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Comment

Interactive comment on “A multi-site techniques intercomparison of integrated water vapour observations for climate change analysis” by R. Van Malderen et al.

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GENERAL COMMENTS

* This paper seeks to intercompare IWV measurements made using a variety of methods, and identify any relevant explanatory factors in cases where disagreement is found. Overall, the methodology in this paper seems thorough and detailed, and the conclusions seem reasonable. However, I found the paper to be needlessly verbose. This paper comes across as a “data dump” to me, and could be substantially improved by simply getting to the point and not including in the text information that is already

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well documented in the tables and figures. Even then, the results don't seem to be that significant in comparison to previous findings from the literature. Perhaps the main conclusions of the paper could be better framed in the last section.

Author Reply: We first want to thank the referee for dedicating time to the review of our manuscript and formulating interesting suggestions and remarks. It is true that the paper contains a lot of data, because IWV observations done with 7 different instruments (GPS, radiosondes, CIMEL, AIRS, GOME, SCIAMACHY, GOME-2) at 28 locations are compared. But this is actually one of the strengths of the paper, and an intercomparison analysis on this scale has not been presented yet in literature. Moreover, the research described in this paper brings together different communities (geodetic (GPS), meteorological (radiosondes and weather stations), satellite (AIRS, GOMESIA) and aerosol (CIMEL) atmospheric science) that only occasionally interact with each other. The different authors also represent these different communities and we tried to present the different instruments and retrieval methods as clearly as possible for the different communities. We know that the paper could seem therefore needlessly verbose. Additionally, because we are aware that interested readers will only have a look at the sections including the instruments of their interest, we added some specific instrument-related trending issues like the impact of the cloud cover and the reduction strategy of satellite overpass measurements on the intercomparisons, and the daytime-nighttime differences for some instruments. Consequently, the paper contains a lot of material. However, we tried hard to cut all irrelevant information in the text, to get immediately to the point, and to use a less chatty and redundant style. We also completely rewrote the conclusions section, as asked for by the other referee as well.

SPECIFIC COMMENTS

* Furthermore, there are a number of grammar issues and awkward uses of English that need to be dealt with. I suggest having a native English speaker carefully proofread the paper, if this has not already been done. I've highlighted some examples in the list

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below, although this list is by no means exhaustive.

Author Reply: The manuscript was proofread carefully again by 4 of the authors, and we put a lot of effort in improving the language.

* Title – The word “techniques” is awkward and should be removed

Author Reply: we removed the word in the title and at many other places in the text. However, we want to mention that in similar other studies, the word techniques is frequently used, also in the title (Schneider et al., 2010; Thomas et al., 2011; Ning et al., 2012). Especially in our manuscript, there is a clear distinction between the instrument or sensor measuring IWV and the technique used for retrieving IWV. For instance, GOME, SCIAMACHY and GOME-2 are three different instruments, but the same (DOAS) technique is used to retrieve IWV, so that we can treat them as one dataset.

* Line 4, p 1076 – “allowing to retrieve” is awkward.

Author Reply: Replaced by “able to retrieve”

* Line 6, p. 1077 – is it really only 60%? Seems like a reference is needed.

Author Reply: According to Kiehl and Trenberth (1997), water vapour contributes for about 60% of the natural greenhouse effect for clear skies. This reference has been added.

* Line 5, p. 1078 – Ross and Elliot 1996 reference is not in list

Author Reply: Added.

* Line 4-5, p. 1079 – “..of either datasets” is awkward.

Author Reply: OK, changed.

* Line 5, p. 1080 – “and presented” should be “to present”

Author Reply: OK, done.

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* Line 11, p. 1081 – what is GIPSY

Author Reply: We added the following footnote: “GIPSY-OASIS, or GIPSY, is the GNSS-Inferred Positioning System and Orbit Analysis Simulation Software package, developed by the Jet Propulsion Laboratory.”

* Line 15, p. 1081 – “end 2007” should be “the end of 2007”

Author Reply: OK, done.

* Line 1, p. 1082 – swap “the” and whole”

Author Reply: OK, done.

* Line 7, p. 1085 – what is resp. ?

Author Reply: We changed it to respectively everywhere in the text.

* Line 19, p. 1085 – condition should be conditions

Author Reply: OK, done.

* Line 12, p. 1091 – Here and numerous times after, the word w.r.t. is used. This word should not be abbreviated, and is highly overused in the manuscript.

Author Reply: We never use this abbreviation in the manuscript anymore, and tried to eliminate the use as much as possible.

* Line 20-23, p. 1091 – This sentence doesn’t make sense

Author Reply: This sentence is changed in “Because these different satellite devices have different ground pixel sizes, we apply different geometrical co-location criteria with the ground-based IGS stations:”

* Last paragraph, p. 1093 – This analysis would be much more convincing if the authors chose an IGS site that contains a WMO station. Then they could compare the difference in the IWV values calculated from the correct p/T values with ones from

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nearby stations. As is, all the authors are doing is comparing variability among nearby sites.

Author Reply: As a matter of fact, of the selected stations, only the BRUS site contains a co-located WMO station, but there are no other WMO stations within 50 km from this site. The OBE2 site also has a WMO station very nearby (less than 2 km), but with an altitude difference of 70 m. The other three neighbouring WMO stations of this site have altitude differences ranging between 150 and 230 m. As the impact of the difference in altitude is larger than the distance, we take the IGS site with the largest altitude difference with a WMO station, FFMJ, as an example. In the text, we changed it to: “The altitude difference of almost 700 m between the stations 10635 and FFMJ is the largest value of all possible WMO-IGS station co-locations in this study and should therefore provide an upper limit.” This analysis is also shortened considerably in the text.

* p.1095 – The section 3.3 title doesn’t make sense

Author Reply: We changed it to “Correction for difference in altitude”

* line 5, p. 1095 – “if” should be “of”

Author Reply: OK, done.

* line 21 – 22 – “techniques altitude difference” doesn’t make sense

Author Reply: We removed this sentence from the text and replaced “techniques altitude difference” everywhere in the manuscript.

* Last 2 paragraphs, p. 1096 – This discussion is pointless, because the authors don’t use the correction that is being described.

Author Reply: We really shortened the discussion, but nevertheless mention the possibility of an altitude correction between GPS and CIMEL: “Correcting the CIMEL IWV data for the altitude difference with a co-located GPS station is not as straightforward.

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The only possible altitude difference correction should then be applied to the GPS IWV data, only using a correction applied to the GPS hydrostatic delay (ΔZHD with a pressure difference applied to Eq. 2 instead of the surface pressure P_s). However, we cannot detect a real improvement in the coincident GPS-CIMEL IWV comparisons after adopting the altitude correction strategy to the GPS IWV retrievals. Furthermore, this correction introduces a dependence of the GPS-CIMEL IWV scatter plot properties on the GPS-CIMEL altitude differences. And finally, we prefer to have the same common reference GPS IWV data for the comparison with the data from the other instruments.”

* Line 9, p. 1099 – dispose is an awkward word here, and elsewhere in the manuscript.

Author Reply: We replaced “dispose” everywhere in the manuscript, for instance by “archived” in this example.

* Line 21, p 1102 – what is GMF?

Author Reply: GMF stands for Global Mapping Function. This explanation is also added in the text.

* Line 20, p. 1103 and line 14, p. 1104 – “threat” should be “treat”

Author Reply: OK, changed.

* Line 7, p. 1104, missing “of”

Author Reply: OK, added.

* Line 25, p. 1110 – then should be than

Author Reply: OK, changed.

* Line 5-8, p. 1112 – the discussion of the direction of the bias in this sentence is confusing

Author Reply: We agree. Therefore, we changed it, here and everywhere in the text, to “the bias is minimal (or driest) in summer, and maximal (wettest) in winter”.

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* Fig 1 – What is the blue line? A Gaussian fit I suppose? I don't see mention of this.

Author Reply: Indeed, this is a Gaussian fit. We added this in the figure caption.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 1075, 2014.

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