Interactive comment on "Tropospheric products of the 2nd European GNSS reprocessing (1996-2014)" by Jan Dousa et al.

Responses to Review #1.

RC1: Dousa and Vaclavovic provide a clear and systematic report on the tropospheric products from a reprocessing of the EUREF network. They use both internal metric – the RMS of coordinate repeatabilities - as well as an external comparison with numerical weather model reanalyses to assess the performance of the standard reprocessing approach as well as a suite of variants designed to test the impact of possible alternative strategies. They are able to validate a significant improvement in the reprocessed timeseries and identify recommendations for optimal performance in the future processing.

The most notable result from their work is that a low - 3-degree - cutoff angle for the data provides the best results. Although this is expected to improve the geometric distribution and help decorrelate the vertical position and troposphere parameters, the mapping functions and antenna models are not expected to be well described for very low elevation angles. It is therefore noteworthy that this processing strategy improved the results.

As it concerns the solution using the lowest elevation angle, the use of VMF1 down to 3 degrees is, in principle, not problematic from a theoretical point of view as the VMF1 has been estimated using numerical weather data ray-tracing at elevations of 3 degrees only. The mapping function is thus optimized for 3 degrees, while not always perfect for higher elevation angles. However, the impact is visible mainly between 'optimized' 3 degrees and roughly 10 degrees when mapping function is directly compared to ray-tracing at elevation angles above 3 degrees, e.g. F. Zus (2015) and D. Landskroun (2017) preparing the revised concept for VMF3 (EGU2017 presentation). A short note and the reference (F. Zus et al., 2015) were added to the manuscript (Section 4.1).

Zus F, Dick G, Dousa J, Wickert J (2015), **Systematic errors of mapping functions which are based on the VMF1 concept**, GPS solut,19(2):277-286, doi:10.1007/s10291-014-0386-4

RC1: Although their results might not be ground-breaking, they are carefully performed and provide valuable information to a range of researchers. With a few minor modifications I recommend this for acceptance and publication. There are a few areas that I think should be cleaned up or further explained in order to ensure that (particularly non-specialists) can better understand the work and its implications:

1) Section 2: I assume, though it is not stated, that a set of (global?) IGS sites were included in the processing of each of the sub-networks, both to ensure strong ties to IGS08 as well as provide the long-baselines necessary to ensure retrieval of absolute ZTDs?

The processed network consisted of EUREF Permanent Network (EPN) only, thus no global stations were included. The overall network extent is large enough (Min/Max Lat: +27°/+79°, Lon: -79°/+58°) to resolve tropospheric parameters in absolute sense and to ensure strong ties to IGS08 by many fiducial stations. The datum has been carefully revised whenever combining clusters into a full network solution and highly reliable a priori coordinates has been carefully applied in a sub-network solutions for pre-eliminating phase ambiguities. Two short notes were added (Section 2).

2) Section 3: It is definitely important that artefacts at day-boundaries are mitigated, and the strategy followed to ensure ZTD continuity seems perfectly good. The smoothness of the RMS across midnight is a good indicator that the strategy is performing as expected, it would be nice, however, to see a quantitative validation of the positive impact. Can this be pulled out of the ERA-Interim comparisons?

Unfortunately not for GOP solutions as we haven't prepared both variants (3-day and 1-day). However, we have evaluated two global Repro2 solutions from the IGS analysis centre CODE (Centre for Orbit

Determination in Europe) in GOP-TropDB previously, which included 1-day (official IGS contribution, COF) and 3-day (unofficial product, COD) solutions. The results indicated up to a several millimetres or even higher differences at midnights in ZTDs due to boundary effect. We suppose the actual effect might reach up to a centimetre or even more considering that values at the middle epoch of low-resolution ZTD products (2-hour) could have been compared only. The paragraph and reference to the CODE product and its evaluation in GOP-TropDB were added (Section 3).

3) Figure 4: I am uncertain what the panel titles mean? In the text (line 173) this figure is referenced as Figure 3, and suggests the data are from 1999, though the x-axis labels indicate 1996?

We are confused what is really questioned as line 173 has no reference to Figure 3 and we haven't found any mistake with the reference to Figure 4 in Section 4.1. However, we should clarify one possible misunderstanding: the GOP product cover the period of 1996-2014 while the year 1999 (day 209) was referred as having a minimum number (12) of fiducial stations applied for realizing of IGS08 reference frame. We slightly modified the text to be clear in this context (the last paragraph in Section 4.1).

4) Seasonality of coordinate RMS (Figure 5 & lines 228-230). Some statement about the source of the seasonality seems called for. This may include a citation where this has been previously described, though it might also/instead reference Figure 7 which indicated that the tropo gradients show a similar seasonality suggesting that the limiting factor may be in the modelling of the atmosphere (rather than a seasonal source(s) of increased ground motion).

Unfortunately, also here we have trouble to identify the correct place in the manuscript as the coordinate RMS is shown in Figure 4 and no relation was found within lines 228-230. However, we agree that a major contribution to the seasonality on coordinates (particularly height component) is due to the troposphere modelling as it is also clear from the high variability of wet zenith component (and most likely wet gradients) due to varying water vapour content. At the end of Section 4.1, we added a sentence describing the reason for seasonality in coordinate RMS and, particularly, the height component.

Minor comments: Section 4.2: discussion first of Table 5 and then Figures 6 & 7 (lines 270-324) means jumping back and forth between ZTD and tropo gradients. I would suggest re-ordering the discussion to address the ZTD portion of Table 5 along with Figure 6 and then the gradient section and Figure 7.

Thank you for suggestion, we have reorganized the text by splitting the evaluations of ZTDs and horizontal gradients into independent subsections and we believed it clarified the description. Section 4 was modified.

Gradient units: tropo gradients are not entirely straightforward to describe - and it is not clear in the paper what the numbers given actually represent, and strictly speaking the units of the gradients in ZTD (or ZWD) cannot be mm... so how are the tropo gradients being implemented, and what do the values given physically mean?

Although are principally assumed to represent ZTD change with a distance (in North and East directions), practically, they have to be parametrized using elevation angles of individual observations instead of the distance. However, the relation between the effective distance and elevation angle requires assumptions about the effective height of the tropospheric effect. The interpretation of the tropospheric horizontal gradients in Bernese software introduces a small angle applied for the tilting the zenith direction in the mapping function with gradients representing (in unit of length) the tilting angle multiplied by the delay in zenith (Meindl et al., 2004). The description was added (Section 4.3).

Suggestions: Abstract, line 24. "assessing" = "comparing"?
corrected

Intro, line 33. "... (GPS) became fully operational in 1995..."

corrected

Intro, line 85. "enhance" = "improve"?
corrected

Section 3. Line 155. "... an interval, or b) by..."
corrected

Section 3. Line 165. "Finally, we represented the piece-wise linear solutions in terms of offsets,..."
corrected

Section 4.2 lines 241-242. Not quite sure what/where the "yield values" are referring to? This sentence needs a little work for clarity.
corrected

Section 4.2 line 249: "...as the same blind mapping function and a priori ZHD values are used for both the GO0 and..."
The sentence was finally removed from the manuscript (see other reviewer comments).

We would like to thank the anonymous reviewer for all the comments which helped us to improve the manuscript significantly.

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