

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

Anonymous Referee #1

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In this paper, measurements gathered during 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,

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and in my mind, the article addresses topics that are of much interest to the ACP readership. Furthermore, one can hope that Google Loon will continue flying long-duration 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.
2. Balloon 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?
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:

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- 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 Δt that serve to compute the winds are independent (which is not explicitly stated), then the uncertainty on the derived wind should be $10\sqrt{2}/\Delta t = 0.23 \text{ m s}^{-1}$, with $\Delta t = 1 \text{ min}$ (p26, l4). It is only when $\sqrt{2}$ is replaced by 2 than one comes to the 0.33 m s^{-1} value reported in the paper, which I do not understand.
- 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 – 31), a 1.5 hPa uncertainty 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 deficiencies in the analysis (Section 3.1, and Figures 3 and 4).

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.
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 deficiencies. I therefore think that providing the wind spectrum would be a very

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valuable addition to the article.

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 questionable: one assumption of this method is indeed that the underlying ‘true’ temperature is not exhibiting diurnal variations (or that the diurnal cycle is less than the sensor uncertainty). 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. 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.

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Minor points

- 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 synthesized 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.
- end of introduction: could you provide the plan of your study here?
- 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 time-series 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.
- Sentences on p6 l18 and p7 l3 do not seem to be consistent: does ERA-Interim performs better than MERRA?

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