Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-396-AC1, 2016

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A comparison of Loon balloon observations and stratospheric reanalyses products" *by* Leon S. Friedrich et al.

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Received and published: 16 September 2016

We would like to thank the reviewers for their insightful questions and suggestions for improvement that they have provided. In the remainder of this document bold text identifies responses to reviewers comments.

Author team.

Reviewer 1 Comments and Responses In this paper, measurements gathered dur-

ing stratospheric long-duration balloon flights performed in the frame of Google Loon project are compared to reanalysis products. These measurements were not assimilated by Numerical Weather Prediction systems and thus provide an independent dataset that can be used to assess reanalysis accuracies. The 70 Loon Balloon used in this study flew in the Southern Hemisphere lower stratosphere. The study focuses on wind and trajectory comparisons since primary observations provided by Loon balloons are balloon positions, from which horizontal wind components are derived. Since reanalysis products are widely used to e.g. study transport processes in the lower stratosphere, this study is particularly relevant to have independent information on their accuracies, which is otherwise difficult to get with more classical datasets that are generally assimilated. I have found that the material and figures of this article are generally well presented, and in my mind, the article adresseses topics that are of much interest to the ACP readership. Furthermore, one can hope that Google Loon will continue flying longduration balloons in the future, and such study is particularly useful to demonstrate the potential of observations obtained with such flights. Yet, I have the impression that the article could be significantly improved if, in several instances, further information were provided. I also think that there is a flaw in how lift-gas temperature measurements are treated in this study. I would therefore encourage the authors to carefully address my remarks below, and would recommend publication afterwards.

Main issues 1. Balloon dataset: I would appreciate if you could provide (perhaps in Figure 1) an histogram of balloon pressures and altitudes. It is important to know whether the balloon measurements are representative of a specific thin layer of the atmosphere or do indeed provide homogeneous information on the 30-70 hPa layer as stated in p4, l15.

A histogram of the distribution of pressure observed over the entire flight period has been added as panel (d) of Figure 1 to address this point. See the updated

Figure (Figure 1 at the end of this document) and the updated caption below.

Figure 1: General Loon flight information including (a) set of all balloon trajectories viewed from south pole, (b) time-line showing individual balloon launch times and flight durations, (c) histogram of observation distribution as a function of latitude, and (d) histogram of observation distribution as a function of pressure.

2. Ballon vertical excursion: in p4 l21, it is stated that "whenever a pressure change greater than 5 hPa occurs within one hour, the balloon is considered to be undergoing an altitude control manoeuvre". Could you provide an illustration of either pressure or balloon altitude timeseries that shows such manoeuvre, and clearly displays which part of the dataset is discarded?

The author team does not see that this has value as a new Figure within the paper. However, we show one example in Figure 2 at the end of this document which shows the excluded region of measurements in red and the data used in analysis around this transition in black. We hope this reassures the reviewer that the methodology used is robust.

3. Sensor precisions: observations performed on Google balloons were likely not primary intended to provide scientific-class measurements, and stated sensor precisions (p4 l28) are rather large compared with current state-of-the-art meteorological measurements. This is not an issue in itself provided that the impacts of the fairly large measurement uncertainties are precisely assessed. This aspect needs to be improved in the current manuscript:

âĂć First, if one assumes that the uncertainty on the GPS horizontal position is 10 m

(as mentioned in p4, l28), and furthermore that the uncertainties on the two positions separated by t that serve to compute the winds are independent (which is not explicitely stated), then the uncertainty on the derived wind should be 10 p 2=t = 0.23m/s, with t = 1 min (p26, l4). It is only when p 2 is replaced by 2 than one comes to the 0.33m/s value reported in the paper, which I do not understand.

We thank the reviewer for spotting this error. We have replaced this number in the updated document.

âĂć The pressure measurement is used as the vertical coordinate in the interpolation of the reanalysis product onto the balloon position. As stated in the paper (p4., l28 ôĂĂĂ 31), a 1:5 hPa uncertainy in these measurements is "rather large" and "could potentially lead to uncertainties when vertically interpolating the reanalysis data sets to the balloon locations". While likely true, this sentence stays very qualitative. It would be much helpful if a typical vertical wind shear could be assumed so as to infer a resulting uncertainty on the interpolated wind. When all these measurement/interpolation uncertainties are properly taken into account, one can better know which part of the differences between the balloon observation and analysis is due to the observations or to defficiencies in the analysis (Section 3.1, and Figures 3 and 4).

A back of the envelope calculation using the hydrostatic equation shows that a 1.5 hPa uncertainty equates to about 300m in altitude. Given a 3.0m/s change over 2km at the bottom of the stratospheric jet in the Southern hemisphere winter (approximated from ERA-Interim climatology) this equates to about 0.4m/s at worst case. This information has therefore been added into the updated document. However, the reality is that this uncertainty would only impact the spread and the findings in this paper are comparable to previous studies. The structure of the errors in Figure 2 also suggests that it is not consistent biases that

make up the differences observed - more a lack of high frequency detail in the reanalyses.

We have added the following text into the updated manuscript: "Using the hydrostatic equation shows that a 1.5 hPa pressure uncertainty equates to about 300 m in altitude. Given a 3.0 m/s change over 2 km at the bottom of the stratospheric jet in the Southern hemisphere winter (approximated from ERA-Interim climatology) this equates to about 0.4 m/s at worst case."

4. Could you also provide confidence intervals in Figure 4, and state which values are significant in Table 2? And please provide only significant digits in this table.

We have recreated Figure 4 and added the 99% confidence interval around the ERA-Interim mean (black dotted line in Figure 3 at the end of this document). Examination of this figure shows that the confidence interval is similar to the width of the line representing the mean value. The confidence intervals on the standard deviations are also similarly small. Thus, rather than add these lines into the final version of the document. We have added the following text.

"Note that the 99% confidence interval associated with the biases is such that they are similar to the width of the line representing the bias."

With reference to Table 2, we have calculated the significance linked to the difference in the means of the Loon observations and the reanalysis output using the student's t test and the f test for the significance level for the differences in the variances of the distributions. In every case, the differences between the Loon observations and the reanalysis output are significantly different at greater than the 99% level, largely because of the very large number of data points analysed. We have therefore added the following text into a revised manuscript:

"However, the statistical significance linked to the difference in the means of the Loon observations and the reanalysis output have been calculated using the student's t test and the f test for the significance level for the differences in the variances of the distributions. In every case, the differences between the Loon observations and the reanalysis output are significantly different at greater than the 99% level."

5. One way to identify the uncertainty on the wind measurements is to compute the spectrum of wind disturbances and look where the spectrum becomes flat at high frequencies. The raw timeseries could then even be filtered to eliminate the high-frequency noise, and comparisons with the reanalyses could be made with these filtered timeseries, which would more accurately estimate the analysis defficiciencies. I therefore think that providing the wind spectrum would be a very valuable addition to the article.

The spectral form and the make-up of the errors is the subject of ongoing work to be developed into another paper. Thus, the author team feel that this suggestion, while of interest, is outside the scope of the current work.

6. As mentioned on p5, l3 -5, the temperature measurements provided by Loon balloons are those of the "lift gas", and not of the ambient air. In Section 3.3, the authors use an empirical method to correct the lift-gas temperatures from observed diurnal variations that are implicitly assumed to be spurious, and claim that "this [method] is commonly used to correct balloon based temperature measurements". While it is true that such method has been previously used (articles cited in the paper), it was solely used to correct air temperature observations in the lower stratosphere. Its use to correct lift gas temperature measurements, as done here, is more questionnable: one assumption of this method is indeed that the undelying 'true' temperature is not ex-

hibiting diurnal variations (or that the diurnal cycle is less than the sensor uncertainty).

No, the method does not assume that the 'true' temperatures do not exhibit a diurnal variation. Quite the contrary. It simply assumes that the bias between the true measurements (which show a diurnal variation) and the lift gas temperature measurements (which also show a diurnal variation) is a function of the solar zenith angle only. The robustness of the statistical model that describes these differences demonstrates that this is a valid assumption.

I doubt that this is the case for the lift gas temperature: the balloon envelop certainly absorbs to some extent the sun radiations, which would unavoidably lead to an increase of the gas temperature during day. It is certainly true that the temperature sensor itself absorbs these radiations, and thus overestimates the gas temperature diurnal cycle. But correcting the measurements to fully eliminate the diurnal cycle is likely excessive.

We do not correct the measurements to eliminate the diurnal cycle.

I would thus recommend to discard using the temperature correction, but I would keep Figure 8, and slightly rephrase the sentence on page 9 l 20: it is not only the "quality of the Loon temperature data" which is an issue, it is also the fact that they only measure the gas temperature, which can be quite different to that of the air.

Yes, and this difference is what we correct for. We believe that the reviewer has not correctly understood the process we have applied for correcting the removing the bias between the lift gas temperatures and the ambient air temperatures.

Minor points

âĂć p2 l35 to p4 l9 is a long report on previous studies that used similar methodology than the one used in this study. I do not discuss the interest of mentioning these various studies to motivate the present work. I nevertheless think that the discussion could be somewhat synthetized and maybe re-organized by e.g. Earth regions (Northern Hemisphere high latitudes, tropics, Southern hemisphere mid/high latitudes), so as to ease the reader to get a clear picture of these previous results.

We have rearranged the ordering of this section so that the Northern hemisphere, the equatorial region and then the Southern hemisphere are discussed. But, have made no other changes apart from ordering of paragraphs given that the other reviewer does not raise this criticism.

âĂć end of introduction: could you provide the plan of your study here?

The author team do not see the purpose of creating a 'map' of the document and given that this is a stylistic point we have decided not to take-up this suggestion.

âĂć p6, l10: could you be more specific on the studies that attribute differences to inertia-gravity waves? Could you furthermore state at which latitude the timeseries displayed on Figure 2 were obtained? The frequency of inertia-gravity wave depends on latitude, and it may be worth testing that the apparent period of the short timescale disturbances in the wind timeseries indeed corresponds to the inertial period.

The attribution of the errors is the subject of ongoing work to be developed into

another paper. Thus, the author team feel that this suggestion, while of interest, is outside the scope of the current work. However, we have identified the range of latitudes related to the the time series in Figure 2 and added text to that effect in the updated document, specifically:

'The wind observations displayed in Figure 2 were collected between 31 and 48oS and cross the international date line.'

âĂć Sentences on p6 l18 and p7 l3 do not seem to be consistent: does ERA-Interim performs better than MERRA?

We think this confusion stems from us stating that MERRA has the largest mean wind difference, then claiming that it is one of the better performing ones. However, this issue is specifically addressed in the discussion of figure 4. In particular, we identify that the other reanalyses have a similar magnitude of wind bias, but the average over latitude nearly cancels.

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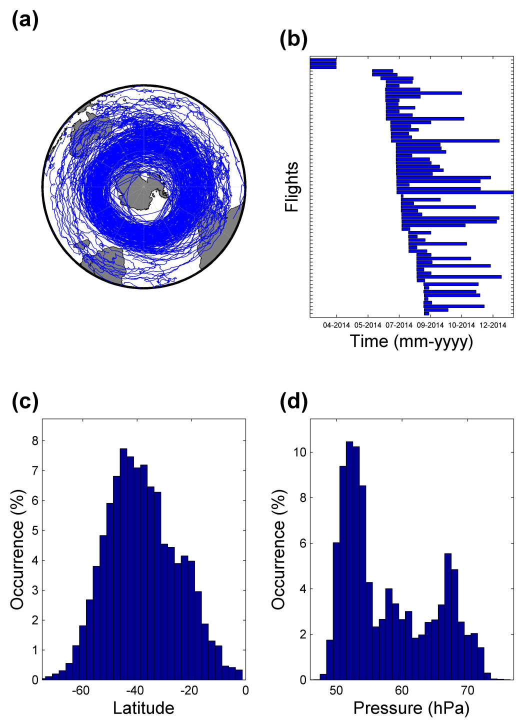

Fig. 1. Updated Figure 1

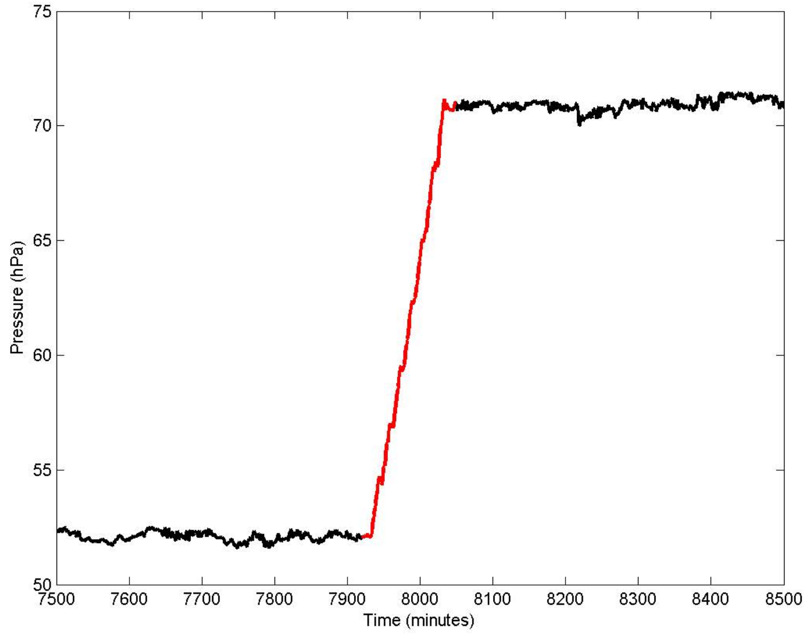

Fig. 2. Example of exclusion region after a control period.

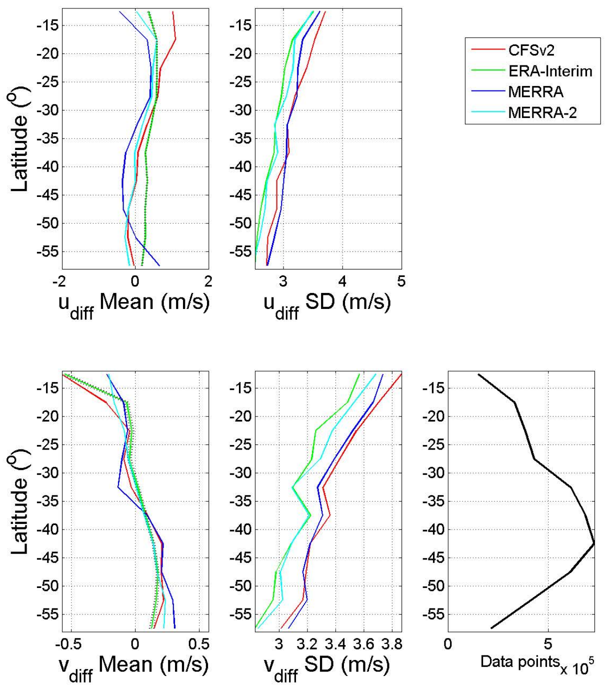

Fig. 3. Figure 4 including a confidence interval on ERA-Interim bias. Note that black dotted line is similar to width of the green line representing ERA-Interim