

Review of “Modeling the ascent of sounding balloons: derivation of the vertical air motion” by Gallice et al. (2011)

Overall recommendation: accept after minor revision.

The authors developed a new model to calculate the radiosonde balloon rise rate in the still air. The uniqueness of the model includes taking into account the changes of the drag coefficient with altitude and the heat imbalance between the balloon and the atmosphere. We raised these two issues in Wang et al. (2009) and are glad to see that this manuscript addresses them. The authors give a very good review of these two issues along with other things from literatures in different disciplines. I would also like to praise the authors for conducting detailed uncertainty analysis, which is essential for evaluating the performance of their model and for users to know. The manuscript is also well written, and the analysis is thorough. The manuscript has high quality both technically and scientifically. However, I have the following general comments that would help the authors improve the manuscript. Based on my evaluation, I think that the manuscript can be published after minor revisions.

General comments:

1. Given the fact that the study only considers the diffusive heat transfer between the balloon and the atmosphere, the model is only applicable to nighttime soundings because of the significant radiative heat transport. Is this true? If so, this should be noted explicitly in the abstract and the conclusion, so the users would not mistakenly use it for daytime samplings. This would also limit the model's applications. It would be helpful to make some qualitative estimation of the impact of radiative heat transport and apply the same procedure to the daytime soundings to see how good or bad the model is. Some kind of assessment of the model's applicability to daytime sounding would be very helpful.
2. The first application of the model is to forecast the balloon trajectory. It would be interesting to compare the forecasted balloon trajectory using this model (maybe other models too) with the measured one (GPS positions) to show how good the prediction is.
3. In section 2.3.1 and 2.3.2, the authors had a detailed summary on some theoretical and laboratory studies on what affects the drag coefficient. One of main factors is turbulence intensity. Then in Section 2.3.3 the authors derive the drag curve using experimental flights. It would be interesting to connect these two parts. For example, the authors can make some qualitative explanation on why generally the ascent rate from LUAMI increases from the surface with altitude, reaches the maximum around ~10 km, decreases with altitude, reaches the minimum around tropopause (~12-15 km), then increases again in the stratosphere. Is this related to known turbulence intensity changes?
4. The model shown in Fig. 4 includes two major improvements comparing with previous studies: more realistic modeling of the balloon effective radius and taking into account of the variation of  $C_d$  with altitude. Did you do some sensitive studies on which one contributes more to better estimation of the ascent rate? This would have some implications on what is more important for future improvements.

Specific comments:

1. P3971, L2-3: Why is the drag coefficient defined with respect to  $S$ ? Does this mean that the draft coefficient is a function of  $S$  (or  $R$ )?

2. P3977, L20-25: Here you raised two other issues, “radiative heat transport into the balloon” and the type of balloons. This means that your results can only apply to night time soundings and that particular type of balloons, right? Did you apply the same procedure to your daytime data to see whether solar radiative heat transport has significant impacts on Cd? The same can be applied to soundings using different type of balloons.
3. P3979, L17-18, “the presence of a parachute and a payload attached to the balloon”: Does it help if you add the weights of the parachute and payload to your total weights?
4. Fig. 6a: One problem with the reference drag curve is that it is too smooth to represent the sharp transition around 2km and 10 km. Does it help if you increase the order of polynomial fit, third or fourth order?