

## Authors' Response to Reviews of

### Evaluation of Total Column Water Vapour Products from Satellite Observations and Reanalyses within the GEWEX Water Vapor Assessment

Tim Trent, Marc Schroeder, Shu-Peng Ho, Steffen Beirle, Ralf Bennartz, Eva Borbas, Christian Borger, Helene Brogniez, Xavier Calbet, Elisa Castelli, Gilbert P. Compo, Wesley Ebisuzaki, Ulrike Falk, Frank Fell, John Forsythe, Hans Hersbach, Misako Kachi, Shinya Kobayashi, Robert E. Kursinsk, Diego Loyola, Zhengzao Luo, Johannes K. Nielsen, Enzo Papandrea, Laurence Picon, Rene Preusker, Anthony Reale, Lei Shi, Laura Slivinski, Joao Teixeira, Tom Vonder Haar, and Thomas Wagner

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Please find below our responses to Reviewer 1, We include the original comments in black, our comments in green, and any alterations to text in blue.

This study does an impressive job of consolidating the information in a wide variety of data sets, and manages to nicely balance depth of analysis with breadth of topic. The results are relevant and interesting, and reveal several important insights. For these reasons, the study is appropriate for publication. I recommend some minor changes to the text.

The first recommended change is to revamp the discussion of diurnal cycle in around line 465 and elsewhere. The text mentions potential biases from a lack of 'full day' observations without being specific about what this means. Sun-synchronous satellite measurements of the true diurnal cycle (exactly 24 hour period) taken every 12 hours will average to the diurnal mean. Consequently, the long-term mean will be unbiased. In contrast, sampling once daily will introduce a bias since the average is over only a single phase of the diurnal cycle. This is the case for the daytime-only satellite observations. The distinction between these two situations isn't clear from the text, but some minor editing should fix this. (A semidiurnal cycle in TCWV or the effects of diurnally varying clouds on sampling further complicate the picture. This is worth mentioning, but beyond the scope of the study.) Because of the potential for the diurnal cycle to introduce a bias, the study should mention the times of day of all satellite data sets and whether the observations are obtained only during day or night. Some information about orbit local times and day-only sampling is provided in the current text, but not for every data set. Consistent statements for all satellite data sets would be helpful. A table may even be appropriate, though that is at the authors' discretion.

Indeed, it is helpful to add information on temporal sampling characteristics. We propose to add the following (section 5, after the reference to Sohn and Bennartz, 2008):

Also, TCWV retrieved from observations in the visible and NIR rely on reflected solar radiation and thus, is available during day time only (affected products are: CM SAF/WV\_cci

over land, GOME Evolution, MODIS (NIR), and MPI-C OMI). Additionally, the majority of satellite-based TCWV CDRs rely on measurements from polar-orbiting satellites. Thus, observations of TCWV are only available at specific times of the day, and the full day is not covered with samples. The following data records rely on single sensor observations only, with the equator crossing time given in brackets: AIRS v6/V7 (10:30+13:30), AIRWAVE v2 (10, 10:30), CM SAF/WV\_cci (10:30, over land), EMiR (10, 10:30), GOME Evolution (10, 10:30), MODIS Terra (10:30), MODIS Aqua (13:30), and MPI-C OMI (13:40). All other satellite based data records sample more frequently, but at different times and frequencies, partly affected by orbital drift and partly varying in time. Details on satellite equator crossing times can be found at <https://space.oscar.wmo.int/satellites>. Examples of equator crossing times affected by orbital drift can be found at: [STAR - Global Vegetation Health Products: Equatorial Crossing Time \(ECT\) for NOAA Polar Satellites and Remote Sensing Systems \(remss.com\)](#). This specific sampling might cause a diurnal cycle sampling bias.

Following the above discussions by the review we added (after the reference to Falk et al., 2022):

It is noted that the effect of orbital drift, of potential changes in diurnal cycles of TCWV and clouds and of potential changes in spatial cloud distributions with climate change have not been considered here.

A discussion of HOAPS data set is needed, especially since it is the reference data set over ocean. (The first reference on p. 9 does not even spell out the acronym.) Even a short paragraph would be very helpful.

We agree and added a short introduction of HOAPS to section 3.3:

As ERA5 was already introduced in section \ref{era5} we briefly recall here key characteristics of HOAPS. HOAPS is a product suite of satellite based climate data records, including TCWV, over the global ice free oceans. TCWV is derived from quality controlled, recalibrated and intercalibrated measurements from SSM/I and SSMIS passive microwave radiometers (Fennig et al., 2020), except for the SST, which is taken from AVHRR measurements. TCWV is retrieved with a 1D-Var scheme. The data record covers the time period from July 1987 to December 2014 and has global coverage, i.e., within  $\pm 180^\circ$  longitude and  $\pm 80^\circ$  latitude. The product is available as monthly averages and 6-hourly composites on a regular latitude/longitude grid with a spatial resolution of  $0.5^\circ \times 0.5^\circ$  degrees.

The text would also benefit from a brief discussion of the use of ocean surface temperature and near-surface air temperature over land in calculating the temperature-TCWV regression. The use of different reference temperatures has implications for interpreting the temperature-humidity relationship over land versus over ocean, and intercomparison of land and ocean relationships.

The contrast in expectations is discussed in section 5. We expanded the discussions in section 5 by adding: “Following discussions in \cite{falk2022} on land areas, the relation between air temperature and surface temperature is complex and locally the difference between air and surface temperature reaches a few Kelvin. This depends on various factors, such as local time, cloudiness and surface type (e.g. \cite{good} and \cite{rayner}).”

Here are more specific comments:

Line 38 forward. How are these goals different from the first phase of the GVAP assessment?

This is indeed not very clear. We propose to change the first sentence after the list into: “These objectives are similar to the objectives from the first phase by only enhancing efforts directed towards process evaluation studies and regional analysis. In particular, as in phase 1 of G-VAP, the assessment effort focuses...”

Line 70. It’s not obvious that Aqua is included in this study. Words like “now includes both Terra and the Aqua” would be helpful.

We propose to change the sentence into “In the case of the Moderate-resolution Imaging Spectroradiometer (MODIS) products, we now include the Terra and Aqua versions, i.e., MOD08\_M3 (new) and MYD08\_M3 (as in previous archive), respectively.”

Section 2.1.1. The local time of the Aqua spacecraft is needed here. Also, the reference to Manning et al. on line 83 should be Susskind et al. Here is the first link on a web search of the title: [https://docserver.gesdisc.eosdis.nasa.gov/public/project/AIRS/L2\\_ATBD.pdf](https://docserver.gesdisc.eosdis.nasa.gov/public/project/AIRS/L2_ATBD.pdf)

We thank the reviewer for spotting this error. Google Scholar had created the reference Manning et al. for the document linked in the comment above. Therefore, we have updated this appropriately so that the references are now Susskind et al. (2003, 2020) in the text. The local overpass times (13:30 and 01:30 hrs) have been added to the text.

Sections 2.1.2. Please mention the local time of the satellite observations, and/or the drifting orbits, as appropriate.

We included the equator crossing times for satellite missions over land and a link to a webpage where equator crossing times of missions with microwave imagers onboard are shown.

Section 2.1.3. Are the GOME observations obtained day and night, or 10:00 daytime only and not 22:00?

The GOME Evolution Climate product is a daytime-only product as they use the red part of the visible spectrum for retrievals from each instrument. We have made a slight edit to the text to ensure this is clear:

“The ‘GOME evolution climate’ product was generated within the GOME-Evolution project funded by ESA, and the retrieval is described in detail in Beirle et al. (2018). It is based on measurements from the satellite instruments Global Ozone Monitoring Experiment (GOME), Scanning Imaging Absorption Spectrometer for Atmospheric Chartography (SCIAMACHY), and GOME-2 in the red part of the visible spectral range, using the retrieval proposed in Wagner et al. (2003, 2006), with all satellites measurements (daytime only) occurring around 10:00 hours local time.”

Section 2.1.4. Some mention is needed of which satellite data sets considered in the study are also assimilated into ERA5. Even a statement like “nearly all” would be helpful.

We propose to insert in line 132: “In fact, ERA5 assimilates most of the satellite measurements considered in this study (see Hersbach et al. 2020, table 4 and figure 5).”

Section 2.1.5. The local times of Terra and Aqua overpasses are needed, and it should be stated that the retrievals are available both day and night (if so).

We added “this product uses the Thermal Infrared (TIR) bands 25 and 27 through 36 to retrieve temperature and moisture profiles, total-ozone burden, atmospheric stability, and atmospheric water vapour for daytime (10:30, 13:30 hrs local time) and night-time (22:30, 01:30 hrs local time) overpasses (Terra/Aqua respectively). The level 2 (L2) product contains the geophysical parameters at a resolution of 5x5 km for both clear-sky day & night scenes.”

Section 2.1.6. Are MPIC daytime-only observations?

Yes, we updated the text to clarify “satellite with an equator crossing time of about 13:30 local time (daytime only observations)”

Line 175 forward. Is the lack of SSM/I etc., data different from ERA5? That seems to be the major distinction, but it's not stated.

We added “, in contrast to e.g. ERA5 which assimilates radiances from these sensors”.

Line 195. Is this bilinear interpolation in latitude and longitude. This is worth mentioning because bilinear interpolation is actually a quadratic function, so has a specific (if confusing) definition.

We use linear spline interpolation to calculate the new grid centre values in this study. The text has been updated to clarify this:

“For the reanalysis records, each monthly gridded TCWV global field was first shifted in longitude space to run between  $-180^\circ$  to  $180^\circ$  before being interpolated onto the centres of the archive common grid using a linear spline function”

Line 224. This paragraph on comparison temperatures is worthy of its own short subsection.

We agree that the details regarding comparison to surface temperatures should be in their own subsection. We have added a heading and updated the text to read (plus the additional discussions mentioned above):

### “3.3 Regression against Surface Temperatures

In addition to calculating trends, and to be consistent with Phase 1 of G-VAP, a regression of each TCWV record against surface temperature dataset(s) is performed following the approaches outlined in Dessler and Davis (2010) and Mears et al. (2007). If we assume relative humidity (RH) is constant, then the Clausius-Clapeyron relationship produces a ratio between changes in water vapour and temperature that is only dependent on temperature. Therefore, under constant RH and pressure assumptions, changes in water vapour mixing ratios can be transferred to saturation vapour pressure values. For a temperature change of 1 K, the expected change in mixing ratio is between 6% at 300 K and 7.5% at 275 K. These values then provide the limits of the range of expected regression coefficients against the chosen surface temperature data records used in this study:

- Over ocean sea surface temperature (SST) data from the European Space Agency (ESA) Climate Change Initiative (CCI) (Merchant et al., 2019; Merchant and Embury, 2020).

- For both land and ocean surfaces, surface air temperature (T2m) from the European Centre for Medium-Range Weather Forecasts (ECMWF) ERA5 reanalysis (Hersbach et al., 2020) is used.

Both temperature data sets were processed on the same grid as the TCWV records in the G-VAP archive for consistency”

Line 237. As discussed above, this is the first mention of HOAPS.

See comment above.

Line 284. The region described as California is almost entirely along the coast of Mexico. (The Baja California Peninsula is in the northern part of the region of interest, but is within Mexico and mostly outside of the California climate region.)

This is true and we changed the text accordingly.

Line 305. Is this the first use of Delta-TCWV? It does not appear to be defined earlier (or else I can't find it).

It is indeed only indirectly defined in the legends of Figures 6 and 7, i.e., it is the bias relative to the ensemble mean. We propose to define it at first occurrence in the text and in the figure captions (see updated manuscript).

Line 317. We noted the wet biases in stratiform regions in Fetzer et al. (2006) and attributed them to a combination of subsidence-induced extensive cloudiness and low TCWV, while clearer conditions have higher TCWV. Interestingly, that study considered only a few weeks of observations.

Thanks for this reminder. We included a reference to this paper also here.

Line 339. Fix Figure ??.

Now Figure 2 is correctly referenced.

Line 347. The 'also' can be deleted since it's redundant with 'In addition'. A similar argument can be made for 'also' and its relationship to 'include' in the next sentence.

We deleted "also" in both sentences.

Line 352. The sentence starting 'The UWHIRS...' should start a new paragraph since the HIRS data sets obviously deviate from the other data sets. This is worth be highlighting.

We started a new paragraph in the updated manuscript. Consequently, the sentence starting "For the other products,..." also starts a new paragraph now.

Line 358. Change "range of regression coefficients are" to "...coefficients is" since 'range' is the subject.

Changed as proposed.

Line 361. Is the 'theoretical range' the one set by the Clausius-Clapeyron relationship? It's not obvious from the text.

We added “following Clausius-Clapeyron”.

Line 365. Should “noise” be “large, and likely anomalous, high variability” or something like that? A noise mechanism hasn’t been clearly established.

Indeed, this should be formulated differently. We propose the following:

“Sea-ice boundaries show up as large MAD values with high spatial variability at high latitudes.”

Line 388. Change to “Also noteworthy are...”

Implemented as proposed.

Line 405. Change the sentence ending slightly to “...trend estimates discussed earlier.”

Implemented as proposed.

Line 411. That should be “at least 50% and 100%” since ‘or’ implies either one or the other, but not both.

Changed as proposed.

Line 415. Are the “expected regions” those mentioned in Table B1? The text is ambiguous here.

Yes, we have updated the text to clarify this to the reader:

“Generally, positive correlations between all datasets occur in expected regions (as outlined in table B1) related to the specific index.”

Line 416. Changing “observed” to “observe” will made this statement present tense, like others in the section.

We changed to “observe” in the updated manuscript.

Line 425. Delete “partly” or else explain the differences more fully.

We deleted “partly” in the updated manuscript.

Line 430. State the reference data sets used to generate the metrics since they are fundamental to the results.

We propose to change the sentence from line 427-429 into:

“Also, new versions of HOAPS (here: HOAPS V4, in phase 1: HOAPS V3.2) and ERA (here: ERA5, in phase 1: ERA-Interim) served as references for breakpoint detection and different SST (here: SST from ESA CCI, corrected v2, in phase 1: OI SST from NOAA, v2) and T\_2m data records (not at all in phase 1) were employed for regression analysis.”

Line 440. Are ‘expected ranges’ determined by the Clausius-Clapeyron relationship? In any case, the reason should be given.

True - a proper reasoning was missing. We added the following to section 3.2, line 225:

“Saturation vapour pressure is a function of air temperature and change in air temperature (see e.g. Hyland and Wexler, 1983). Saturation vapour pressure can be transferred into a change in mixing ratio assuming constant relative humidity and pressure. For a temperature change of 1 K the expected change in mixing ratio is between 6% at 300 K and 7.5% at 275 K.”

and to change the sentence in section 5, starting line 444 into:

“It is recalled that the relationship between TCWV and surface temperature is affected by advection, precipitation, and other small-scale and regional events, which impact equilibrium between surface and atmosphere. Also, surface temperature and TCWV instead of near-surface air temperature and mixing ratio are considered here (e.g., Mieruch et al., 2014). Violations of these assumptions can give reason to larger than expected regression values (Trenberth, 2005).”

Line 444. Shi (2018) should be Shi et al.

Indeed, this has been fixed.

Line 445. Not clear what is meant by ‘that time.’

It is indeed not very clear. “With that” refers to “event”. We propose to delete this as it should be clear that a lag that depends on an event naturally depends on time as well.

Line 465. See earlier comments about diurnal sampling with sun-synchronous satellites.

We enhanced the information and the discussion (see above).

Thank you for the all the careful work!

Eric Fetzer

Thanks a lot, also for the detailed review.