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Atmospheric Measurement Techniques Discussions

Interactive comment on "Vertical air motions derived from a descending radiosonde using a lightweight hard ball as the parachute" *by* H. Chen et al.

Anonymous Referee #3

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The manuscript "Vertical air motions derived from a dropsonde using a lightweight hard ball as the parachute" by C.H.B., Chen et al. introduces a new dropsonde type for measuring vertical wind speed. The authors claim that the added value of their new dropsonde is related to the spherical symmetry of the parachute allowing an easier deduction of appropriate drag coefficients to be used in the calculation of the dropsonde descent rate and, therefore, in the retrieval of the wind vertical velocity. This is stated in a few lines reported in section 2 where the spherical symmetry of the dropsonde is presented in opposition with the traditional dropsonde design. The introduction of a new technique is a very important moment for the scientific community towards the

improvement of our knowledge of the atmospheric processes. To this purpose, the new technique should be presented along with measurements, simulations (when needed), comparisons and tests able to show the advantages and the improved or comparable performances of the new technique with respect to what has been already presented in the past literature. However, the measurements reported in the manuscript mainly aim at showing the sensor stability but there are not enough information to quantify the real advantages in using this dropsondes respect to previous designs presented in literature. There is only one comparison with a ground based wind profiler: the comparison of vertical wind velocity profiles showed in Fig. 9, limited to altitude below 5 km above the ground, is quantified by the authors as reasonable, also considering both the presence of large uncertainties in the measurement of wind when small values are measured and the uncertainty due to the collocation of atmospheric measurements. The comparison, instead, reveals large differences, partly hidden by the poor quality of the same figure. Moreover, when small values are detected the agreement looks much better than for larger wind values. This is clearly visible by the comparison of the values of the vertical wind in the boundary layer, in contrast with the authors' considerations reported in the manuscript. The manuscript is also quite short, includes repetitive sentences and, as already highlighted in the first stage of the review process, and it looks more suitable for an extended abstract of a conference or for a report than as a scientific publication. A larger number of comparisons with other techniques should be provided to allow the reader to identify the effective advantages in using the new dropsonde design. This will largely improve the quality of the manuscript. Minor issues, in the general frame of the manuscript, but highly relevant, are the absence of any quantitative estimation of the uncertainty affecting the estimation of wind velocity and the lack of a description of the sensors used to measure the temperature and relative humidity, shown in Fig. 9. In conclusion, I ask a major revision of this manuscript and I think the manuscript can be accepted only if a more extensive characterization of this new dropsonde type will be provide by the authors. I also suggest the authors to read more about the other techniques mentioned in the manuscript for the measurements of ver-

tical wind velocity and about the step forward and the high accuracy already achieved with them (e.g. cloud radars).

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 8107, 2013.