

Interactive comment on “Alternative Strategy for Estimating Zenith Tropospheric Delay from Precise Point Positioning” by Jareer Mohammed et al.

Anonymous Referee #2

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In this manuscript Authors describe alternative strategy for estimating ZTD from precise point positioning method. The main assumption of proposed approach is to estimate both hydrostatic and wet part of ZTD using different process noise and different mapping functions for both components. Generally, this is very interesting research but some major remarks should be taken into account before this manuscript can be accepted for publication.

Major remarks and questions:

1. Authors presented results of ZTD estimation obtained using alternative strategy of PPP method and compared these results to ZTD obtained using double differenced

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method. Why Authors did not compare their results also to the conventional PPP?

2. Line 24-26: It is not entirely true. Authors should remember about VMF, where different values of ZHD and ZWD are provided during the day.

3. In my opinion, in the introduction there is lack of information about VMF. Authors can also pay attention to VMF3/GPT3 (doi: 10.1007/s00190-017-1066-2).

4. Table 2: Which ANTEX version was used by the Authors? Why Authors did not fix ambiguities?

5. In PPP solution Authors used precise products from NRCAN while in DD products from CODE. Products from different sources can cause differences in parameter estimation, which may lead to wrong conclusions. Ephemerides and clocks should be used from the same sources, either from CODE or from NRCAN. In my opinion it will be nice to see comparison between two PPP methods: first with conventional strategy, and second with alternative strategy proposed by the Authors. Of course in both solutions the same processing parameters and precise products should be used. Moreover, Authors should remember that for precise tropospheric parameter estimation the VMF1 is often used. Unfortunately Authors did not provide any information about it and did not use solution with VMF1 for comparison (e.g. in Table 5). I think that this is a big deficiency of the presented manuscript. In the paper it is hard to find explanation for this, especially that VMF1 was used in DD solution.

6. One more question related to solutions using traditional strategy. In section 3 Authors presented used tropospheric models. Why Authors did not use GPT2 which is more precise than other presented models? Of course the VMF case should be also reconsidered in this place.

7. I have also general comment to all results presented in tables: 5, 6, 7, 9, and 10. I think that in case of ZTD differences it will be nice to see also maximum and minimum values. I also advise Authors to think about replacing mean values into a median.

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8. In validation section it is not clear how the ZTDs from IGS and Authors strategy were compared. In IGS tropospheric products are available with 5 minutes interval. I think that information about estimation interval should be placed into manuscript (both for IGS and Authors solution), as well as information how the comparison of two products looked like. Was ZTDs comparison conducted at the same epochs or maybe some averaging was used? It should be clearly explained in the text.

9. Section 6.1: I have serious objections to the content of this section. Authors show how the 'new strategy' impacts on convergence time. However they only presented comparison to the low precise tropospheric models. I think that it is necessary to present results to more precise solution e.g. with VMF. Furthermore, Authors did not provide any information about processing strategies, or number of used stations. Readers may also have a problems with results interpretation. Are there mean value of convergence time in Figure 7? Or maybe these values are for one station? If there are mean values, Authors should present also RMS or STD parameters.

10. Section 6.2: In this section Authors compared only for two stations. Why exactly these? Authors present results for conventional and alternative approaches. Which tropospheric model was used in the conventional solution?

11. In presented by Authors solution the ZHD and ZWD components are estimating separately. In presented manuscript Authors presented only total value of ZTD. However this not mean that ZHD and ZWD are correctly estimated. For example there can be some biases for both component but with opposite signs. Thus in ZTD this error will not be visible. It should be notice that proper estimation of ZWD is crucial for many application, e.g. for conversion to IWV and analysis of atmospheric opacity is performed (doi: 10.1007/s10291-017-0675-9). Unfortunately in presented manuscript there it is not explained whether the ZWD can be directly used for such (or similar) application.

12. In presented manuscript there is also lack of ZHD and ZWD estimation errors analysis. I think that it is necessary to show how the values from covariance matrix

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looks like during the processing time. Also it will be nice to see post-fit residuals. Of course, only examples for selected stations can be presented.

The proposed by Authors strategy is very interesting but it needs to be checked, tested, and verified more deeply before publication.

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