with the WRF run, it would be easier (and possibly even more convincing) for the authors to focus on the measurements and compare the CMET results with the meteorological forecast provided by ECMWF directly (currently these are used as the boundary conditions and initial conditions for their WRF run). Then, the authors can focus on the measurements and how they compare with ECMWF, the meteorological features that determine the balloon movement, and where the measurements and ECMWF do not agree, pointing to where the model can be improved in the future.

The reviewer has highlighted a number of issues in the WRF modelling that need to be addressed. We agree with the reviewer's suggestion to re-focus this ms on CMET balloon observations that can be compared to ECMWF model output.

- The paper will be much stronger if the authors use the data to evaluate and improve WRF in a second paper mostly focused on modeling.

We agree and will likely plan a follow-up WRF modelling paper including sensitivity studies for example on air-sea-snow-ice and land-snow-ice coupling, on boundary layer schemes, etc. The precise scope for any follow-up work will be made at a later date.

Minor comments:

- The paper should be re-edited for clarity and wording

As part of the revisions, the ms is being further checked for clarity.

- I would suggest to move the meteorological overview from the supplement into the main text of the paper.

We agree. This meteorological overview is now presented at the start of the Results section to place the CMET flights in context.

- A few more details of how Figure 12 was generated and some more discussion of what this figure shows are needed.

We agree. Please see our response to similar reviewer comment above.