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Interactive comment on "High Spatial Resolution mapping of Precipitable Water Vapor using SAR interferograms, GPS observations and ERA-Interim reanalysis" *by* W. Tang et al.

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Dear Reviewer, We thank you for your comments and suggestions concerning our manuscript. These comments and suggestions are valuable and helpful for improving our paper. We have carefully gone through the comments and the suggestions, and have made the corresponding corrections in the revised manuscript. The text in the manuscript is revised and adapted in different places according to the reviewer's comments. Point-to-point responses to the comments are given below in red. And the modified text also mark in red in the revised manuscript.

Responds to the comments: Anonymous Referee #2

1. This is a relatively new domain, with high potential for the high resolution capability of a Syntetic Aperture Radar (SAR), although a major limitation is the low repetition frequency. The launch of Sentinel 1B can overcome this limitation since the repeat interval of the Sentinel 1 constellation goes down to 6 days (I suggest to mention this in the paper).

Ans.: Yes. With the new launch of Sentinel-1A satellite (launched in April 2014), we can get SAR data with a repeat acquisition rate of 12 days and in combination with the recently launched (April 2016) Sentinel-1B, the acquisition rate decrease to 6 days. This high repeat rate together with the large illuminated swath (250 km) make the Sentinel 1 constellation a more attractive source of data for meteorology studies. This is added to the revised manuscript in the part of abstract and introduction.

2. The paper is quite clear, although there are some sentences that should be revised for the English and repetition to be avoided. Attached there is a revised version of the manuscript with suggested English revisions.

Ans.: The English revisions was adapted according to the suggestions in the supplement file from the reviewer.

3. Page 2, line 12: 10-20 is the ground resolution of the SAR image (e.g., case of Envisat images used by the authors) but I doubt the resolution of water vapour is the same magnitude for several reasons. For instance, a multilooking could be necessary (you considered 40x8 looks), the image is built by the synthetic aperture which is several km long and thus spans different path in the atmosphere (similarly to the reversed cone they describe as for GPS), and so on. This is something that worth to be shortly discussed.

Ans.: Yes, when multilooking processing is applied, the resolution (exactly the pixel size) is reduced (Hanssen, 2001). We discussed the advantage of InSAR for water vapor mapping in this sentence, rather than discussing the specific resolution we got in this paper. Here, we used “as fine as” to describe that under the best circumstances that the highest resolution the ENVISAT SAR system can achieve.

4. Page 2, line 32-34: this is misleading. From the paper, I understand you are not using GPS to retrieve a calibrated “absolute” PWV map, but still providing a differential ΔPWV map with the unknown bias inherent to InSAR removed by using GPS. If I understand correctly, the sentence should be revised. For instance you may consider to write: “The main problem is that the ΔPWV differential maps from InSAR suffer from an unknown bias, which requires a reference observation to be removed. This calibration procedure was implemented by using absolute measurements of PWV from a few GPS stations in our study area.”

Ans.: Yes. You understand correctly. We just used GPS observations to calibrate the bias for differential water vapor maps (ΔPWV) from the differential SAR interferograms, this bias is caused by the unwrapping procedure. We used the word “relative” to refer this bias would be confused because the calibrated ΔPWV is still relative, and the “absolute” refer to the water vapor information on each SAR acquisitions. We have revised this sentence in the manuscript to make it more clearly.

5. Page 4, eq. 3 and 4: please provide reference for these equations. Is it (Hanssen, 2001) or (Smith and Weintraub, 1953)? Help the reader to retrieve the exact reference for you formulas.

Ans.: We got these equation from (Doin et al 2009). The reference of (Doin et al, 2009) has been added.

Doin, M. P., Lasserre, C., Peltzer, G., Cavalié, O., and Doubre, C.: Corrections of stratified tropospheric delays in SAR interferometry: Validation with global atmospheric models, Journal of Applied Geophysics, 69, 35-50, 2009.

6. Page 4, eq. 3: the pressure P(z) is written as function of a generic height “z”, whereas the equation provides the total phase delay. Moreover, the g0 is not exactly the ground gravity acceleration, but the acceleration at the mass centre of a vertical

atmospheric column, according to the Saastamoinen model (1972). Please revise as necessary.

Ans: Yes, the pressure P, water vapor partial pressure e and temperature T are a function of height z. We changed the term âĹĚ_hyd and âĹĚ_wet in the left hand side in equation (3) and (4) to âĹĚ_hyd (z) and âĹĚ_wet (z) to indicate that this two term are also as a function of height z. As for the term g_0, I have read the reference of Saastamoinen (1972) carefully, and found that you are right. g_0 is the local gravity at the mass center of the atmospheric column between z_0 and z_ref. Although they have very similar value, they have different physical meaning.

7. Page 5, line 5: it should be 1/cosïAËŻs′inc

Ans.: Revised.

8. Page 5, line 15: You compute the dry delay by using eq. 3. I am wondering why you do not use the rigorous formula of the dry component of refractivity, and compute it from ERA, as done for the wet component; consider that from the ERA output you know the pressure but also the temperature as well at each height. Please add a brief comment on this.

Ans.: According to (Doin et al, 2009), we used equation (4) to calculate the hydrostatic and wet delay. It is equivalent to using the rigorous formula of the air refractivity.

9. Page 5, line 11-14 and line 22-24: The following point is not made clear in the paper in my opinion. For more details, you can refer to the discussion in Basili et al., 2014. The absolute delay (both dry and wet) are function of the surface height (i.e., the topography). Sampling speaking it is due to the different thickness of the atmosphere interposed between the surface and the antenna. The dependence of this decreasing trend is roughly linear (with slope K) or, better, exponential (Basili et al., 2014 for the wet component). In lines 11-14 it seems you are discussing the dependence of such an “absolute” PWV on topography. Instead, in line 22-24 and in Fig. 2c you are showing

the differential ΔPWV computed from InSAR. In this case the dependence on height of ΔPWV is due to the difference in the atmosphere stratification, which determine the slope K. Therefore, it may happen that ΔPWV can decrease but also increase with height (as in fact happen in the figure, where a dynamic range of 2 cm is apparent, or in some plots of Fig. 10), being eventually negative. I suggest to clearly pointing out this aspect.

Ans.: With regard to "Page 5, line 11-14", we discussed the importance of the hydrostatic delay which should not be negligible. Instead, the hydrostatic delay was predicted by using ERA-Interim reanalysis and subtracted from the total delay to obtain more accurate maps of wet delay and then more accurate maps of water vapor. For "Page 5, line 22-24", we just demonstrate the hydrostatic delay (Fig. 2c) predicted from the ERA-Interim model. We discussed the dependence of ΔPWV on height in Page 9, lines 17-21. We discussed this dependence with more details in that place according to your suggestions.

10. Page 5, eq. (5): this is a "differential" ΔZWD InSAR, according to the previous discussion.

Ans.: Revised.

11. Page 5 and 6 eqs 6 and 9: provide a reference.

Ans.: References are added.

12. Page 6, lines 20-24 and Fig. 3: This is not clear to me. If Π is computed using eq. (7) the inverse relationship with Tm (i.e., 1/Tm) should be exact (all the other quantities are constant) so I would not expect a scatter of points in Figure 3, neither a linear trend. Please clarify.

Ans.: The value of Π is computed using eq. (7) and Tm is calculated using eq. (9).What we plotted in the Fig. 3 is the Π values with the average Tm for the thee ERA-Interim grid points located within the SAR scene (see Fig. 1). So that is why you see this scattering rather than an exact linear line. If using the Tm for each ERA-Interim grid point, the plot would be the exact linear line.

13. Page 7, line 4: I suggest some editing to avoid confusion between “differential” measurement and bias error that characterize the InSAR retrieval of water vapour (see note in the pdf file).

Ans.: Modified according to the supplement file you provided.

14. Page 7, line 18: as K is added to InSAR, should you write “subtracting” rather than “adding”.

Ans.: Yes. I have changed it.

15. Page 8, lines 16-23: a similar comparison was performed in Cimini et al., 2012, so it is worth to make reference to this paper somewhere (your matching score seems to be better).

Ans.: Yes. This paper collected water vapor (WV) measurements from different techniques (radiosounding, ground based microwave radiometers, GPS, MERIS, MODIS, ECMWF ERA-Iterim) and compared the accuracy of the WV retrieval. We have a similar result with this paper when compared the WV from MERIS and from GPS. We have added a sentence to describe this and provided the citation.

16. Page 8, lines 29-31: A smaller value of height ambiguity means that a given height change produces a larger phase difference, that is there is a larger sensitivity. Am I wrong?

Ans.: The height ambiguity is defined as the height difference corresponding with a 2π phase change and represents the height sensitivity of the interferometric phase. It depends on the interferometric configuration, i.e. satellite height, radar wavelength and spatial baseline. h_a=(λRsin(θ))/(2B_n) Therefore, the relationship between the height H and the phase difference ΔÏŢ is ÎŮ=h_a$\times$ΔÏŢ/2π

You are right, small values of the height of ambiguity indicate the larger sensitivity of topographic measurement, i.e. the more accurate the topographic measurement for InSAR of topographic mapping application. In our paper, we generated the DInSAR differential interferograms by subtracting topographic phase using external DEM. Errors in the external DEM used to remove the topographic contribution will propagate into the phase results. The sentences in the manuscript maybe confusing so they have been replaced by "Small values of the height of ambiguity indicate that possible errors in the external DEM could generate only negligible phase artifacts." Hope it will be more clear.

17. Page 9, lines 17-20: this is another sentence which is misleading, since it confuses the decrease of "absolute" delay with height with the dependence on height of the "relative" delay, which is the quantity plotted in Figure 8. Please correct or clarify if I am wrong. Than the plot does not say that PWV decreases as altitude increase, but rather that the trend of PWV with height was different in the two days, and thus ΔPWV is still dependent on height. This is a major point to clarify.

Ans.: Yes. You are right. In fact, the "absolute" water vapor content (the dominant source of delay) generally decreases with increasing terrain altitude because of the tapering of the atmosphere over higher surfaces, but this relationship is not true for the "relative" or differential delay in the InSAR interferograms. The actual delay vs. height trend is associated to the atmospheric vertical stratification, which in turns depends on the specific meteorological conditions at each SAR acquisition times. We cannot conclude that in the manuscript so we changed it to just say the delay is significantly correlated with terrain height.

18. Page 10, lines 1-6: This sentence probably may better accommodate in the conclusive section. However, you should give an idea on how the differential information ΔPWV can be exploited for the applications. Are you thinking to some specific assimilation approaches (e.g., 4D-VAR) or others? Note that the problem was faced in Pichelli et al., 2014, where e method to get an absolute PWV merging an external reference

(e.g., the model itself or MERIS) and InSAR was proposed.

Ans.: I think it is better that those sentences to be a small conclusion in this section because the comparison of MERIS and GPS in this part. And the conclusion of the whole paper will be the water vapor maps form InSAR. The reference you mentioned is very helpful, I did not know this paper before. It would be more interesting to be as an additional constraint in variational data assimilation models if we could retrieve the absolute water vapor information at each SAR acquisitions. But the difficulties are that isolating the potential deformation signal from the atmospheric signal.

19. Page 11, line 9: you cannot conclude the resolution performance is 20 m as you needed to multilook SAR images and work at 160 m resolution.

Ans.: Yes. I revised it.

20. Page 20-21, Fig. 9: It can be interesting to put all the points in a scatterplot as done for a single interferogram in Fig. 8 to appreciate the correlation between the two datasets.

Ans.: We have plotted all the points in a scatterplot in Figure 10. And explained in the text. 21. Page 16, Figure 2c and 2d: I understand these represent a differential delay (from InSAR). Please clarify in the caption.

Ans.: Revised.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/amt-2015-391/amt-2015-391-AC2-supplement.pdf

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