Answers to comments by reviewer 1

Major concerns

1. An error calculation from known uncertainties like trajectory uncertainty or smoothing length is missing and needed to evaluate the precision of the new method. The current presented comparison to ECMWF model data allows only hand-waving estimation of the precision of the measurements.

Thank you for your comment. Now the precision analysis for the density is done, which is added in Discussion Section.

2. The authors speculate about the importance of the Magnus effect on the results of their measurement throughout the whole manuscript about 10 times. A simple estimation of the magnitude of the effect is needed. Maybe the rocket spin period or the modulation period of the GPS positions would allow to estimate limits for the Magnus effects.

The Magnus effect is estimated in Section 3.1 Aerodynamic force, the ratio between the Magnus lift and the drag is around 0.004 in the extreme case, thus it can be neglected. The reference to Magnus effect is now minimized, but we still include the reference to it and the estimate.

3. Most figure captions do not give sufficient information. Only the caption to figure 6 gives sufficient information. Please expand the captions so the reader can understand what is plotted without guessing. The information is often given in the text somewhere, but there it distracts the reader and makes understanding of the figures difficult. One example: The caption of figure 2 should at least hold the definition of the angle and the residuals.

Captions are changed to make it clear.

4. In the results section the authors estimate the precision of their measurements by comparison of the rigid sphere measurements to the ECMWF model in the middle atmosphere. The discussion is mostly hand waving and lacks careful discussion. Statements like “This indicates that the calculated density is accurate, yet the accuracy is somewhat lower above 70 km than below 70 km.” (line 285) are not convincing. This approach of estimating the precision seems inappropriate. This approach seems wrong, especially since the authors state in the manuscript on line 129: “Whereas the densities from ECMWF are reliable below 50 km, as the altitude goes up, the uncertainties increase”

Thanks for pointing out this, the confusing text is changed.

Between 50 and 70 km, the ratio between the calculated density and the one from ECMWF is between 0.87 and 1.06, however the ECMWF value is not reliable, the accuracy must be analysed in another way, which is conducted in Sec. 5. Here, we can see that the calculated density is reliable below 50 km and above 70 km. If we trust the reference density completely, we can summarize that the accuracy is somewhat lower above 70 km than below 50 km.

Of course, there is a possibility of the geophysical difference between the densities observed in a point measurement from a model assimilating scarce measurement points.

Minor comments

1. The abstract is not well written. It is to short and not descriptive of the actual manuscript. It is partly just a list of basic statements that were not developed in the manuscript. One example for such a basic statement is: "Aerodynamic drag relates atmospheric densities to other variables such as velocities of spheres, drag coefficients, and reference area. "
The abstract does not give essential information like where, when, and with what vehicle the measurements were performed. It would be valuable to add information about the vertical resolution to achieve a defined precision limit as well as the altitude range where this is achieved.

The abstract is modified according to your comment.

2. Section 2.2 should be revised. There are paragraphs that consist of single sentences.

Sectoin 2.2 is revised now.

3. Line 16: Instead of "Li et al., 2013" it should read "e.g. Li et al., 2013"

The reference is fixed

4. Line 31: Did Martineau, 2012 actually measured winds or did they only used simulated data?

In fact, this thesis is a very advanced piece of work. It refers to an actual experiment, launched on a sounding rocket. Unfortunately, there are no published results from this experiment. There are a couple of AGU general assembly talks, but the only detailed publication is the thesis by Martineau.

From his thesis, they took measurements by employing rigid falling spheres, he presented the mathematical model to derive the density, but did not show the model for the winds or analyze data. In fact, there were issues with the precision accelerometers saturating due to the sphere rotation.

5. Line 66: Please clarify why these two FFUs were selected.
These two FFUs had the best quality data to be analyzed. Another FFU had data of considerably inferior quality, while the GPS raw data from the fourth FFU was lost.

6. Line 74: Please clarify if the ENU is fixed during the flight or varying with time.

It is a local reference frame varying with time, to show wind speeds visually.

7. Line 81: The enhanced accelerations in x and y need to be discussed. Otherwise the enhanced

accelerations might be misinterpreted as a problem in calculating the ENU.

Do you mean the accelerations shown in Fig.1(c)? They are resultant accelerations, which are added in the figure caption and the text now.

8. Fig.1: The x-axis scaling of subfigure c is different than the others

Fixed

9. Line 99: “the acceleration estimates are valid below 70–80 km.” This statement seems hand waving and needs to be expanded. What is the precision threshold that defines "valid"? Is the upper limit 70 or 80 km? Figure 2 does not show altitudes but the discussion of Fig. 2 would benefit from having an altitude scale.

Estimating uncertainties of aerodynamic accelerations are smaller than 0.1 m/s^2, and considering the aerodynamic accelerations are around 0.3 and 1 m/s^2 at 80 and 72 km, the aerodynamic acceleration is valid at 80 km, and becomes more precise as the altitude goes down, since the aerodynamic acceleration becomes larger.

The altitude scale is from Fig. 1(a) that is added in the text now.

10. Line 124: What is meant by "NRLMSISE-00 generated the data"?
NRLMSISE-00 is an empirical model, which can output the atmospheric data at assigned altitudes.

11. Line 125: What is the actual activity level used?
The magnetic AP index (7 different time intervals), and the F10.7 flux (2 different time intervals) were entered when we run NRLMSISE-00, although the effects of AP and F10.7 are neither large nor well established below 80 km. The indices are taken from the National Geophysical Data Center (ftp://ftp.ngdc.noaa.gov/STP/GEOMAGNETIC_DATA/INDICES/KP_AP).
The magnetic AP index input consists of:
1) the daily magnetic index (5),
2) 3 hour AP for current time (12),
3) 3 hour AP for 3 hours before current time (7),
4) 3 hour AP for 6 hours before current time (7),
5) 3 hour AP for 9 hours before current time (5),
6) average of eight 3 hour AP indices from 12 to 33 hours prior to current time (1.375), and,
7) the average of eight 3 hour AP indices from 36 to 57 hours prior to current time (11.75).

The F10.7 flux input consists of:
1) the daily average for the previous day (97.3), and
2) the 81 day average centred on the day of interest (100.47).

12. Line 126: "at the apogee" should likely be "below apogee" or something more descriptive.

It refers to the longitude and the latitude of the apogee. The whole trajectory can be projected on the surface of the Earth in terms of longitude and latitude, and we use the coordinates of the apogee point here.

13. Line 128: Please provide reference for this statement. Le Pichon et al. discuss only temperatures and winds. Le Pichon et al. state "... consitent up to ~40 km". Please clarify where the limit of 50 km comes from.

Thank you for your pointing this! The limit should be 40 km instead of 50 km. The related portions of the text are changed.

14. Figure 3: Please mark the altitude levels of the ECMWF data. The ECMWF data seems to be interpolated, please specify what interpolation was used.

The ECMWF data are denoted by '.' now. The altitude resolution of ECMWF changes between 20 and 4400 m. The altitude resolution of NRLMSISE-00 is 1000 m. The linear interpolation of the NRLMSISE-00 altitudes on the ECMWF data is used.

15. Figure 5: Judging from the difference of E2 and E3 and the location of the polar vortex (Fig 6) it seems likely that data E1 and E2, E3 are not consistent. Please check the data again.

The data have been checked again, and no problems were discovered. The difference is because the data do not cover the same longitude and latitude. Since the trajectory moved across different longitudes and latitudes, we think it is good to show some representing locations.

16. Line 201: "kgs" space or dot missing between kg and s.

Fixed

17. Line 222: "... will not be in hydrostatic equilibrium". The following discussion assumes hydrostatic equilibrium. Please clarify.

Thanks for your question. It is a typo, 'not' should be removed.

18. Figure 8: It will be useful to draw the trajectory of the actual Re and Ma numbers of the flight. The figure caption should hold the source of the data.
Fixed

19. Line 239: "Pasiaki et al.,)": Year missing

Fixed

20. Line 265: What numerical method is used to calculate these derivates?

A finite difference of the velocities is used to calculate the acceleration.

21. Line 267: The references according the wind speed in the upper atmosphere seems outdated. There are many more wind obsevations that show wind measurements in the uppper atmosphere since 1963. Maybe taking a upper limit from HWM-14 would be more up to date: http://onlinelibrary.wiley.com/doi/10.1002/2014EA000089/full

Thank you, this is now fixed.

22. Line 279: From Fig. 9 and the text it is unclear how different altitudes are treated. Please clarify, if the relative change is the absolute sum of all residuals.

To make it clear, two sentences are added.

The iteration is performed at one single altitude each time. When a density and a temperature are determined at the current altitude, the iteration moves to next altitude.

23. Figure 10: It is likely that some of the small scale structures in the density ratio is caused by interpolation of the different dataset on a uniform grid. Information is needed to clarify this.

You are right. The interpolation causes the small scale structures.

24. What is the cause of the enhanced density from LW2 and LW4 around 25 km.

This is discussed in the discussion section.

This sensitivity of the drag coefficients on the parameters introduces systematic errors, which might be responsible for the positive deviation of the density ratio at 25–28 km (see Fig. 10)

25. Figure 11: The temperatures from LW2 and LW4 look very smooth. This indicates that dependent data points were drawn. Please provide information about the actual measurement resolution and the smoothing length used to calculate density and temperature from the actual acceleration profiles. It would be helpful to plot the actual measurements grid.

The altitude resolution is between 1 and 12 m, and the actual measurement grids are plotted, but it is not distinguishable.

26. Line 310: Where does this threshold value come from? What precision of density, temperature and wind does it correspond to?

The threshold value come from fig.2(b) and 2(c). First, at apogee, the aerodynamic acceleration should be very low. Second, the aerodynamic acceleration should be opposite to the velocity.

In general, with the altitude going down, the relative uncertainty of the aerodynamic acceleration becomes smaller, the precision of density becomes higher. The corresponding precisions are 30%, 5%, 2%, 1% at 80, 70, 60, 54 km. The temperature precision should be not affected, according to the reference: http://onlinelibrary.wiley.com/doi/10.1029/91JD02395/full. The wind precision depends, if the acceleration uncertainty is in the same direction of the acceleration, the wind precision will not affected.