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Atmospheric Measurement Techniques Discussions

Interactive comment on "Evaluation of wake influence on high-resolution balloon-sonde measurements" *by* J. Faber et al.

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We like to thank Richard Wilson for reviewing our manuscript and express our gratitude for his suggestions. A point by point answer to his comments is given in the following.

1) I wonder about the possible impact of wake on the turbulence detection from standard radiosondes. This issue could be addressed by considering the statistics of the time during which the payload stays in the wake, i.e. of the spatial extent of the payload-wake encounters. The two presented example have spatial extent of 15 m and 6 m, hardly detectable from radiosonde measurements (some authors - Ferron et al., Wilson et al. - recommend to undersample the vertical profile in order to detect inversions in the potential temperature profile. For radiosondes, this lead to vertical resolution of

about 15 m). Do the authors think that such a statistics could be obtained from the presented probabilistic model? Perhaps beyond the subject of the paper, I think such a result could increase the scope of this work.

We clearly share the opinion that retrieving the proposed statistics would increase the scope of this work. Unfortunately however, we do not see a way forward to obtain the necessary information. This is the case for two reasons:

- Retrieving the statistics from our LITOS measurement does not work, because the wake from the ropes partly masks the wake from the balloon thereby introducing a bias towards smaller spatial extends of the payload-wake encounters.
- Retrieving the statistics purely from our wake detection algorithm might be questionable, because the wake encounter probability does not give a clear discrimination between wake affected and wake free altitude bins. We chose to discard all turbulence measurements from altitude bins showing $P_{\text{wake}} > 5\,\%$ in order to avoid wake influence on our turbulence measurements. Retrieving such statistics using the 5 % threshold however, would overestimate the spatial extend of the payload-wake encounters.

Nevertheless, we still assume that these wake encounters may influence Thorpe analyses from standard radiosondes even if the dataset is undersampled to a vertical resolution of about 15 m. Tiefenau and Gebbeken (1989) find periods of 5.5 s and 11 s on radiosonde temperature data caused by the payload swinging in and out of the balloon's wake. We assume that such long periods may be detectable even in undersampled radiosonde data sets.

2) p17, l5-6: Can you be more specific about this affirmation?

In order to solidify this affirmation, we added a visualization of the ninety-fifth percentile of the radiosonde dataset to the plots of this section. Furthermore, we calculated the spread of the wake probability depending on changes in wind shear, rotation of the wind vector and changes in relative vertical velocity, respectively. The revised text reads: "From the analysis of these three parameters, we find that within the ninety-fifth percentile of the wind shear for the given radiosonde dataset, the wake probability changes from 0.1 % to 96 %. Within the ninety-fifth percentile of all examined rotations in the horizontal wind vector the wake probability changes from 68 % to 95 %. For the variation in relative vertical balloon velocity the wake probability changes from 43 % to 84 %. Therefore, we conclude that within the spread of the given radiosonde dataset wind shear has the strongest influence on the likelihood for wake encounter."

3) p21, l15-16: the assertion that the wake of the balloon contributes to noise (meaning instrumental noise) is questionable. The signatures of the balloon's wake on the temperature profile, either temperature peaks or turbulent eddies, are not a contribution to instrumental noise (assumed uncorrelated), but are likely responsible of false inversions in the potential temperature profile.

We agree with the reviewer that our assumption resulted from misinterpreting the role of instrumental noise in Wilson et al. (2010) and Wilson et al. (2011). The corresponding statements have been deleted from our discussion.

Minor comments
p8, l13: Euklidian -> Euclidian

Thanks for pointing out this typo. The correction has been made.

p 8, l10: why a factor 2 in the definition of L?

The factor of two in the definition of L is a safety margin. It is needed, because in case the wind shear changes over the distance between the payload and the balloon the wake will not move along a straight line and will therefore need more timesteps to reach the closest distance to the payload. However, we agree that a factor of 2 is excessive for this purpose. In order to save computational cost, this factor has been

reduced to 1.2 in the revised manuscript.

p8, l10: the notation L = (...). min(w_rel) is not very satisfactory (the dot can be read as an operator...)

Thanks for the suggestion. We changed the sentence to " L = (...). In this case, min(w_rel)".

Appendix and figure A1: Can one conclude that w is estimated to be zero in the troposphere for all flights?

We cannot sensibly estimate w in the troposphere due to aerodynamically induced variations in the ascent rate. Our wake prediction algorithm however needs an input for w. Therefore, we set $w = 0$ in the critical and supercritical Reynolds number range. To make this more visible, we replaced the solid line in Figure A1 by a dotted line where w is set to zero.

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