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Interactive comment on "Estimating ocean tide loading displacements with GPS and GLONASS" *by* Bogdan Matviichuk et al.

Anonymous Referee #2

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This paper considers the estimation and quality control of ocean tide loading (OTL) displacements estimated using GPS, GLONASS, and GPS+GLONASS. The paper is largely incremental in nature, closely following the GPS OTL displacement measurement work of Penna et al (2015) and using the same regional (largely UK) data set, but extending their work to include more tidal constituents, three coordinate components, some different satellite orbit and clock products, and the GLONASS constellation and its combination with GPS. Such GLONASS and GPS+GLONASS OTL measurement demonstrations have yet to be published, so the dissemination of these tests is of interest and benefit to the scientific community. I do however feel that there are quite a lot of aspects (as detailed below) that need to be improved and/or carefully checked before the paper can be deemed acceptable for publication.

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Main points (in sequential order):

1. Section 1 Introduction: It would be helpful to make clearer what the main aims and objectives of the paper are, in order to better put the many documented tests into context. The Introduction provides a generally useful overview of previous works and some limitations, but the last paragraph fails to build on this, just describing what will be included in the paper, without explaining why such tests are being done, or what the paper is seeking to achieve and why. Please also revise some of the description about why anelasticity etc has not been studied until recently, as it is not the case (L41) that limitations in PREM were the reason. The limitations were the accuracy of the ocean tide models (as recognised on L47) and the quality and availability of GNSS measurements/processing.

2. Section 2 Dataset selection: From Figure 1, one cannot see what M2 up OTL displacement the actual GNSS stations experience, except for those stations right on the coastline. This figure would be much more useful if the displacement over the land were also plotted. Note that the ocean tide model series is TPXO, so the model is TPXO7.2, not TPXO.7.2 (Figure 1 caption and L368). All stations in the figures and study are in Europe (L84), so please describe the stations' geographical distribution more accurately. Regarding the selected stations, given that the GNSS minus model residual is being used as a quality indicator (with zero the aim), some indication of expected errors based on previous studies should be included. Are some of the larger residuals later shown likely to be associated ocean tide model or Green's function errors and hence not just indicators of GNSS measurement quality? E.g. Bos et al (2015) state that they excluded the GNSS stations CARI and SWAS from their geophysical results because of large ocean tide model discrepancies in the Bristol Channel.

3. Section 3 GNSS data processing: Please clarify what exact OTL model was used in the GipsyX processing and what was used in the subsequent results. It seems strange in a technical paper such as this where the variables are the GNSS constellations, orbits etc, to use some form of OTL model remove-restore procedure. Was

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hardisp used with FES2004 and Gutenberg-Bullen in the GipsyX processing? If so, I am not clear why it is then said on L140 that the "OTL displacements modelled in GipsyX were added back using HARDISP" when referring to the post-processed time series? So if FES2004/Gutenberg-Bullen was used in the GipsyX processing, the carrier phase residuals shown in Figure 2 presumably contain some FES2004/Gutenberg-Bullen errors, while the OTL up displacement also shown in Figure 2 is a residual to FES2014b/STW105d (how were these computed as, unlike for FES2004_GBe, there is no explicit statement of their source)? Hence are results from different OTL models displayed in the same figure, which would seem to be inconsistent? I do not follow the statement on L159-160 that FES2014b_STW105d is used unless stated otherwise, but then in Section 4, it is suggested that FES2004/Gutenberg-Bullen was used. Exactly what was used for which test needs to be clearly explained as it is hard to fully interpret the results at present, but why the need to mix and match? Please also note that CODE and ESA orbits provided via the IGS are in a centre of network (CN) frame, rather than CE. Regarding orbit and clock products, the authors should make clear that their statements regarding PPP AR and particular products are in the context of GipsyX, and not necessarily other GNSS processing softwares. On L136, please provide details as to what is meant by "The raw 4-yr timeseries were filtered".

4. Section 4 Process noise optimization: Figure 2 shows that the M2 residual varies with the coordinate process noise, which is to be expected, yet the synthetic signal introduced (what amplitude?) is recovered with apparent zero error with all process noise ranges. I think the authors need to carefully check their processing and implementation: there are various ways by which the effect of a synthetic signal can be introduced to GNSS processing, but intuitively if a very small coordinate process noise is applied, then one would not expect to be able to recover it. Then in the supplement, the authors show that they recover a 6 mm amplitude synthetic signal perfectly with a very small process noise (which is counter-intuitive), but not at all with a larger process noise. Note that Martens et al (2016), which the authors cite but not in relation to synthetic signals, obtain results corroborating those of Penna et al (2015), in that the synthetic

signal is not recovered if a very small process noise is applied. The authors also state in the supplement that in kinematic positioning constant nominal coordinates should be used, but this is not always appropriate, for example the case of a rapidly moving GNSS receiver.

5. Section 5 and Section 5.1: Overall, I found the order and logic of all these tests a bit piecemeal and not particularly well flowing and ordered. To improve the readability, I encourage the authors to improve the description of why each test was undertaken, and how they build on the preceding ones. Section 5.1, although entitled "Effect of using GLONASS", is really a generic introduction to the subsequent tests, as almost all of them assess the effects of GLONASS and GPS+GLONASS in some way. So I think this section would be better as an introductory Section 5, that then leads into the others. For example, it seems odd to include so little discussion in Section 5.1 on the benefits of using GLONASS for K2 and K1, and the overall performance of GLONASS and GPS+GLONASS. I was left wondering where this was given the section heading, until I saw a separate section called "K2 and K1 constituents". Please include quantification in the description of improvements, rather than simply saying, for example, "smaller". Any suggestions as to why there are variations in residuals among the different constellations and ambiguity fixing approach for the different coordinate components?

6. Section 5.3: It seems strange that the complete opposite of larger vs smaller GPS or GLONASS residuals arises when using CODE and ESA orbits and clocks. Any more explanation for this? For instance, I am not clear why the correction of atmospheric tides in the CODE products will improve GPS but not GLONASS?

7. Section 5.6 Noise and uncertainty: This paper concerns the generation and analysis of OTL displacement residuals. So if the modelled OTL displacements applied are in a frame compatible with the orbital products, as I assume has been done throughout the paper (this is implied on L106-107), then are the residuals (and their uncertainties) actually sensitive to the frame given the model and GNSS measurement precisions?

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Figures 3-5, in which JPL and CODE/ESA residuals are compared, do not suggest the residuals themselves are. It is not clear how the JPL products in CM "provide a significant advantage" (L288) over ESA and CODE products in CE (CN) in relation to phase uncertainties. The authors state that the effect will depend on the constituent's amplitude, but the CE-CM difference (and hence whether the CM value or CE value has the larger amplitude) will depend on the location globally. What exactly has been applied in the creation of the two panes in Figure 8? I am also puzzled by the results for the averaging of the amplitude uncertainties across all constituents. It seems odd that the K1 and K2 amplitude uncertainties are commensurate in precision with the lunar constituents such as M2 and N2, given the errors and associated problems with K1 and K2.

8. Section 5.8 Timeseries length: The authors should summarise the status of the various IGS Analysis Centre reprocessing and operational products for the reader who might not have intricate knowledge of what was done and when, and hence what timespan such products cover. What are the key differences between the repro2 products of 2010.0-2014.0 and those used for 2014.0-2019.0? If the operational products adopted repro2 procedures for much of this time, then one would not expect noticeable changes, but if major changes arose in the operational products during the 5 year window, then comparing time series lengths involves multiple variables, when the desire is to just vary the time series length. To me it would be better to describe the test shown in Figure 12 first, in order to evaluate the impact of any differences in products, as well as showing another test of measurement noise. The authors say they found no significant differences at M2, N2, O1, P1 and Q1, but Figure 12 suggests there is a noticeable difference at P1 for GPS, perhaps unsurprisingly given P1 challenges discussed earlier in the paper. It is stated on L335-336 that GLONASS showed significant differences for K1 and K2, but Figure 12 suggests the differences are bigger for GPS, so what is implied here? It is stated on L325 that all eight constituents' variation with time series length was assessed, but then only S2 and K1 are shown (in Figure 11) and discussed. I had to search through the supplement to find the other constituents, but this needs to

be made clear in the paper itself, and state why the emphasis in the paper is on S2 and K1. Many of the constituents appear (from inspection of the supplement) stable over time, which suggests that even if there are changes in the products, they are not having an impact. I think more should be made of this point in this section, as it is a positive result. I am unclear what is meant by the sentence on L338-339 – please rewrite.

9. Supplement: It is not clear what new information related to tidal cusps has been found by the authors or why they have raised this point. Section 6.1.1 of the Penna et al (2015) study does not state that there are tidal cusps, but discusses that any slight gradual increases in power around M2 are likely caused by spectral leakage.

Other points:

Please provide a concise statement defining the bounds of the box and whisker plots used throughout the paper, as there can be different conventions used. E.g. on L191 I did not follow what is meant as the stated lower bound.

Please decide on “vector difference” or “vector distance”, and use consistently throughout the paper.

L58: Not clear why ESA and CODE orbit/clock products are specifically mentioned here but those from other IGS Analysis Centres are not. The justification for these two is provided in Section 3, but on L58 the mentions are out of place and premature without more explanation.

L203: Please explain in what way different elevation cut-off angles will modulate the expression of signal multipath into solutions. I think you mean by this that you expect less multipath with larger cut-offs?

L209-210: What is meant by the sentence “For the. . . (including K1 and K2)”? It does not make sense, and what does increase the stability mean?

L211-213: Sentence contradicts itself. Please clarify.

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L221-222: By “partial expression”, do you mean partially propagate?

L253-255: Please refer to Figure 3 so that the reader can ascertain what is being referred to.

L256: Is ‘tightest’ a technical term?

L258-259: What is meant by “The GLONASS K1 east is not true”?

L270-271: What is meant by “better consistency between products”? I think it better to say that GLONASS gives smaller values, rather than being “preferred”.

L313-314: Please state where (this paper?) the “similar behaviour previously observed with ESA products” was.

L315-316: Please refer to the figure in question, and quantify “completely” and “slight increase”.

L316: It is mentioned that the implementation of ambiguity resolution results in a slight increase in the median of the up component. Any explanation for this? From inspection of Figure 10, I am not convinced anything of significance is showing compared with the float solutions.

The authors need to carefully go over the English and presentation to improve the readability of the paper (particular the use of “the”) and ensure all acronyms and abbreviations are defined. Non-exclusive examples (in Sections 1-3 only) include:

L3: “close to several” -> “close to that of several”

L4: is -> are

L5: over -> of

L6: “Western Europe” -> “western Europe”

L17: hyphen not needed in “solid Earth”

L24: Incorrect reference format

L31 (and elsewhere): The reference should be Wang et al (2020) not 2019. L451-453 is out of date

L33: I would put “predominantly” before “estimated”

L49: “areas”, rather than “conditions”?

L54: I would add “period” and “orbital”

L55: “constellation period” -> “constellation repeat period”

L62: “constellation period” -> “constellation repeat period”

L62: remove spaces before the “11” and “8”

L79: “observation” -> “observations”

L80: insert “the” before “selected”

L105: change to “within each processing”

L107: No need to describe the file format

L108: change to “using the free” L110: change “single products solution” to “single product’s solution”

L113: change “Products” to “products”

L134: no need to describe the basic merging / concatenation of IGS files

L143: insert “the” before “processing”

L146: insert “the” before “Eterna”

L148: remove “the” before “solid Earth”

L151: remove “using the procedure”

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L158: first bracket is in the wrong place

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