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Atmospheric Measurement Techniques Discussions
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# *Interactive comment on* "Atmosphere Density Measurements Using GPS Data from Rigid Falling Spheres" *by* Yunxia Yuan et al.

**Anonymous Referee #1**

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Major concerns ————————-

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.

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.

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



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Discussion paper

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.

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”

Minor comments ————————

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.

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

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

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

data?

Line 66: Please clarify why these two FFUs were selected.

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

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.

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

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.

Line 124: What is meant by "NRLMSISE-00 generated the data"?

Line 125: What is the actual activity level used?

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

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 $\sim$40 km". Please clarify where the limit of 50 km comes from.

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

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.

Line 201: “kgs” space or dot missing between kg and s.

Line 222: “... will not be in hydrostatic equilibrium”. The following discussion assumes

hydrostatic equilibrium. Please clarify.

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.

Line 239: “Pasiaki et al., )”: Year missing

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

Line 267: The references according the windspeed 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

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.

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. Infomation is needed to clarify this.

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

Figure 11: The temperatures from LW2 and LW4 look very smooth. This indicates that dependend 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.

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

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