

## **Answer to Reviewer #2**

We would like to start by thanking you for all the time and effort which you spent reviewing our paper. All your comments, suggestions, and questions were taken into account and the necessary corrections were made.

### **General comments:**

Use of GPS derived Water Vapour (WV) in Europe is a well established techniques but there exist a large difference on regional level. While west and central Europe the topic has reached maturity in south and particularly east Europe it is currently under development. This paper presents the first results of GPS derived water vapour for Israel. Covering this region is a much needed positive development however this paper has major weaknesses, which make the study incomplete and needs to be addressed in full before proceeding to publication. Below is the summary:

### **Abstract:**

The first sentence “can help us to understand the physical conditions in the upper atmosphere” is incorrect. To the best of my knowledge GPS Meteorology niche is the lower atmosphere. The term “upper atmosphere/troposphere” is incorrectly used in the entire paper and I can advice the authors to seek collaboration with atmospheric scientists to cover the obvious gaps of knowledge in their team.

Our intention was to point out that GPS technology, in general, can be used to study both lower (IWV) and upper (TEC) atmospheric conditions, however we agree that in the context of severe weather and flooding events, GPS is used to calculate IWV in the troposphere. We changed the description here to lower atmosphere. The term upper troposphere is used in the introduction section when we indicate that the ability to constantly monitor changes in water vapor, at high spatial resolution, is insufficient especially in the upper troposphere due to the small concentrations of water vapor.

### **Introduction:**

The paper lack review of the-state-of-the-art in the GPS meteorology and Meteosat methods and water vapour products. The focus in the introduction is the WV derived with radiosonde (RS).

We have revised the introduction part and included more recent related research efforts.

Further problem is the use of misleading or general statements. For example:

1) line 59-60 “this problem manifests the most in the upper troposphere” – incorrect  
Corrections were made in the revised manuscript.

2) line 78-79 “: : ;, which conduct upper-air measurements to characterized the temporal behavior of atmospheric boundary layer” - incorrect and misleading. [Corrections were made in the revised manuscript.](#)

3) line 80-81 “This makes it almost impossible to precisely detect the horizontal boundaries between moist and dry air” - what do you mean here? [It is almost impossible to precisely detect the horizontal boundaries between moist and dry air when using a single permanent sounding site which gives a vertical profile.](#)

4) line 86 “vary constantly” - not clear does it “vary” or is “constant”. [Corrections were made in the revised manuscript.](#)

5) The terms WV/IWV/PWV are mixed in the text and also figures, which makes poor impression and makes the paper difficult to read. Also different units are used “kg/m<sup>2</sup>” and “mm” through the paper which is not helpful. [Corrections were made in the revised manuscript.](#)

In short the introduction is not focused and lacks: 1) review of the previous studies in the GPS Meteorology and products from satellites and 2) clearly defined aim and objectives of the study. Thus it is not acceptable in this form. [We have revised the introduction part and included more recent related research efforts.](#)

#### **Technical Approach and Methodology:**

The proposed in this section method to derive WV from Meteosat is not convincing. [We have revised the technical approach and methodology part](#)

#### **2.1 PW/IWV**

1) It is not clear how ZWD is obtained and what is its accuracy. [ZWD can be either obtained from: <http://garner.ucsd.edu/pub/solutions/gipsy/trop/> or calculated by GIPSY-OASIS software. We calculated it by ourselves using GIPSY-OASIS software and the input parameters for our strategy are now better described. The mean and rms values were added through the entire revised manuscript.](#)

2) The requested surface observation radius is 10 km. It is unclear why such narrow radius is selected and preferred. Published studies suggest that the appropriate radius of surface observations can be much larger. Unless sensitivity studies are done the selection of this radius seems arbitrary. [10 km is the closest distance from the IMS stations to the GPS stations. Based on \[Bai and Feng, 2003\] and Israel’s relative small area, we assumed that 10 km radius is reasonable value.](#)

3) Missing is information of derivation of surface temperature from Meteosat. The accuracy of this products is also not clear. [The technique which allows to translate METEOSAT-10 images to absolute temperature is described in documents related to](#)

METEOSAT-10, e.g. in PDF\_TEN\_05105\_MSG\_IMG\_DATA.pdf [Muller, 2010]. Briefly, we obtain pixel luminosity and due to the formulas in the document, mentioned before, translate it into the temperature.

## 2.2 WV

1) The proposed WV extraction from Meteosat is not convincing. Without proper treatment of the bottom part of the atmosphere this procedure is incomplete and thus the poor comparison reported in section 3. Methods to derive WV product from Meteosat WV channel have been published in the literature and it is advisable to review those methods or use the processed by Meteosat WV products. **We have increased the number of points and obtained better results.**

2) In my opinion the proposed Least Squares procedure (equation 8) to link GPS-IWV and Meteosat pixel value is not very appropriate. It will likely smooth the high temporal and spatial variability of WV. Thus it needs to be demonstrated that this procedure is appropriate on day to day basis. **This procedure might smooth high spatial variations, but not temporal, because of the fact that for the estimation described in equation 8 we used images which represent different seasons and weather conditions.**

### **Results:**

1) The advantage to use Meteosat temperature to surface observations is not clearly demonstrated. The large difference of “1.36 mm” is likely contributed by the accuracy of Meteosat product. **The main advantage of using METEOSAT temperature is the large number of measurements (every 15 minutes) and high spatial resolution (3-11 km, coverage for METEOSAT pixel).** It is also shown that in general temperatures from METEOSAT and IMS ground measurements are equal. For estimation of WV values we can use METEOSAT temperature because of the relatively weak dependency of IWV on temperature relative to ZWD.

2) The good agreement between the GPS and RS IWV is poorly demonstrated. Statistic with 9 points is not really meaningful. A proper comparison will need to be done covering preferably one year of observations in all seasons. **We have added more points which represent different seasons and weather conditions**

3) The reported large mean difference of 4.48 mm between GPS and Meteosat WV likely reflects the proposed method for derivation of Meteosat products in section 2.2. **After your constructive comment, we have revised our technique and obtained smaller mean difference – 2.75 mm.** The main problem of interpolation is that it is not sensitive to the areas with bigger amount of water vapor (for example, clouds), which are located between stations. For clear sky conditions and flat areas, in our opinion, the results are good in terms of bias and error values.

**Conclusions:**

Incomplete statements needs to be carefully reviewed and corrected. [Corrections were made in the revised manuscript.](#)