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Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #2

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General comments:

The authors present the results from the first analysis of two active falling sphere measurements covering altitudes from approximately 80 km to the surface. Although falling spheres have been used extensively in the past to obtain density, temperature, and wind measurements, the instrumented rigid falling spheres used by the authors represent a new development in the technique with potential improvements in the measurements.

The description of the instrumentation is adequate, although not very detailed for a measurement techniques article. The authors provide an excellent literature review of similar measurements made in the past, going back to the early days of aeronomy and space studies.

Overall, the technique and the description of the measurements are interesting. My concern, however, is the general characterization that the results are in good agreement or general agreement with the independent measurements or model estimates for the same time and location. There is clearly good agreement below 20 km, but presumably, that is not the altitude range where this technique will be most valuable. Above 30 km, there are very large differences between the falling sphere estimates and the other profiles. The density ratios are the most difficult to assess since the only basis for comparison are the values from a dynamical model (ECMWF) and an empirical model (NRLMSIS). Nonetheless, difference ratios exceeding 10% would concern me and would certainly be expected to affect the dynamics significantly if the measurements are used for that type of analysis. The lidar data actually provide a temperature measurement that can be compared directly to the falling sphere values, and again, significant differences of 30K or more are found. The wind estimates are difficult to assess since only model comparisons are available above 30 km. The model winds at a specific location are probably the least accurate of the parameters used in the comparisons.

The description of the analysis technique, the first results, and the available comparisons should be published, but the results should be characterized more realistically. The results from the first analysis presented here do not compare particularly well with the available independent measurements and model estimates, but they indicate the potential of the technique. The authors suggest improvements in the analysis technique that can be applied to refine the results. Perhaps those refinements can produce better agreement and should be emphasized in the discussion.

Specific comments:

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.

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.

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.

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

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.

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.

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.

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

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.

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