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Interactive comment on "A semi-empirical error estimation for PWV derived from atmospheric radiosonde data at the Canary Islands" *by* J. A. Castro-Almazán et al.

Anonymous Referee #1

Received and published: 22 March 2016

The work is of interest and potentially publishable in AMT. It is probably on the very brief end of what could be done and constitute a publication. I would hope that more in-depth analysis were feasible maybe using data from other locations which would serve to increase the impact and applicability of the findings. I would urge this to be pursued.

I also have a number of specific concerns regarding analysis experimental design that I detail in the major comments.

It would also be useful to know if and if so where processing software to perform the analyses is available from and under what licensing restrictions, if any.

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Discussion paper

Finally, the paper would also benefit from proof reading and language adjustments by a native English speaker prior to resubmission to make for an easier read.

Major comments

1. The RS-80 and RS-92 sondes are distinct models using (in the case of the humidity sensor very) distinct observing methods. It is probably unwise to consider them to be the same instrument type. Yet in several places this appears to be the case. It would make more sense to treat the RS92 and RS80 as distinct samples and at least show their equivalence.

2. Both sonde types have sensor response times that are likely lower than the archived measurement intervals for the high vertical resolution soundings. This is particularly acute for the humidity sensors in the upper-troposphere where the effective response time can extend to 30 seconds or more. This has the effect of smoothing the fields recorded vis-à-vis the true sampled state and reduces the effective degrees of freedom such that it is substantively lower than the implied profile measurement points count. Its not clear that this true data resolution, which is a function of the instrument performance rather than instrument measurement reporting frequency, has been adequately taken into account in Section 3 methodological approaches. Intuitively this would lead to an over-estimation of the effects of reduced sampling.

3. The reduced sampling 'standard' profiles are generally standard + significant levels (not just the levels in 5.1 which are too pessimistic an assumption) where the significant levels are defined as inflexion points in the T, RH or wind profile behaviour. This is a more information-rich sub-sampling than the options being considered in Sections 3.2, 4 and 5. It follows that the significant levels approach, which is akin to an optimal information content filter, will require fewer levels to recreate the salient profile features and total column estimates than those being considered in Section 5. The authors could get a trained operator to assign what significant levels would have been for the 64 high-resolution soundings and then repeat their analysis using standard and significant

levels. This would be a more useful and applicable comparison and increase the utility and value of their results. If they are already doing this in Section 5 early analysis this is not made sufficiently clear.

Minor comments

1. The final sentence of the abstract should be folded into the preceding paragraph rather than be a fragment sentence paragraph.

2. Line 98 change drooping to dropping

3. Lines 117-124 – while clearly these are reasonable estimates for the later RS-92 measurements based upon the referenced studies it is substantively less clear whether these assumptions hold for the earlier RS-80. In particular I'd expect on the measurement techniques a higher uncertainty on the humidity sensor for the RS-80 owing to use of a single sensor that could become contaminated.

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