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Discussions

Interactive comment on "Water vapor mapping by fusing InSAR and GNSS remote sensing data and atmospheric simulations" *by* F. Alshawaf et al.

Anonymous Referee #2

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The paper presents the results of fixed-rank kridging applied for merging two vertically integrated water vapor fields (precipitable water, PWV) - one obtained from a combination of InSAR and GNSS the other from a WRF simulation. One example is shown, for which the merged fields show a slightly better comparison with MERIS observations than the InSAR/GNSS field in terms of spatial correlation coefficient and rms. The WRF field compares much worse with the MERIS observations for the shown case.

General remarks

While the method presented looks attractive, since it allows to merge spatial observations of very different nature (distributed point measurements and gridded areal averages), I have severe doubts that from the two numbers reported (correlation coefficient

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and rms for one scene) any conclusions – as done here - can be drawn. Also, just by looking at the fields, I would not have guessed that outcome in numbers. Without the evaluation of a representative number of scenes, such conclusions cannot be drawn. But there are other reasons, which in my view make the conclusions even more questionable.

1) While the quality of the observation data from remote sensing mainly depends on the applied measurement/estimation method, the quality of the model field depends mainly on the quality of the forecast (here dynamical downscaling). So while the error of the former has in principle no spatial correlation, the error of the latter will by highly correlated in space because it will depend on the quality with which atmospheric flow is predicted. For example a timing problem in the prediction might generate a very accurate field but due to e.g. a delay the patterns would be shifted and lead – when compared point by point - to a very bad quality. The method presented here, however, does not account for this most common error of predicted atmospheric fields. Thus there is a large potential that the WRF field rather tends to worsen the results of merging. Since the WRF field is produced by double nesting in a quite large area (Fig. 2) without data assimilation within, I do expect considerable timing errors.

2) The region, for which the case study is performed, is characterized by strong rather large-scale topography, which is of course dominating the vertically integrated water vapor field (high in the Rhine valley and low over the ridges). This alone leads – by the way – to the nice resemblance of the MERIS and WRF data e.g. in Figure 3. And it will of course improve the merged product in places, where the InSAR/GNSS observations are not available due to incoherent scatter. The use of topography information in the merger – instead of WRF – would probably lead to even better results, especially when the timing error in the WRF field is large. I assume, that with the topography information added, WRF would not lead to any improvement. The authors need to test this.

3) I hypothesize, that the improvement by adding WRF is solely due to the filling in of WRF data in areas, where InSAR/GNSS fails, namely in the forested and usually

HESSD

12, C1109–C1112, 2015

elevated regions. Again using topography could lead to the same if not better results.

4) Also MERIS comes with an error. I did not find where this error enters the methodology. Also MERIS cannot estimate PWV where there are clouds; the case shown must be a very rare cloudless case.

Thus in conclusion, I recommend to reject the manuscript, but strongly encourage to resubmit with the points raised above taken care of. In preparing a revised manuscript, I strongly recommend considerable shortening; the current text feels like a cut-and-paste from a PhD theses. There are also many repetitions. Reduce the statistical theory to the necessary. Why – again – explaining kriging. Better explain the new method and concentrate on the differences to kriging. Also the way the InSAR/GNSS product is produced needs more explanation, and how often such a product would be available.

Further remarks

1. Abstract: avoid the terms “accurate” without putting numbers to it. I question that the PWV maps have accuracies of submillimeters. As written above, the second to last sentence/conclusion cannot be drawn based on the results.

2. 364/21: What do you mean with volumetric concentration? Check the value.

3. 364/23: Neutrosphere is strange. Better use troposphere, where the water vapor you measure really is.

4. 364/25: There is no “precise” prediction of clouds and precipitation.

5. 365/20: What is meant with the atmospheric phase?

6. 366/11: You have to define the scales (lengths) here.

7. 366/16ff: The meaning of this sentence is unclear. Parameterizations change the model outcome with variable effect on the quality depending on the situation/climate. Better remove it.

HESSD

12, C1109–C1112, 2015

8. 366/23: What do you mean with spectrally?

9. Since the merging of 2 data sets is proposed for a better analysis of vertically integrated water vapor fields, the availability of the InSAR/GNSS should be discussed in the introduction in view of water vapor variability in time.

10. 373/19: What exactly is the roughness of a spatial vatiation?

11. Remove the WRF wet delay from Fig. 9. It is not discussed in the text and not described in the heading.

12. Use the same gridding/area in Figs 10. and 11 and put more numbers at the water vapor color bar.

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