Response to Reviewer 1's remarks on "Estimating ocean tide loading displacements with GPS and GLONASS" se-2020-22

We thank the reviewer and editor for constructive remarks which we respond to below in **bold.**

NOTE: On additional check we found some minor errors in our plotting routine which slightly affected figures 3, 6, 9, 10, 11 and their supplementary versions. We updated the figures and their analysis. These changes do not affect the conclusions.

The key updates are:

- **Fig 3: GPS AR (JPL) the variance in up is smaller by ~1.3 mm for S2 and 1 mm for K1 but larger by ~2.5 mm for K2**
- **Fig 6. CODE GPS a ~1 mm bias in S2 appears for solutions with cutoff angle >= 10 while median values change marginally.**
- **Fig 9. GPS (JPL) a ~0.7 mm bias in S2 east appears. The variances of K1 and K2 become smaller by ~2 mm**
- **Fig 10. GPS AR (JPL) shows a clearer linear dependency with cutoff angle.**
- **Fig 11. A constant ~0.3 mm S2 east bias appears**

Why, for example, is the OTL signal removed a priori in the GipsyX PPP solutions (using FES2004_Gbe, cf. Line 106)? Later, the OTL signal is added back in (imperfectly) using HARDISP (Line 140). Why not simply turn off the OTL model in GipsyX and attempt to recover the full expression of OTL, thus removing any doubt that the a priori model is somehow favoured via constraint? In this scenario, one could adopt a less constrained random walk (RW) for recovery of the position. Why indeed does the 3.2 mm/sqrt(s) recipe from Penna et al. (2015) again emerge as the optimal constraint for the RW position estimate? Figure 2 does not seem to suggest that the amplitudes of the recovered OTL constituents become unbounded with higher process noise. Indeed, they seem to stabilize. One wonders what the outcome would be with a more disruptive estimation strategy that allows the position to move more freely and independently (with no background OTL model). Perhaps I am missing something here, and would of course invite the authors to clarify.

The approach we use is the same as adopted by Penna et al (2015). This approach is adopted to 1) remove in a convenient way the companion tides using the hardisp routine and 2) it reduces time series variability and hence allows for the selection of a tighter time series process noise which helps reduce noise.

Our finding of the optimal process noise setting is the same as Penna et al (2015). This is not surprising to us given that we use a similar site which is therefore subject to very similar site noise, tropospheric variations and OTL. The key difference is the time period and the addition of GLONASS data. That the findings on the optimal parameters are the same as Penna et al suggests that data noise or product noise is less important than the geophysical signal. Note that we choose a slightly conservative process noise that allows for sites to have significant freedom. On additional testing, we found that the overall vector difference for M2 between the approach used in Penna et al. (2015) and approach with OTL not modelled during processing run is ~0.1 mm (Fig. R1), however, this difference also includes phase variations due to companion tides and hence this is an upper bound on the difference.

Given this is a standard and widely used approach we do not modify the manuscript.

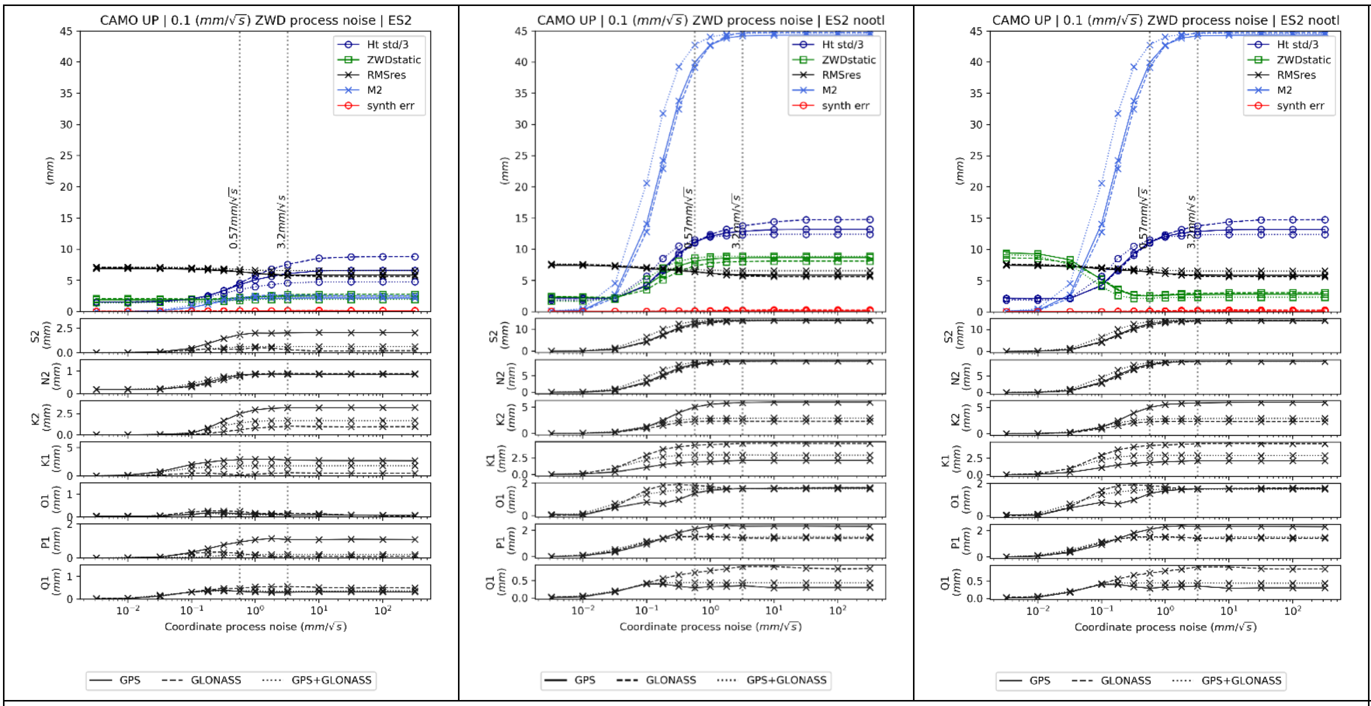

Figure R1. The effect of varying coordinate process noise at test site CAMO for the up component (2010.0 – 2014.0), performed with ESA repro2 products with OTL modelling enabled (left), disabled (centre and right) during PPP run. The right plot ZWDstatic was computed relative a static solution with OTL modelling enabled.

I think the estimated clocks and zenith troposphere also provide clues to this sensitivity problem. One could probe the time series of these “nuisance” parameters for signs of energy at the sidereal periods linked to the GPS repeats.
We tested the difference in estimated wet zenith delay between solutions with and without modelled OTL and found the difference to be negligible thus demonstrating that the zenith delay is not absorbing OTL with the chosen settings. The tropospheric gradients were also analysed, and the differences were again negligible. Given these and earlier tests by us and Penna et al. (2015) we are content that the solutions are optimal. To test if any tidal signal was absorbed by the wet zenith or gradient parameters, we repeated the solutions but without OTL modelled at the observation level and compared the sets of parameters. We found negligible differences suggesting that OTL is not being absorbed into these parameters without chosen process noise settings.

One other suggestion is to consider relegating additional detailed discussion to the supplementary material. The salient points sometimes get obscured by the detailed descriptions of the results and cases. The paper is otherwise well written.
We have reviewed the manuscript for overly detailed discussion of the results and editing the text appropriately throughout.