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Interactive comment on "Differential InSAR for tide modelling in Antarctic ice-shelf grounding zones" *by* Christian T. Wild et al.

Laurence Padman (Referee)

padman@esr.org

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GENERAL COMMENTS

This paper describes the use of multiple DInSAR images to improve tide models in complex regions around Antarctica's coastline. The manuscript extends the work already presented in Wild et al. (2018; Frontiers in Earth Science), particularly by bringing in new data from GPS and tiltmeters on Darwin Glacier.

Overall, I found the work to be a valuable addition to discussions of tides in areas that are difficult to model, and the manuscript is mostly well organized and written. However, I don't think the authors are as clear about the problem(s) they are trying to solve as they need to be (see MAJOR COMMENT 1) and some areas of the text are too

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complex, jargon-filled or reliant on prior studies to be easily followed (See SPECIFIC COMMENTS).

– Laurie Padman

MAJOR COMMENTS

1. The authors need to more clearly lay out sources of errors in tides, before they get to Results. Looking at the Abstract, p.1, l.11, for example, the impression is that the improvement in the "feature-rich coastal areas" is somehow in the tide models themselves, whereas it is mostly just because the DInSAR-derived flexure model is being used to scale the tides. Part of this is deciding what you mean by "tide model" vs "ice surface height response to tides". Maybe a clearer sequential description of the issue is needed. It might be that you decide just to be specific about what you mean by "tide model"... "In this paper, we use the term 'tide model' to refer to ..." But then, when you define improvements that are mostly associated with taking flexure into account, you still need to emphasise that the improvement is because of the inclusion of flexure, not a true improvement in a tide model.

2. It was not totally clear to me where in the processing chain the IBE is applied. It should probably be treated like a tide; therefore, added to the model-predicted tide before scaling to GPS data and/or DInSAR alpha-maps. However, the authors also need to know that the IBE is *not* simply -1 cm per +1 hPa: there is a dependence on time scales and, also, the ocean physics of how the water moves around under forcing by surface atmospheric pressure. These issues are discussed a bit by Padman et al. (2003), but also do some searching on more recent IBE issues; e.g., "MOG2D" and Rui Ponte's papers.

3. As far as I can tell, the method used to optimize the tide "model" relies on scaling the total tide prediction (by the alpha-map) to optimally fit a GPS or multiple DInSAR fields. That may, in fact, be the best that's possible when the GPS record is short or given the limits on DInSAR availability. However, it misses the strong possibility that different

errors are present in different tidal constituents. For example, in the Ross Sea, O1 and K1 are both large (but also see the next comment). If O1 in the tide model is accurate but K1 isn't, then you get a different expectation between DInSAR double-differenced values than you get by assuming errors are the same, in fractional terms.

4. (a) The authors appear to treat K1 from 15 days of GPS as a reasonable estimate, noting that the short record just means you can't resolve the fortnightly tides. However, K1 is close in frequency to P1 (6-month modulation period), and P1 has an amplitude of 15-20% of K1. On 15 days of data, t-tide just finds a "K1" that is really "K1+P1". So, over a 6-month period, the "K1" tide from analyses of short records can vary a lot (by 30-40% from minimum to maximum) without the tidal physics actually changing. You can reduce the error from this by using inference, where the inference parameters come from a longer GPS or maybe your favorite tide model. But then you'd also need to include P1 in the prediction scheme. (b) The same issue applies to semidiurnals S2 and K2. (c) Also note that models can differ systematically between diurnal and semidiurnal constituents; i.e., one model might have good diurnals and poor semidiurnals, while another has the opposite.

5. These issues may help explain why different models best suit your needs for the model scaling. E.g., perhaps the best model (for diurnal-dominated Ross Sea sites) for DInSAR analyses is one where O1, K1 and P1 amplitudes are all "wrong" but highly correlated, so that the alpha-map scaling approach works well, whereas an overall more accurate model (for correlating with GPS) has compensating errors between different constituents.

6. It wasn't clear what happens to true tide-model *phase* errors, which could cause as big an overall error in DInSAR fields as you get from amplitude errors.

7. I was surprised to see the Minchew et al. (2017) Rutford study get so little attention here (p.2, l.31-34). It's true they needed a dedicated SAR "mission" to get an empirical model out of it, so it can't yet be applied everywhere. But it'd be good to have more

information on what they learned, e.g., “we’d need >100 DInSAR fields”, “accuracy of retrieved ‘total’ tide is less than we get from our approach”, “even with 100’s of DInSAR, can’t empirically determine tidal harmonics S2, . . .”

SPECIFIC COMMENTS

The authors might be interested in Appendix B in Rignot et al. (2000, JGR-Oceans), where Doug MacAyeal was, I think, the first to lay out the idea of inverting InSAR to tidal constituents.

p.2, l.34 to p.3, l2: I don’t like the structure, “Baek and Shum (2011) *failed* to . . .” Maybe invert the sentence, “Baek and Shum mapped the dominant tidal constituent (O1) in . . ., but data limitations prevented them from developing a full tidal model.” Note that even getting O1 right is valuable; it indicates ways in which ocean tide models need to be changed, and these changes will filter into other constituents. Also, *no* method (even yours) will lead to a “full tidal model”.

p.3, l.10-13: This misrepresents Stammer et al. (2014). The very small error (<1 cm) cited by Stammer et al. is for the deep ocean (nothing to do with ice), *under* the TOPEX/Poseidon and Jason satellite orbits, which were chosen specifically to resolve tides. (Padman et al., 2018) gives a summary.) The high accuracy comes mainly from assimilation of the high-quality T/P/J data sets, and only secondarily from the “physics” issues such as the long wavelength of tides in deep water and other issues like friction that are also more easily dealt with (or smaller) in deep water. Note that errors along the shallower margins feed into deep ocean tides in purely dynamic (“forward”) model solutions so that, without accurate data to assimilate, deep-water tides will be much less accurate.

p.4, l.9: It’s really unfair to cite Pawlowicz et al. for t_tide, but not to include “which is based on Foreman (1977).” Foreman did the harder part of the coding; to a significant extent, Pawlowicz mainly converted FORTRAN to Matlab code.

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p.4, l.27: “A tide model *must perfectly predict* . . .” Why? Clearly they don’t, even after tuning.

p.4, l.28-29: This cite to what Wild et al. (2018) did is too terse. At a minimum, at this stage we should know whether (i) the model is resolving individual tidal constituents, or just scaling a prediction of total tide, and (ii) whether just tide model amplitude is considered to contain errors (correlated between different constituents?) or whether possible phase errors can exist and be accounted for?

p.6, l.7-9: Need more details on t_tide analysis. Especially, tell us if you used inference to separate K1 and P1, and S2 and K2. See MAJOR COMMENT 4. An analysis of 15 days without inference usually predicts tides in the same 15-day window really well, but it cannot be used to extrapolate to other time periods.

p.6, l.25-28: As I read this, I didn’t understand why the Hillary comparison was called “close to” while the Shirase one was “slightly above”. Hillary is also “slightly above”.

p.7, l.7-9: See MAJOR COMMENT 1; This improvement is large, and impressive, and important to know about. But the primary reason for the improvement is taking into account the DInSAR-measured flexure, not what I infer from “tide model improvement”. It’s a terminology issue, but confused here because you talk about “scal(ing) the tide-model output” which implies that the tide model really refers to the model without ice mechanics taken into account.

p.7, l.24-26: This is a jargon-rich statement that really doesn’t tell me why discontinuities occur.

p.8, l.29 to p.9, l.1: See MAJOR COMMENT 4; You can’t directly compare K1 from a long record (say, >6 months) with K1 from a short record, unless you used a reliable inference scheme to separate K1 from P1.

p.9, l.7-9: This statement about orbit alignment with high tide might be true; I need to check it. However, typically the long-term diurnal variability is semi-annual.

p.9, l.20-21: From this I assume that phase is not considered in any of the processing chain taking advantage of tide models to improve interpretation of DInSAR. That's okay, but it needs to be much more explicit in the "Methods".

p.9, l.26-28: This statement needs to be defended with the right figure. You kind-of get into it in the next paragraph, but it should be an easy figure of "error due to viscoelasticity" vs "A dot".

p.10, l.9-16: This, for me, is very dense text and hard to interpret for my own interests. Can it be simplified?

TECHNICAL CORRECTIONS

1. The authors often hyphenate words that don't need it; e.g., "ice-shelf" and "tide-model". This makes sense in compound expressions like "ice-shelf height" and "tide-model physics", although even there it isn't usually needed. But it is wrong if used as in "the ice shelf moves . . ."

2. My preference is for using past tense for anything that *was* done: "We measured . . .", "We analysed . . ." etc. Then present tense for things that are determined to be true, e.g., "The ice shelf flexes with the tides . . ."

3. Authors routinely misspell "heterogeneity" and related.

4. Use of "Theta" for a least-squares adjustment value is confusing to me, since Theta is often used for *phase*. Why not "delta-h" or something else more related to height? If you continue to use Theta, then give the units (m) where it appears, e.g., legend on lower panel of Fig.4 and in Fig.A1.

Fig.1 caption: (a) This isn't really the "% displacement due to ocean tides", which implies, to me at least, that (e.g.) "30%" would mean that 30% is due to tides, and the other 70% is not-tides. I think you mean that it is the fraction of the tide-model signal that appears in the flexurally-constrained ice surface elevation signal. (b) Sentence starting "The GPS station" is not a sentence. (also two adjacent 'and's)

Fig.2: Why not use same colouring for middle panel as for top and bottom panels?

Fig.3: Why not use same colouring for top panel as for lower two panels?

Fig.5: Font size is incredibly small! First, eliminate all repeated text. All panels use the same Easting and Northing limits, so you can delete most of those, and close up the gaps between panels.. Then, colour scales and bars are unreadable. But they are same, right (?) so only need to appear once, presumably *off to the side of* the matrix of panels. And reduce number of color labels on the color bar to create some space. “A” values should have a white background: black on mid-gray is fuzzy. Same for SAR-pass IDs (1-12). Dashed lines and symbols are hard to see. Maybe, like the colorbar, put labels outside the matrix and have arrows pointing in to the sites on one panel.

Fig.7: Make the legend just single column. Then, add the lines in the order they appear in the processing chain. So . . . “Shirase GPS”, “Adjusted model for Shirase”, “Scaled model for Hillary”, “Hillary GPS”.

Fig.8: (a) since you mention “Diamond Hill”, point it out on the map. (b) Color bars: increase bar and text size; reduce number of color labels; provide a white background for the region of each panel devoted to the color bar.

Fig.9: All the same comments as for Fig.4.

(new figure request): See earlier comment, “p.9, l.26-28: This statement needs to be defended with the right figure. You kind-of get into it in the next paragraph, but it should be an easy figure of “error due to viscoelasticity” vs “A dot”.”

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