

**Author response to reviewers comments
on
“Observations of precipitable water vapour
over complex topography of Ethiopia from
ground-based GPS, FTIR, radiosonde and
ERA-Interim reanalysis” by Mengistu Tsidu et al**

We thank both reviewers for their thoughtful comments and suggestions on improving the manuscripts. We have used all the comments and suggestions except under circumstances where they only need clarifications.

We also thank the handling editor and the journal editorial team for allowing us to improve the manuscript.

The response to comments and suggestion are indicated in bold face following each comment.

Anonymous Referee #2

Comment

Compared to its first version the article “Observations of precipitable water vapour over complex topography of Ethiopia from ground-based GPS, FTIR, radiosonde and ERA-Interim reanalysis” has improved considerable but still does not show the quality needed for publication. My main criticism is that the manuscript

1. still fails to motivate that one of the most interesting aspects of the GPS network in Ethiopia: it allows to analyse the quality of the most important global reanalysis, i.e. ERA-Interim. The paper still treats ERA as it is just another observation

Response

We appreciate the reviewer for noting the considerable changes from its first version. We have made several changes at different parts of the manuscript to emphasis reviewer's point.

Comment

2. fails to present its results in a clear way. For example in Section 3.2 the presentation of Bias, RMSE, standard deviation, correlation between the different data sets would be easiest in a Table – and not all values need to be repeated in the text!

Response

We agree with the reviewer that a table may serve as additional short summary. This additional information is now given Tables 4 and 5 in the revised manuscript in Sections 3.1-3.2 respectively.

Comment

3. misses to show the full potential of the GPS network, e.g. it should show the diurnal cycle. Even if ERA-Interim has only 4 data points such a comparison would also be interesting.

Response

We agree with the reviewer that diurnal cycle is one interesting aspect. We have now included it as Fig. 10 under Section 3.3 of the revised manuscript. The title of the section is altered to reflect the new additions as “Diurnal and seasonal variability of ERA-Interim and GPS PWVs”.

Comment

4. does not provide enough arguments to make the case that the ECMWF model convection scheme does not work sufficiently over orography.

Response

We strongly believe that going into details of convection schemes used in ECMWF model is not the focus of this study. This will distract the main objectives of this work which is to assess the comparability of different data sets on PWV from this region. Moreover, two published works on the region have already indicated the problem with ECMWF model in particular and any model in general in reproducing closely related quantity i.e. precipitation. These publications are cited in support of our inference that the convection scheme is the source of the discrepancies between the three data sets and ERA-Interim PWVs. Furthermore, we have now included the diurnal cycles of PWVs from ERA-Interim and GPS which shows ERA-Interim have not captured diurnal cycles as compared to GPS. This supports our claim to certain extent.

Comment

5. suffers from poor use of scientifically precise language and contains several lengthy discussions that are distracting the reader. It needs to be reduced to the significant results.

Response

We are not sure whether we can satisfactorily deal with the very generic comment of the reviewer. However, this generic comment is somewhat also reflected in the reviewers specific comments in one way or the other which we have fully addressed in the following.

Specific comments

SPECIFIC POINTS Abstract: - Acronyms not spelled out in abstract – likewise AMMA in p, 9874

Response

We have now spelled out all the acronyms.

Specific comments

p. 9871, 115 “Some studies have suggested that a substantial increase in water vapour content in the tropics could give a larger impact than a doubling of the carbon dioxide concentration (Buehler et al., 2006; Nilsson and Elgered, 2008).” These studies did not investigate doubling of CO₂. If you want to say that water vapor is important just cite Stevens, B., & Bony, S. (2013). Water in the atmosphere. *Physics Today*, 66(6), 29-34. and keep it short. There are some statements that are not 100% correct in this paragraph but I can’t go through every sentence...

Response

The comment is taken and the whole introduction is rewritten to take into account this comment and others.

Specific comments

p. 9872, l3 “Atmospheric water vapour exhibits substantial diurnal variations..” What does substantial mean? To my knowledge it is not more than 2 mm (the most under continental summer conditions) and thus not much more than the uncertainty of GPS – please give a value in the text. In fact I am missing plots of the diurnal cycle at the seven stations which would be a real interesting information.

Response

We used “substantial” to mean large variation with respect to other atmospheric constituents. We do not think this qualifying word can be measured in terms of number. Moreover, we doubt whether the 2 mm limit works for all regions. This can be verified from the new analysis on the diurnal cycle for our region which is included upon suggestion of the reviewer in his/her general comments.

Specific comments

In general the introduction contains many lengthy explanations but does not make the why reanalysis is important and it is therefore equally important to evaluate its quality. One example for an unnecessary statement is “In recent decades, the use of GPS has been extended to investigation of the upper and lower atmosphere.” You could certainly reduce the whole introduction by a factor of 2.

Response

We have tried to address the concern of the reviewer regarding length of the introduction and included brief introduction on why reanalysis is important.

However, we do not agree with the reviewer's citation of statement such as “In recent decades, the use of GPS has been extended to investigation of the upper and lower atmosphere.” as unnecessary. This statement has to be seen in the context of the overall flow of ideas. The paragraph containing this statement is a follow up of paragraph on traditional instrumentations that have been in use to monitor PWV. Therefore, this statement and the whole paragraph shows the expansion of monitoring methods from traditional instrumentations to ground-based GPS receivers which were originally designed for the purpose of position determination.

Specific comments

P. 9874 Statements like “..they are steps in the right direction“ should be omitted

Response

We have replaced it with “they have made important contributions” in the revised version.

Specific comments

p. 9875 and following: The discussion of measurement uncertainty needs to be improved and I again recommend to integrate the subsections in 3.1 into section 2 as it is more logical to present the measurement uncertainty together with the measurement description. This would also avoid unnecessary doubling of information.

Response

We have followed the reviewer's suggestion. In fact, all subsections under Section 3 on data quality and error characterization is now part of the data descriptions and error characterization under Section 2 in the revised version.

Specific comments

p. 9875 “..carried out at Addis Ababa synoptic meteorological station for long time despite gaps..” long time is unspecific – give at least a percentage for the data availability. Suggestion is to put it into a table and refer to it.

Response

This station operates since 1970s. This information is now included in the revised manuscript. However, we are only interested in the common time window from 2007 to 2011 of all data set as shown in Table 3.

Specific comments

P. 9875, 111: The motivation for use of ERA-Interim does not fit here – motivation is for the introduction..

Response

We have now incorporated it into the introduction of the revised manuscript.

Specific comments

p. 9875: It is not interesting that you get pressure data via automailer bit how is it derived? Why not use ERA-Interim – how does it compare and how is it interpolated to real surface height?

Response

We understand that this comment arises due to information gap on how we use pressure and temperature profiles in retrieval of trace gas species from observed irradiance by FTIR and other similar sounding instruments. The atmospheric layers considered in retrieval of trace gases varies slightly from one algorithm to the other. However, in almost nearly all cases the altitude range covered in retrieval extends up to 120 km, which is assumed to be top of the atmosphere. To our knowledge, ERA-Interim does not provide this kind of data. The automailer is a system whereby a person places a request for the data at a particular location and then the data is processed and sent back to the user.

Specific comments

p. 9876 “several biases” sounds weird – these are different sources for error

Response

It is true that satellite clocks, receiver clocks, and receiver are different sources of error. However, in the context given here, we are referring to the errors or biases not the source themselves. We prefer to use bias instead of error since error includes also some statistical error components.

Specific comments

P. 9876, l20: This is not only tropospheric zenith delay! Though the stratospheric portion is probably negligible you should not reduce it to tropospheric.

Response

We know that it includes all portions but we want to stick to this term that is customarily used in most literature.

Specific comments

p. 9877, l 12: As you say the coefficients a and b vary with temperature and humidity (only roughly and indirectly with latitude and season) – why do you then give values that you don't use – this is irritating.

Response

As the information is available elsewhere and not relevant to our specific case, we have accepted the reviewer's comment and omitted lines 7-12 of the old manuscript in the new version.

Specific comments

p. 9878, l22. It is a bit strange that you always use different data sets for the different data sets ..Why not the same source as for the FTIR or the Tm calculation

Response

GPT model is commonly used by GAMIT software for processing PWV from GPS. FTIR uses NCEP data that includes upper atmosphere, not the same as the standard product since the standard product extends up to 1 hPa level. This is also why we do not use ECMWF for processing FTIR observations. We never calculate Tm ourselves as correctly indicated in the manuscript. We have used readily available product which has proved to be good as indicated by other investigations from literature (this is also indicated in the manuscript). Therefore, our main target is to use the default GPT model commonly used and assess its performance against observations wherever possible.

Specific comments

p. 9879 What type of humidity sensor has been used on the radiosonde? As you mention Vaisala it is most likely a capacitive one measuring relative humidity which must have been converted to dew point..

Response

The sensor is a capacitive sensor which measures relative humidity as correctly pointed by the reviewer. We have used the dew point temperature to calculate vapour pressure as a function of altitude which along with surface pressure are then used to calculate mixing ratio. The mixing ratio is then integrated to determine PWV. This information is included in Section 2.2 under third paragraph of revised manuscript.

Specific comments

p. 9880, l. 5 rho at which altitude (level)?

Response

The water vapour density is the value at surface and this is now specified in the revised version.

Specific comments

p. 9880. Here you explain how FTIR works this should come first when you introduce it in 2.1 It is needed to understand the microwindows (mentioned at least twice). Why is the table on microwindows for the FTIR needed anyway? Therefore you need to merge 2.1 and 3.1.1 in order to avoid redundancy!

Response

Based on the reviewer's general comments, we have now merged section 3.1 with 2.1. In view of the merger between Sections 2.1 and 3.1, we have also made minor amendment on the texts.

Specific comments

p. 9880, 116: “..strongly depend” you need to give a number if you make such a statement

Response

This is a well established dependence and has been reported in several previous works. We have included additional references to support this statement as indicated in the third paragraph under Section 2.1 of the revised manuscript.

Specific comments

p. 981, 15: Better “The retrieval algorithm allows to characterize the diff...”

Response

The statement is rewritten as suggested above in Paragraph 5 of Section 2.1.

Specific comments

P. 9882, 15: refer to table and figures for gaps of water vapour VMR can also be characterized by assessing the dif..

p. 9883, 12 PWV is just the integral of water vapor density – thus it does not formally depend on temperatureonly because you or others do conversions – but you can easily show that the rel. humidity measurement is the dominant error source. The whole idea of 3.1.3 is unclear: what is the main conclusion? Why not merge it with 2.3?

Response

We agree with the reviewer that the main error source is in the measurement not in the conversion. In fact, we are not discussing about error in the integration but rather about mean calibration bias which is a function of RH and temperature. This section is also merged with section 2.3.

Specific comments

p.9883, 123. Radiosondes are assimilated – you can not judge the quality with them! As before for the soundings: I do not see any sense in having this section – what is your message? Most of it can be deleted. Why is it under Results and discussion?

Response

This section is merged with Section 2.4. The statement on the quality of ERA40 on land is also removed since radiosonde data is assimilated as correctly pointed out by the reviewer.

Specific comments

p. 9883, l. 23; it is only Berrisford et al., 2009 – Paul is the first name..

p. 9884, l. 24 “t has been known from several previous studies that PWV from radiosonde is

generally dry biased at the upper ends of PWV ..” That needs a reference and should have been discussed in section 2 already.

Response

Both comments are taken and relevant changes are indicated in the revised manuscript.

Specific comments

p. 9885;, l. 5 “0,75 deg is not high resolution – Note, that it is not the grid point that counts! The model value represents the complete model grid cell – smoothed over the complete orography.

Response

We understand that the model value represents the model cell. Even then, the 0.75 deg grid spacing translates to the model grid cell available smallest area thereby allowing smoothing over relatively homogeneous orography. Currently, this resolution is the highest publically available dataset. Some authors have employed horizontally interpolated data, using surrounding grid points in the past to account for the heterogeneity of the grid cell. However, only data from the nearest grid-point (in horizontal coordinates) are used in the present work following the work of Bock et al (2005). As indicated by Bock et al (2005), it is recommended at ECMWF for verification with sparse observational data. This information is now included in the revision.

Specific comments

p. 9885 “While this difficiency (spelling!) is generally common to all high latitude regions, the major problem which is more specific to the region is lack of observations that would have been assimilated into the model for a better results” Do you mean high altitude instead of latitude? Is the statement really correct? What about Tibet or Chile?

Response

We appreciate the reviewer for noting our oversight and indeed we wanted to say high altitude. The deficiency that we referred to is about general GCM wet bias over highlands and dry bias over lowlands inherently related to convection scheme. This deficiency might have exacerbated by lack of assimilated observations over our region. Therefore, we are not referring to a model which is specifically tuned to match observations over Tibet or Chile. To avoid such kind of ambiguity, we have rewritten the statement by only referring to Ethiopia for which we already know the model performance as indicated in third paragraph of Section 3.1 of the revised manuscript.

Specific comments

p. 9886, l17; The correlations in PWVs from the three observations and reanalysis model at Addis Ababa imply strong correlation despite wide range of variation in PWV in the area. and the time series provide uncertainties of radiosondes from literature (see above), “ Well compared to the literature I think it is rather poor – compare to literature. Later you mention similar studies but do not give references. The end of the section should be significantly shortened.

Response

Our statement above is made in view of the fact that Addis Ababa is a high altitude station with complex orography. Obviously, we do not expect as good agreement as say in Tenerife where the model itself is well constrained. To be more precise and avoid similar impression regarding the message convened in the above statement, we rephrased such that “The correlations in PWVs from the three observations and reanalysis model at Addis Ababa imply reasonably good correlation despite wide range of variation in PWV in the area”. The

statement “ Similar studies ...” is not appropriate here, we have removed it. We have also reduced most of the text in the last paragraph of Section 3.1 in the revised manuscript.

Specific comments

p. 9887, Section 3.3: The first part is rather lengthy and not well structured. The possible error introduced by the missing pressure measurements is interesting but never a clear number is given! In terms of data gaps it would be much easier to adjust Table 3 as suggested and then only to refer to it

Response

The first part of this section is now shortened. Reference is also made to the error introduced as a result of missing pressure which is already described in Section 2.2 of the revised manuscript.

Specific comments

p. 9888, l.15. There are many thinks that might cause a dry/wet bias in the model from parameters in the land surface model via the turbulence to convection parameterization. Here and in the following you make to strong statements about the quality of ERA-Interim

Response

As pointed by the reviewer, there are several factor that influence model output and certainly there is difference between models. However, we have conducted several RCM experiments in the past over Ethiopian from which we have deduced that convection scheme plays major role. These works have been cited as appropriate in different parts of the manuscript. Owing to other possible factors as well, we have tried to slightly soften our statements in different parts of the revised manuscript.

Specific comments

p. 9889, l10: Is therefore a truth sounds too strong...

Response

We have soften this statement and restated as follows:

“The high GPS PWVs might be a reasonable values that could be attributed to other factors”

Specific comments

p. 9889, l10. I have never heard that close to lakes such an increase in IWV exists...I made a rough calculation of evaporation and would only guess a very small effect in the column – also taking t circulation effects into account should reduce this even more– do you have some more insights?

Response

The maximum difference between ERA-Interim and GPS is in the wet seasons, specifically JJA and SON as can also be seen in Fig.11. The integrated moisture flux shown in Fig. 13 of the revised manuscript also suggested that moisture is transported to the GPS sites across the water bodies as indicated in the manuscript. However, we agree with the reviewer that there could be other factors such as temperature. Statement that reflects this possibility is now included at the end of Section 3.2 of the revised manuscript.

Specific comments

p. 9889, l 123: Figure descriptions, like “the station labels on the horizontal axis..” have nothing to do in the text. The text should only contain the results. Figures are just referred to for support. One should be able to understand the text without looking at the figures and vice versa.

Response

We have now removed this text.

Specific comments

p. 9890 You say that convection is the main source of moisture? What about the land module?

Response

The land module might have a role. However, unlike the convection scheme which we have found to have significant impacts on our region, there is no any study on land module in the region to our knowledge so far to make any statement on it. Nevertheless, we have now included statement on the possible role of land module and other factors in the first paragraph of Section 3.3.

Specific comments

p. 9890, l.24: What is a silent feature? mention drift. - are there soundings at midnight to investigate dry bias - the GPS PWV quality is interesting but why are highly uncertain values not eliminated or

Response

There is only one sounding at 12 UTC per day. Therefore, investigation of dry bias from day-night difference is not possible in this case. However, we have employed the solar radiation dry bias (SRDB) correction algorithm as implemented by Wang et al (2013). The the algorithm is based on the idea that the effect of solar heating on the humidity sensor can be estimated by the effect of the temperature warm bias on the saturation vapour pressure. Vaisala RSN2010 table is used to drive temperature correction. A figure on relative humidity (RH) correction for a typical radiosonde measurement, mean of all the corrections along its estimated standard deviation is now included since this was also suggestion from reviewer's #1.

The highly uncertain values whose estimation error exceed 2 mm are excluded in this study. There were GPS observations with estimation error as high as 5 mm.

Specific comments

Section 3.4 and Conclusions The influence of the topography – and land surface type- is discussed in the text but not clearly documented. Can you exclude errors due to local installation? The statements about the convection scheme are rather strong – is this supported by PWV differences between ERA and GPS diurnal cycle?

Response

One of errors due to local installation might be multipath effects of GPS signals. Such errors can be avoided by considering high elevation GPS observations. In this work, the lowest elevation angle considered is 10 degree which is large enough to reduce such errors. Other possible problem with installation is the stability of the installation. However, such problem has only significant impact in other areas such as geodetic applications.

The diurnal cycle supports our statements about the role of convection scheme in the observed discrepancy between GPS and ERA-Interim. A paragraph on diurnal cycle based on new Fig. 5 is included at the beginning of Section 3.3 in the revised manuscript.

Specific comments

Table 3: For the observations you should add the data availability in percent. That would also allow

you to give a precise figure caption – the current one is not acceptable.

Response

Measurement with FTIR can only be made during clear sky conditions. It is not a regular time interval observation. Data availability in percent can not be defined for FTIR. For radiosonde and GPS, the percentage of available data can be determined within the time interval considered in Table 3. But one can also get general picture of the data gaps from Fig.4.

Specific comments

Figure 2. Could you indicate data gaps by just introducing a vertical bar and maybe the years? Or just show a PDF? As it is now I do not see an advantage showing a time series compared to a PDF. I do not understand the last sentence “Moreover, measurements on a given day are at close interval of few minutes to half an hour.” Why do you need it here?

Response

Figs 2-3 have now x-label as date. Although the interval between the date labels are not the same due to data gaps and irregular time interval measurements, it is still important to have the time series in order to associate how degrees of freedom, statistical and systematic errors vary with observed PWV from FTIR. We mean to say when observations are taken at irregular interval of time, it does not make sense to talk about data gaps. This is in contrast to GPS which is output at every 2 hours or ERA-Interim at interval of 6 hours etc.

Specific comments

Figures 5 to 6 – why don't you combine them in one figure? The standard deviation would certainly better characterize the agreement than the correlation coefficient.

Response

The suggestion is taken and now we have Fig.6 with three panels.

Specific comments

Figure 9 and 10 (left) Start x axis at 0.7 – otherwise you do not see the interesting features.

Response

Honestly, we did not understand this comment. The x-axes in Figures 9 and 10 are GPS station names. We have looked at other figures but could not see any figure that the comment potentially refers to.

Specific comments

SCIENTIFIC LANGUAGE Exemplarily I use the abstract to illustrate that the authors need to go through each sentence and check whether it is formulated in a scientifically sound way;
p. 4. “..but also because of its role in amplifying other feed-backs in general circulation models.”
Feedbacks occur in reality and the motivation is to make climate models reproduce them,

Response

It appears that the reviewer does not like any thing that is implied! The problem with being specific on everything particularly in abstract is the enormous increase in the length of the text.

Specific comments

p. 5 "In recent decades, monitoring of water vapour on regular and continuous basis is becoming possible as a result of increase in the number of deployed Global Positioning Satellite (GPS)

ground-based receivers at a faster pace.“ First there are also other monitoring techniques, e.g. MERIS see Lindstrot et al., 2014 and second the phrasing “at a faster pace” sounds like a commercial advertisement.

Response

We have rephrased this statement as follows:“ In recent decades, monitoring of water vapour on regular and continuous basis is becoming possible partly as a result of steady increase in the number of deployed Global Positioning Satellite (GPS) ground-based receivers.”

Specific comments

p. 13 “The PWVs from the three instruments and reanalysis show good correlation in the range from 0.83 to 0.92. “ Is this really good? I have seen several studies there its 0.95+

Response

We have already responded to the reviewer's previous comments whether the correlations were very good or not. We have already made minor changes by shifting from “very good” to “reasonably good”. It appears the reviewer is not comfortable with characterization of the results as good either. We differ from the reviewer on this point. We have showed at different parts of the manuscript that Ethiopia is characterized by complex topography and sparse observations. As a result, models are badly constrained. Moreover, the major circulations patterns are attenuated by local features such as complex orography and land surface characteristics etc resulting in highly localized small scale convective systems that can only be captured by meso-scale modeling approach.

Specific comments

p.- 14 “The radiosonde PWV shows dry bias with respect to other observations and reanalysis. ERA-Interim PWV shows wet bias with respect to all while GPS PWV exhibits wet bias with respect to FTIR.“ That is confusing why don't you say On average FTIR shows the highest PWV followed by GPS and radiosonde observations. ERA-Interim shows the highest vale x mm higher than FTIR .

Response

We agree with the suggestion and we have incorporated it.

Specific comments

p. 18 “Despite the sensitivity of GPS PWV to uncertainty in surface pressure in general, observed surface pressure is used only at four GPS stations” I don't think this has been such a conscious decision – I would reformulate and include an estimate about the uncertainty: Only four out of seven GPS stations included simultaneous pressure observations. Neglecting pressure information in the PWV retrieval can cause errors of up to?of

Response

We have taken this comment and included it.

Specific comments

p. 25 “The main cause of the variation is linked to variation in ECMWF model skill over different regions and seasons which might be related to poor observational constraint from this part of the globe and sensitivity of model convection scheme to orography among .”

Response

We have rephrased it as follows: “The cause of the variation is linked to variation in ECMWF model skill over different regions and seasons which might be related to poor observational constraint from this part of the globe and sensitivity of model convection scheme to orography among others.”