Atmos. Meas. Tech. Discuss., 7, C2308–C2315, 2014
www.atmos-meas-tech-discuss.net/7/C2308/2014/

Atmospheric Measurement Techniques
Discussions

Interactive comment on "Global Hawk dropsonde observations of the Arctic atmosphere during the Winter Storms and Pacific Atmospheric Rivers (WISPAR) field campaign" *by* J. M. Intrieri et al.

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Received and published: 21 August 2014

Referee #3

Hello Reviewer #3, Thank you for providing detailed and comprehensive comments on the paper. The information you provided has improved the text in many ways so, we greatly appreciate the effort that you put into reviewing the manuscript. Please see below for point-by-point responses and text/figure edits that were made in response to your comments.

Major issues:

1. The comparison to reanalysis data is now given in the end in Sec. 4.3. It would be much clearer to move the general explanation of the reanalysis data (first paragraph of 4.3) to a new section 3. The comparison to the polar vortex and boundary layer structure can then be added as subsections to what is now 4.1 and 4.2, respectively. This is because when looking at the figures one has already done the comparison to the reanalysis by eye, and it makes sense to discuss this right away.

The organization you proposed was actually close to our original overall structure. This structure was changed based on previous editor's comment. The authors feel that both reviewers comment work but, have decided to keep the sections in their current order given the flight track explanations work well in either order.

2. A more complete and balanced description of the capabilities of the platform (Global Hawk) should be given. Right now there are several sentences in the manuscript that read in my opinion too much like unbalanced praise, and the limitations are only mentioned on the side (such as the problems with cold temperatures). I would recommend a paragraph that more clearly describes the actual niche that can be taken by the Global Hawk, compared to manned research aircraft. This should cover aspects such as which airspace is actually available to drones according to current legislation, nationally and internationally, what the cost per flight hour is, which airports can be used, which flight levels or altitudes the drone is allowed to operate in, how much planning in advance is required. For example, is it feasible to plan a flight one day or less ahead of the launch, or is the drone more suited to pre-programmed repeatable flight patterns?

We have added text in Section 2 that describes more on the GH operations including why the wedge area was defined, the fact that no drops were allowed over land areas, the 7 day lead time to file flight plan, the 24 hr window for mods, etc. However, other details such as costs per flight, airports, allowable flight levels, etc. were not included since this information is basically determined individually for any given flight area and would change accordingly. We also felt that the information was not critical to the paper's main presentation points.

3. Give more concrete objectives for the WISPAR mission, what has been targeted, in what aspect? There is no presentation of the drop sondes from the atmospheric river in the manuscript, this should be clearly mentioned. On the same note, the title is therefore somewhat misleading in my opinion, since it prominently states "winter storms and Pacific atmospheric rivers" but nothing to that effect is actually presented. One solution may be to leave the acronym in the title, but to remove the full description. Also, in the abstract it should be clarified that no material relating to the meaning of the WISPAR acronym is presented in the manuscript.

The use of the phrase "winter storms and Pacific atmospheric rivers" in the title was merely to spell out the acronym of the field program. We have edited the title as suggested by the reviewer and added information in the text to clarify that only the Arctic flight is being discussed in this paper.

4. I am not sure what the authors make a point about validating reanalyses. Usually one would want to ingest all available observations into the data assimilation system, rather than acquire separate data to perform validation. Data denial experiments may be a possibility to do such validation. Also, the data sparsity of the Arctic is not significantly larger than in many other areas of the world. For example, there are a number of regular sounding stations in the Arctic (see attached figure 1), and polar orbiting satellites provide quite dense observations due to overlapping tracks. It surely is a harsh environment, but probably more densely sampled than for example parts of the southern Pacific. Reanalyses are frequently used to conduct studies on Arctic climate over extended (e.g. decadal) time periods. Such evaluations are carried out despite very limited information on the quality of the atmospheric representation delivered by these products. Measurements such as those coming from WISPAR, which are not uploaded to the Global Telecommunications System (GTS), provide unique opportunities to evaluate the performance of reanalyses at high latitudes. We agree with the reviewer that ingesting all available observations is favorable for producing the best reanalysis product. However, data denial experiments are generally configured to give insight into

the importance of specific observations on a forecast, but are unlikely to allow for a fair evaluation of the realism of such a product. With regard to the reviewer's comments about data sparsity in the Arctic, we do agree that the Southern Ocean is likely less sampled than the Arctic. The fact remains that there is a void of upper air stations over the Arctic Ocean. While that gap is partially filled by information from polar orbiting satellites, not all reanalysis products take full advantage of those measurements. Additionally, polar-orbiting satellites also leave a gap in measurements north of $\sim$82 N. We have tempered our language in the manuscript (replacing data-void with data-sparse) with regard to the lack of measurements in this region, but we do believe that the region is under-sampled relative to much of the northern hemisphere.

5. Fig. 4 and the respective discussion should be split into two parts. A first part could compare the (vertically enlarged) interpolated drop sonde curtains to differences for the same curtains interpolated from reanalysis data. A second part in a separate figure could focus on the 11km transects. You could make the point here what targeted observations may have to offer if the jet position or strength, for example, show marked differences to the reanalysis.

Because the curtains illustrated in the top part of Figure 4 include a temporal component, we deemed it to be challenging and unfair to perform a direct comparison with reanalyses (even/especially with temporal interpolation of the reanalysis output). It is for this reason that we provided reanalysis output from both the 0600Z and 1200Z times for comparison with the dropsonde measurements in the bottom half of Figure 4. With respect to the lower panels, we feel as though they show that the reanalyses actually do a good job in recreating the position and strength of the jet – if anything, it is the center of the vortex that is poorly represented, with the reanalyses having wind speeds that are too high and wind directions that are more northerly than the measurements indicate. Having said this, these errors are relatively small (wind speed error $\sim$ 5 m/s; direction error <15 degrees). The vortex center is furthest from the locations of upper air sounding stations, and therefore the reviewer's point on the utility of targeted observations appears to be valid –this has been pointed out in the manuscript.

6. All figure panels should have labels, and be referred to by the panel label from the text. I suggest to use a normal weight font in all figures. Some figures are too small and need to be split and enlarged (Fig. 4-7, see detailed comments).

We feel as though it is easier to navigate the figures when they are labeled by the quantity being shown. We agree with the reviewer that the labels on Figure 4 are too small and have enlarged them. With regard to using bold vs. normal weight font, we believe that the axes are easier to read when bold and will leave this to the discretion of the editor. If the editorial staff agrees with the reviewer that the fonts should be normal weight instead of bold, we will revise the figures accordingly. While we appreciate the reviewer's feedback on splitting Figures 4-7, we believe that their current configuration provides consolidated views on the topics that aligns with the distribution of discussion of those topics in the text and have therefore left them as they were.

Detailed comments: P 4070 L 24: This is a long list of objectives, and should be more focused - validation of reanalyses seems a bit far fetched. Most direct are rather targeted observations, i.e. (3). (2) is not exemplified in the paper altogether.

We have replaced "validation" with "comparison". Also, this list is introduced with, " Ultimately, routine UAS observations can ..." so, it's meant to be more broadly interpreted than just relating to the analysis from this one case. However, we have edited the sentence to temper the assertion (e.g. deleting "substantial"), in response to the reviewers comment.

P. 4072 L. 6: Report about any general parameters (p, T, q, images, radiation) that may be measured in situ by the NOAA/NASA Global Hawk. Listing the in situ GH observations have been added to the manuscript as suggested. "Standard in situ observations acquired by the GH include air speed velocity, pressure, temperature, as well as system parameters including skin temperature, fuel temperature, etc. and an HD camera that forward-looking imagery to provide situational awareness for the plane and scientific

goals.

L. 20: give frequency in Hz Added as suggested.

L. 22: give the exact weight Provided as suggested.

L. 23: what is the fall speed of the sonde? At which altitudes does which vertical resolution apply? What is the vertical resolution when taking into account time constants of the sensors? A table detailing the sensor package may be helpful here. The following sentence was added to section 2: "The sondes have a fall rate of between $\sim$30 ms-1 at 15 km and 0 ms-1 near the surface." A table was not included.

P. 4073 L. 4: give as minimum and typical separation time Text was added to describe the minimum drop and separation time at the most commonly used altitude. L. 9: using established post-processing methods Added as suggested. L. 18: which kind of hygrometer (see comment above)? Added as suggested. L. 25: Indicate the AR with an arrow in Fig. 2 Arrow added as suggested indicating AR on Fig. 2

P. 4074 L. 11: "has been corrected in future sondes" - rephrase. How has it been corrected? Rephrased to indicate it was an communications issues solved through engineering fixes.

Fig. 4: How have the curtains been interpolated, at which resolution have the dropsondes been used? All of the dropsondes taken on the N and S legs were used. This is supported by the latitude values on the axis and the corresponding "dots" that are color-coded and reference back to Fig. 2 (also described in the caption). P. 4077 L. 26: delete "etc" Deleted as suggested.

P. 4078 L. 4: delete "and more" Deleted as suggested. L. 14: As far as I know ERA-Interim has 60 vertical levels. 11 grid point - do you mean 11 vertical levels? The 11 grid points (or levels) refers only to the lowest 3 km (as included in the text).

L. 20: "amongst other things": delete and rephrase Deleted as suggested.

P. 4080 L. 4: "A look at individual profiles": guide the reader, using panel labels, and mentioning colors, quantities, etc. Text added as suggested to guide reader wrt Figure 6.

L. 15: The lead is most certainly a feature that is not in the reanalysis, since it is a subgrid-scale feature that would require a dedicated parameterisation. You may want to check how sea ice is assimilated in the reanalysis products.

Text added: Discussion on the handling of sea ice in reanalyses: For this date, ERA-Interim gets its sea ice concentration from the Operational Sea Surface Temperature and Sea Ice Analysis (OSTIA; Donlon et al., 2012), which takes satellite measurements and produces a sea surface temperature and sea ice concentration product at 1/20 of a degree. R-2, on the other hand, utilizes AMIP-II sea ice boundary conditions (Hurrell et al., 2008). This product is based on the monthly mean HAD-SST and the NOAA weekly optimum interpolation SST analysis. This product only provides monthly mean sea ice concentration data on a 1 degree grid. L. 24: One critical aspect is what the cost per sonde is compared to ground based soundings, taking into account the aircraft operations overhead. This type of comparison/cost analysis was not included in the paper. Given it's a demonstration technology, costs can decrease rapidly, if and when, it is used more routinely.

P. 4081 L. 1-5: This summary paragraph appears somewhat one-sided and overly positive for a scientific manuscript. Point well-taken, however, it was the first successful deployment at high latitudes. We changed “extreme” to “atmospheric” in the second sentence to temper the enthusiasm.

L. 15: It is certainly nice to have a far-reaching platform to deploy sondes, but it needs to be mentioned that there are limits with respect to the airspace where this can be done. Also, it remains a critical aspect to identify the right locations where targeted observations can be performed. Some literature references should be given here, or earlier on in the paper.

That is a very valid point. Text added in Summary section to address the limitations mentioned above.

Fig. 3: I cannot distinguish any sea ice features from cloud in the MODIS image. Maybe a true color composite with adjusted contrast is more helpful. Add arrow indicating flight direction. Mark sondes later on shown in other figures, e.g. by dots or crosses in the circle centers.

The MODIS image was removed from Fig. 3 so that it now only shows the flight track. Flight track times on the figure indicate aircraft direction. Text was added to describe the 3-6-7 color scale. The dots are color-coded to relate to sondes described in other figures.

Fig. 5: Make top panel a separate Figure, and enlarge. Again, leads can hardly be seen in this false color composite. Enlarge panels 2-5 and arrange in a square layout.

That is a good suggestion and we will increase the size of the top panel. However, Figure 5 is currently configured to fit into a single column so, we will keep the 4-stack configuration.

Fig. 6: Increase vertical extent of top row panels by factor of 2 The data did not extend that far and is shown at its full extent of $\sim$13 km.

Fig. 7: There are many more panels shown than are actually discussed. Consider reducing to what is actually discussed in detail. The authors have ensured that all panels are discussed in Section 4.3.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 4067, 2014.