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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.

Anonymous Referee #2

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The paper deals with the retrieval of integrated precipitable water vapour (IPW) from SAR interferometry and a GPS receiver ground network. 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). The paper is quite clear, although there are some sentences that should be revised for the English and repetition to be avoided (I am not an English mother tongue but I am proposing some editing in the pdf file). Generally it sounds technically correct. The main concern regards some confusion about the absolute PWV respect to the relative (i.e., time dif-

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Discussion paper

ference) ïĄĎPWV. As better explained in the detailed comments below, it seems the author sometime discuss the behaviour of the former (e.g., its dependence on height) whereas the discussion is referring to the latter. Finally, the authors did not include in their reference list some papers, which deal with the same topic. Attached there is a revised version of the manuscript with suggested English revisions. Note that symbol "delta" is not correctly reproduced so you should read "delta_PWV" when you see a strange font.

Detailed comments:

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.

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 ïĄĎPWV 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 ïĄĎPWV 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.”

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.

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.

Page 5, line 5: it should be 1/cosïĄśinc

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.

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 ïĄĎPWV computed from InSAR. In this case the dependence on height of ïĄĎPWV is due to the difference in the atmosphere stratification, which determine the slope K. Therefore, it may happen that ïĄĎPWV 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.

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

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

Page 6, lines 20-24 and Fig. 3: This is not clear to me. If ïĄń 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.

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).

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

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).

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 ?

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 ïĄĎPWV is still dependent on height. This is a major point to clarify.

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 ïĄĎPWV 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.

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.

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.

Page 16, Figure 2c and 2d: I understand these represent a differential delay (from InSAR). Please clarify in the caption.

1. Cimini D., N. Pierdicca, E. Pichelli, R. Ferretti, V. Mattioli, S. Bonafoni, M. Montopoli, D. Perissin, "On the accuracy of integrated water vapor estimates and the potential for mitigating electromagnetic path delay error in InSAR", Atmospheric Measurement Techniques (AMT), doi:10.5194/amt-5-1015-2012, 5:1015–1030, 2012. 2. P. Basili, S. Bonafoni, P. Ciotti, N. Pierdicca, “Modeling and Sensing the Vertical Structure of the Atmospheric Path Delay by Microwave Radiometry to Correct SAR Interferograms," Geoscience and Remote Sensing, IEEE Transactions on , vol.52, no.2, pp.1324,1335, doi: 10.1109/TGRS.2013.2250292, Feb. 2014 3. Pichelli, E., Ferretti, R., Cimini, D., Panegrossi, G., Perissin, D., Pierdicca, N., Rocca, F., Rommen, B., “InSAR Water Vapor Data Assimilation into Mesoscale Model MM5: Technique and Pilot Study”, IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, DOI 10.1109/JSTARS.2014.2357685, 2014

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/amt-2015-391/amt-2015-391-RC2-supplement.pdf

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