Reviewer 1 (R. Allan):

1) Is this product superior to combining microwave TCWV over ice-free oceans with reanalysis data over land (e.g. Allan et al. 2013 Surv. Geophys. doi:10.1007/s10712-012-9213-z)? If the authors can provide evidence for this then it may help in demonstratingits usefulness.

1) We think that it is valuable to have both datasets available. It is hard to tell which one is superior, but having one which over land is completely based on measurements, and as such is fully independent from model and reanalysis data, surely provides complementary information. Even more important, the MERIS product available over land has a spatial resolution of 0.05°. The product is readily available on request.

2) While microwave TCWV datasets have a mature methodology, to my mind this is not yet the case for MERIS TCWV data. Will preferential sampling of clear-sky scenes lead to underestimated monthly mean water vapour since clear events which can be quite rare in some regions, are generally less moist than cloudy scenes (e.g. Cess and Potter 1987 Tellus doi:10.1111/j.1600-0870.1987.tb00321.x; Allan et al. 2003 QJRMS doi:10.1256/qj.02.217). See also comment (10).

2) There is no way to avoid the clear-sky limitation when using near infrared measurements, no matter how mature the methodology is. As long as this is stated clearly in the text (which it is) we see no general problem. The clear-sky bias has been described in several papers, also in Sohn and Bennartz (2008) and Mieruch et al. (2010).

3) Abstract - "global time series" are mentioned yet the paper does not present them. This would be a stringent test for homogeneity

3) We show a quasi-global time series of differences between GUAN and our data set. Apart from that, we have not shown them, since we are currently preparing a second publication about this interesting topic.

4) p.60, L17 - "uncertainties are not changed" - this is not clear to me. Does it mean that uncertainties are the same in both datasets or the same as their parent products?

4) Has been rephrased. We meant that the uncertainties are the same as in their parent products.

5) p.61, L18 - TCWV and low level moisture are also strongly linked with intense rainfall and the hydrological cycle (e.g. Held and Soden, 2006 J Clim doi:10.1175/JCLI3990.1; Allan et al. 2013 Surv. Geophysics).

5) Thanks for the clarification, we have included this in the manuscript.

6) p.61, L23 - TCWV is not a key variable in the water vapour feedback since changes in water vapour in the middle and upper troposphere are more important than changes at the lowest levels which influence TCWV most strongly (e.g. Soden et al. 2002; Allan et al. 2003 doi:10.1007/s10712-012-9213-z).

6) Thanks for pointing this out. We have rephrased the paragraph.

7) p.63, L12 - the NVAP product suffered from inhomogeneities; how does the new GVaP product compare with the up-to-date NVAP global time series? This may be work for future studies.

7) Within the GEWEX water vapor assessment (G-VAP) long-term satellite based data records are characterised. NVAP-M, the successor of NVAP, is part of this excercise. When looking at Vonder Haar et al. (2012) it seems that NVAP-M is still affected by inhomogeneities.

8) p.65, L10 - how is the altitude of the freezing level parametrized? This may be affected by a warming atmosphere and influence the retrieval.

8) It seems to us that the information regarding page and line does not correspond to the comment. However, the 1D-Var TCWV retrieval is not applied in presence of heavy precipitation so that the likelihood for the presence of a freezing level is reduced. If not present in the input the freezing is not parameterised.

9) p.15, L15 - how does the present method compare with the method applied to the widely used Wentz et al. products? Are there big differences?

9) The method used by Wentz et al. is described in e.g., Wentz (1997), Wentz and Spencer (1998) and Hilburn and Wentz (2007). It is not a 1D-Var scheme as it is utilised here. Schröder et al. (2013) compared the precusor versions of the SSM/I products from RSS and CM SAF and found overall bias and RMSD values of 0.5 and 1.5 mm, respectively. Within the GlobVapour project the old and the new product were compared against ARM data from Nauru: correlation and RMSD are very similar while the bias differs by 0.7 mm. Hilburn et al. (2010) showed that the difference between the RSS v6 and v7 products are small, with the v7 product being wetter at high TCWV values. Thus, we do not expect large differences to the RSS products.

10) p.66, L2 - are daytime clear-sky conditions representative since there could be more water vapour during the day than at night yet less water vapour in clear-sky regions than cloudy. To what extent are these biases significant and do they compensate each other?

10) We can only guess here. Over land, one could expect a higher water vapour content during daytime, due to the generally higher temperature. This could to some extent compensate for the clear-sky dry bias. Apart from this, the same answer as for point 2) applies: Users have to be aware of what kind of data they are using and we claim that we have pointed out the limitations of the data set to a sufficient degree.

11) p.66, L18 - if the cloud data used for screening suffers from inhomogeneity this could affect the TCWV time series through changes in systematic biases and scene identification.

11) Correct. However, we have no indication for any inhomogeneities in the cloud mask time series, since MERIS was a radiometrically and spectrally very stable mission.

12) p.67, L15 - "modelled" based upon what input data?

12) This is explained in the paragraph following the sentence.

13) p.69, L7 - daily or monthly climatological averages?

13) It is actually a three year average (spatially resolved) that is used here. We will change the sentence to "…for which spatially resolved, three-year averages are used."

14) p.69, L23 - F13 has quite stable ECT but F14 drifts by a few hours. Presumably the diurnal cycle of TCWV is not large enough for this to introduce significant errors?

14) The diurnal cycle in TCWV is relatively small over the ocean and we do not expect a large impact. In fact, during the generation of the CM SAF SSM/I FCDR we assume the major contributor to the diurnal cycle (of brightness temperatures) over ocean is cloud liquid water (LWP) due to clouds drying off in the morning and moistening in the evening. This effect has been studied by O'Dell et al. (2008) using microwave satellite data. Their work has been adapted to BTs at 27 GHz and indeed the diurnal cycle is small over the ocean, except at stratus regions off the west coasts of the major continents.

15) p.71, L13 - does masking out precipitation regions bias TCWV low or is this effect small?

15) Our understanding is that this "almost all sky sampling" bias, that is, the TCWV difference between e.g. microwave observations and true all sky observations such as from radio occultation have not been quantified yet. This is issue was discussed at the latest G-VAP workshop. The need for such analysis was published in a GEWEX Newsletter (Schröder et al., 2013, Nov issue). If we assume that the precipitating clouds have similar TCWV than the surrounding cloud than we can expect an underestimation (e.g., Schröder et al., 2013, in AMT).

16) p.71, L19 - can the smooth transition of water vapour between land and ocean be evaluated using high resolution reanalyses?

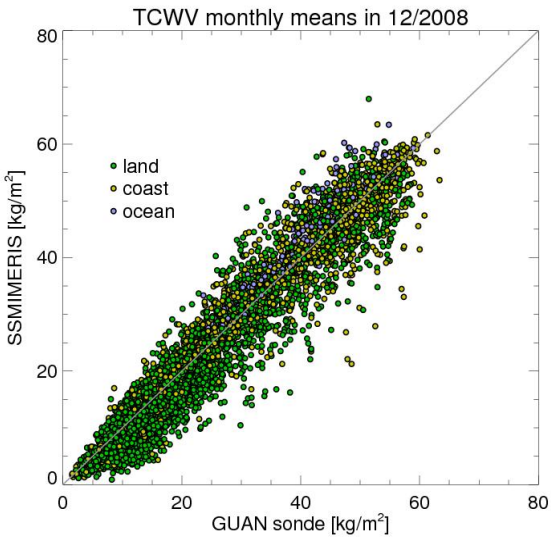
16) The spatial resolution of typical reanalysis data might not be high enough for this purpose but we will look into this.

17) p.73, L16 - is there any evidence of a trend or jumps in the record?

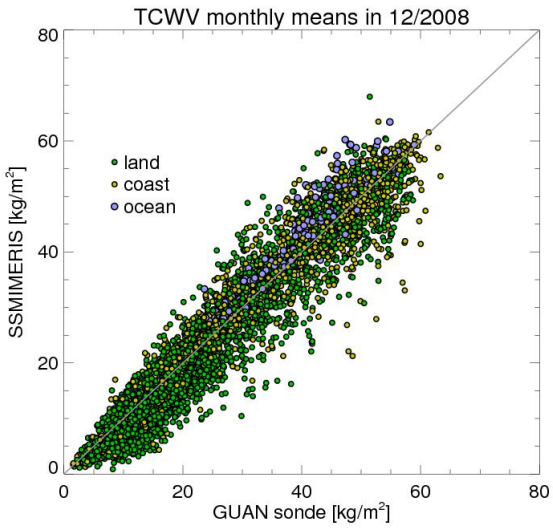
17) We have not explicitly analysed the trend or breakpoints in this paper. From other studies we can conclude that the SSM/I record is homogeneous, that is, does not exhibit breakpoints (Andersson et al., 2010, Schröder et al. 2013, www.cmsaf.eu/docs, then search for HOAPS and FCDR), and that the trend is slightly positive and within expectations from Clausius-Claperon (presentations at G-VAP workshops and Mieruch et al., 2014, submitted). With respect to the MERIS fraction of the data set, we are currently preparing a publication, so we left out the results here and concentrated on presenting the dataset.

18) Figure 6 - it is difficult to see the ocean points. I suggest making the symbols bigger as there are less ocean points. These points appear to be biased high; is there any reason for this? This seems to be the opposite bias found for the MERIS-SSM/I comparisons in Fig. 8.

18) The plot is modified by enlargening the ocean symbols (see below). The different biases found in the two comparisons are hard to interpret, keeping in mind that in the other comparison the MERIS land retrieval was applied to ocean cases without a fully realistic treatment of the ocean surface reflectance pattern and the impact of this on the scattering correction.



Old Figure 5



New Figure 5

19) p.74 - this is a very nice comparison of MERIS and SSM/I over sunglint regions.

19) Thank you.

20) p.74, L9 - is the wind speed from SSM/I?

20) No, it was taken from ECMWF reanalyses, now stated in the text.

21) Figure 8 - is cloud also screened from SSM/I in this case?

21) Yes, now stated in the text.

Corrections ...

All proposed corrections were implemented.