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Interactive comment on "Anassessment of GIA solutions based on high-precision GNSS velocity field for Antarctica" *by* Wenhao Li et al.

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The authors present an analysis of GPS time series with a focus on common-mode filtering and its effects on derived velocity fields. It then compares the results vertical velocities to a range of GIA models. This is a worthy topic of investigation.

I am concerned that the manuscript in its present form does not present a clearly robust advance on the work already published in the literate, some of which is cited in the manuscript.

1. The work is in part of update of the work of Liu et al and Martin-Espanol et al. Liu et al consider ICA in the context of Antarctic vertical velocities and compare the resulting velocities to GIA models. Martin-Espanol et al do the same but without the filtering.

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Discussion paper

The authors do not sufficiently engage with these papers to explain the advance of their work. The Liu et al work is mentioned in a single sentence in the introduction only despite it being so similar. I think the main advance is longer time series but there could be other things, but I also think there are some backward steps (see below). In general, the introduction does not contain a complete review of people who have worked on the testing of GIA models in Antarctica and so does not set its own unique contribution in context.

2. the robust comparison of GPS velocities and GIA models requires robust consideration of elastic deformation. The authors uses the grids of Riva et al. for this purpose. This approach is problematic due to 1) that product not overlapping fully in time with the authors' GPS time series; 2) the authors appearing to apply the correction as a time-constant rate whereas elastic deformation is potentially highly nonlinear; and 3) the Riva et al product being explicitly designed for *far* field studies since the input data sets are low resolution (from GRACE in the case of Antarctica), As such, the product cannot accurately represent changes over spatial scales <300km. This is a backward step compared to the approach of Martin-Espanol (although there is room for further improvement in this regard). As such, I do not think the GPS velocities, after correction for elastic effects, can be compare to GIA models robustly.

3. The discussion and context misses one key recent publication, that of Barletta et al in Science where they show that the Amundsen Sea embayment is likely underlain with low viscosity mantle and hence is sensitive to very recent load changes only. The discussion of the northern Antarctic Peninsula does not consider the work of Nield et al 2014 even though that work is cited elsewhere in the manuscript. There are instances where discussion or conclusions are made which are not new or from the present work. For instance, p9 lines 5-24 are either out of date or repeat points made in the literature previously (also p11 Line 10-16 and abstract line 22-24)).

4. some of the methods are not fully described. The authors do not describe when they estimate offsets in the timeseries (due to, for example, equipment changes) - the

methods (line 21 p 3) suggests just a single constant offset is estimated for each site - if this is so, this would be a major methodological error.

5. in common mode filtering there is a chance to remove some trends or accelerations by accident. The authors do not test if their ICAs show linear or long-period accelerations which could remove important velocity. IC3 seems to show something along these lines. The authors also do not try and relate the ICAs to any physical or systematic errors (this point not being critical but it does mean the filtering is very blind and just a mathematical application, which can be dangerous).

6. Section 2.1 says the authors remove offsets, annual and semi-annual terms before further analysis - this sound slike it is before their later HECTOR analysis - in which case the uncertainties from HECTOR will be under-estimated - they should be estimated within HECTOR.

The English is quite good but there are locations where it needs editing to make sure the meaning is intended. I indicate some of these.

Minor remarks: P1L1: GNSS in title but GPS throughout all the paper (and only GPS used in the analysis by NGL) P1L11: past changes in mass loading L13: deformation of the crust is the changing shape L20 and throughout: specifying velocities to 0.01mm/yr is not warranted by the uncertainties. suggest 0.1mm/yr L22: the WANG model does not over-predict but for the wrong reason (c.f Nield et al 2014) L24: Filchner L28: delete slower. Not clear how it influences "plate tectonics" L30: English

P2L9: delete "considering ...;" P13: effaciously - wrong word L15: "on a spatial scale" - English L19: not enough information to understand why Gaussian distribution is relevant L21: new paragraph at "Relative to PCA" L27: these are good refs for introducing the coloured nosie, but they are analysis of very old data - suggest something new like Santamaria-Gomez or work of Klos L31: what does complex terrain have to do with it? L31-32: English

P3L8: the two variants of IJ05R2 are described but only one is shown in the later analysis (which one?), L17: Could do with a summary (and reference to relevant paper of Blewitt or Kreemer etc) to the analysis. Note that the analysis uses the GMF for tropospheric zenith delay which makes the analysis sub-state of the art but probably not critical here (it will come into the common mode noise) L21: Argus et al 2014 and Wolstencroft et al mention the effect of ice or snow on the positions. These mainly affect the horizontal signals - I presume your editing here just considered the vertical component of hte coordinate? P4L8: the simulation approach is not clear and needs further description. I think the ICAs include coloured noise but did these simulations? What is the impact?

L17: the definition of 'residual' is not clear. I think this is different to the residual series in Section 2.1 L27: Bos is not an appropriate reference for AIC. Why not BIC also?

P5L15: is the comparison of before/after filtering using the same noise model or estimating a new noise model each time? L17: the approach to rejecting stations is not clear L20: here and after "variety" is not the right word. variation? change? Not clear what is meant by "Considering the elastic ... effects."

P6L5: Barletta et al Science 2018 is needed to be considered L7-14: needs an introduction to explain why these stations are discussed. Are these the ones with large differences to previous works? I presume these results are from teh filtered results? But is the difference due to the different data span or the filtering or both?

P7L1: see also the work of Schumacher et al GJI on the difference between ITRF2008 CM and GIA model CE L26: "greater uncertainty" - than what? L27: delete "the authors of"

P8L9: in what follows there seems to be a lot of discussion of the unfiltered results (discussing WM not WM* for instance)

P9L3: 79 stations was reduced to anotehr number due to rejecting some sites L14-

18: these statemetns need references. The mention of Thwaites having sub-glacial channels is not clearly related to the actual paper. L32: Nield et al 2014 is curiously missing here. Zhao et al EPSL is also missing for southern Peninsula. Nield et al 2016 GJI is missing for Ross Sea region

P10L16: the variation in accumulation could be important but the authors do not describe why. These should, in principle, be corrected in a robust elastic model

References:Wang reference has an issue with journal of geodynsmics and Nature Geoscience listed Nield et al reference needs space between rapid and bedrock Geruo et al suggests the author's family name is Geruo - it is actually "A" so this should be A et al.

Figure 2: it would be good to see the time series at a larger scale so we can see the detail.

Figure 4,8: the rainbow colour scale is now regarded as misleading - search online for #endrainbow

Figure 5: some of the smaller arrows are hard to see. Are coloured circles more useful?

Table 2: which 6G model? _C? Note there was a bug in some of the calculations for ICE6G but they were updated on Peltier's website

Matt King, Aug 19, 2019

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