

angeo-2019-87-RC2: Answers to the referees' comments

Dear Editors and Reviewers,

We would like to thank you again for your valuable comments and deeply appreciate the time and effort spent on the reviews. We hope that the current version of the manuscript is now acceptable for publication in the Annales Geophysicae and look forward to hearing from you soon.

In the following, we address all the provided comments and suggestions in detail.

Yours sincerely,
Marion Heublein

On behalf of the co-authors Patrick Erik Bradley and Stefan Hinz

General remark

The finalizing comments of the second reviewer are answered below. The respective changes in the manuscript are color-coded in **blue**.

Referee #2: Modifications & answers highlighted in blue

Major comment:

In case of major comments you unfortunately haven't repeated your processing using a vertical discretization with a higher number of layers as I recommended to you (i.e. at least with 10 vertical layers). I agree that your work is focused on evaluating the new methodology for GNSS tomography (CS), however the experiment still should be based on rational conditions under which the tomography can be providing some useful information about 3d water vapor field. I admit that in your revised version of the manuscript (namely in Section 5) you point out weaknesses of your study (including the discretization), however if you are really not able to repeat your processing using a more detailed vertical discretization (still based on synthetic slant delays and a single WRF simulation) I ask you to at least emphasize in the text the potential limitation of using only 5 vertical layers on the validity of the results. If this is done and my below given minor comments are solved, I can agree with the publication of your manuscript in Annales Geophysicae.

We emphasized the potential limitation of using only 5 vertical layers on the validity of the results.

Minor comments:

- P1L18: I am not sure if "three dimensional (3d) atmospheric water vapor distribution is essential for climate research". I would say that climatologists are rather interested in spatio-temporal regional and global trends in water vapor 2D distribution and can live without the 3d information. Please reformulate the sentence accordingly or provide a reference for your statement.

We reformulated the sentence.

- P1L20: Can you provide a reference for your statement that "a precise knowledge of the water vapor field e.g. is required for accurate deformation monitoring using InSAR."? Water vapor does not influence only microwave signals, it causes a problem also i.e. in optical remote sensing

We added a reference.

- P8L3: I think you can shortly note in the text that numbers of signal directions you use as GPS-only or multi-GNSS are a bit pesimistic and in reality one can expect higher numbers. Usually at permanent GNSS stations (from which most of them have excellent visibility over the sky) you have 8-12 observations for GPS-only, 12-18 for GPS+GLONASS and we can expect around 25-35 satellites once Galileo and BeiDou are fully operational. Regarding the BeiDou - you answered my question that "there is no special reason." why you have ommited this system from your work, however I think you should inform the reader that there exists also this 4th GNSS which can be also used to increase the number of signal directions in tomography applications.

We informed the reader that BeiDou could also be used to increase the number of signal directions in tomography applications and that there may be up to twelve observations for GPS-only.

- P8: from L9 to L27 the text is repeated, I mean it is the same as text which you have from P7L24 to P8L9

Thanks for your attentive reading. We deleted the repeated text.

- P8L28: in your section 3.4 you repeat some information which were already given and I think the rest of the information can (should) be provided within the section 3.3. Therefore I think sections 3.3 and 3.4 should be merged into one section to improve the readability of the manuscript.

We merged the two sections into one in order to improve the readability of the manuscript and updated the title of the merged section.

- P10: what I see in figure 3 is: LSQ is everywhere except four boxes in the upper-left and lower-right corner providing better results than CS - although you summarize this in Section 5, I think you should mention it also here.

We also added this information into the description of Figure 3.

- P10L9: I recommend to replace "absolutely need to" from your sentence "... the chosen solution strategy as well as the effect of varying signal directions absolutely need to be taken into account." with "should be taken into account". Your statement with "absolutely" is too strong in my opinion considering the limited dataset you use.
Thanks for the remark. We replaced the terms as you proposed.
- P11, figure3: please check figure for 5 signal directions/27 sites. It has some problem with plotting the caption for CS mean/std.
Thanks for the remark. We solved the plotting problem.
- P12: I am addressing my major comment for P8L13 from my previous review - in your reply you write: "We also analyzed differences among individual voxels but did not find any significant differences among the individual voxels. We also studied the distribution of the differences at various height levels. Yet, LSQ or CS did not systematically provide better / worse results either in the boundary layer or at the top of the troposphere." Please, add all this information to your manuscript to inform the reader about these results.
We added all this information to our manuscript.
- P14L3: this is the first time in the manuscript you use term "ZWD", you should therefore provide its full meaning and ideally shortly describe the relation between ZWD and SWD
We defined the abbreviation ZWD and added a reference for mapping between ZWD and SWD.