Answers to reviewer 2

1. lines 213-214. "...as it can provide data up to 150 km." The statement appears to refer to MSIS, but the empirical model provides values to altitudes higher than 150 km. Please clarify.

Indeed, MSIS can provide values to altitudes higher than 150 km. Since our GPS data only extend to the apogee around 138 km, 150 km is mentioned in the paper. To make it clear, 150 km is removed in the paper.

2. lines 218-219. Why is there a cut-off for the drag coefficient at 95 km? The density and temperature values used to estimate the drag coefficient appear to cover a broader range of altitudes. Please clarify.

The drag coefficients come from experiment data of Bailey and Hiatt (1972). They are only available for certain ranges of Mach and Reynolds numbers, which correspond to an altitude range between 16 and 95 km in the paper.

3. lines 234-235. Why was 80 km chosen as the reference altitude? The subsequent analsysis is limited to the height range below 80 km, and that is presumably the reason for the choice, but why were the higher altitudes ignored? Please explain.

80 km is chosen according to the valid acceleration data. Please look at Fig. 2(b-c), 2(b) is the value of the aerodynamic acceleration, 2(c) is the angle between the aerodynamic acceleration and the velocity. As we know that at apogee around 115 s, the aerodynamic acceleration should be very small, but it is around 0.1 $m/s^2$ in Fig. 2(b) for LW2, thus we estimate conservatively that the uncertainty of the aerodynamic acceleration is 0.1 $m/s^2$. Above 80 km (about 224 s), the accelerations are very small, around 0.1 $m/s^2$ for LW2, and even smaller for LW4. In addition, the angle between the aerodynamic acceleration and the velocity should be around 180 degrees, but it is smaller than 150 deg above 80 km for LW2. Hence, the data higher than 80 km are not valid, therefore not presented.

To try to recover the information from higher altitudes, a complete reconstruction of attitude of the FFUs should be done, which is outside the scope of the first analysis of the data presented here.

4. line 243. Explain the J2 effect briefly to make the paper more self-contained.

Since the Earth is not a perfect sphere, the irregular shape of the Earth makes the gravitational field is not exactly central. The most important correction term is J2 term. This is now explained in the paper in a better way.

5. line 258. The assumption of zero vertical wind is a practical choice that most likely affects the density estimate most directly. Can the authors estimate the magnitude of the

potential error introduced by this assumption? The vertical winds in the mesosphere can be large, of the order of several meters per second.

Assume that the vertical wind is smaller than 1 m/s below 50 km, and smaller than 5 m/s between 50 and 80 km. From Fig. 1, below 50 km, the vertical velocity decreases from larger than 1000 m/s to about 200 m/s, between 50 and 80 km, it is larger than 1000 m/s. Hence, the relative uncertainty of the vertical velocity is smaller than 0.5%, as shown in the discussion part of the new version, which is smaller than uncertainties of the aerodynamic acceleration and the drag coefficient, thereby having only a minor effect on the density. The error transfer is discussed in the new version.

6. lines 282-285. Ratios of 0.87 to 1.07 for the density do not represent particularly good agreement. Objectively, this could represent the difference between cyclonic and anticyclonic flow, for example. "This indicates that the calculated density is accurate..." Quite the contrary seems to be the case.

It is a challenge to evaluate the calculated density exactly. The ratio is with respect to the ECMWF value, however, this model is has some uncertainties. In the new version, the error transfer is analyzed in the discussion part. The uncertainties of the densities in the extreme cases are 37%, 9.3%, 5.5%, and 4.5% respectively, at 80, 70, 60, 54 km.

7. lines 287-289. Similar comments apply to the discussion of the temperature comparisons, although the comparison with the lidar measurements make the differences even more problematical since the lidar data represent an actual measurement rather than a model estimate.

It is also challenging to evaluate the temperature exactly. It is good to have lidar measurements. Unfortunately, because of tropospheric cloudiness, lidar information was not available concurrent with the LEEWAVES launch. Instead, comparisons in this paper are based on lidar measurements obtained during the day prior to the launch. The temperature precision of a falling sphere are not affected, according to (Schmidlin et al., 1991), the density bias does not affect the temperature.

8. lines 330-331. Is the conclusion warranted? If such large differences are acceptable, what is the objective basis for making that determination?

The formulation is changed in the paper.

9. Some of the differences between the falling sphere values and the independent measurements and model estimates could be due to geophysical variations rather than instrumental error. Discussion of such effects would be helpful.

That makes sense, since the independent measurements and the model estimates are not exact at the same moments or in the same areas as the measurements of the falling spheres. We extended the discussion in the paper now.