

General Impression:

Triggered by the initial observations from Gudmundsson in 2006 (Nature, doi:10.1038/nature05430) a number of papers have since investigated modulations of ice-stream flow with tides. So far, the observed amplitudes could be reproduced qualitatively but not quantitatively. Thompson et al. (The Cryosphere, 2014, doi:10.5194/tc- 8-2007-2014) suggested that a time-dependent variability in till strength (induced by variations of basal water pressure through tides) could improve the match between models and observations. This hypothesis is confirmed in this study. The results appear convincing and are presented clearly and succinctly. I am no expert in this specific area of ice-sheet dynamics and therefore mostly provide an outsider's opinion on how the paper could be improved; none of the comments strongly questions the derived conclusions. I congratulate the authors for a well-written paper which seems to wrap-up a long-standing questions why tidal signals are visible so far upstream of the grounding-line.

Comments:

- 1) Observations (thickness and surface velocities) are used in order to keep the model output comparable to the GPS data at RIS. However, it is not mentioned where the observations come from and what the potential errors are. This is mostly worrisome for the "observed medial line flow" (p. 2406, l. 7) which I assume stems from satellite observations using InSAR and speckle tracking. The "caution" that should be "exercised" according to Gudmundsson (Nature, 2006, doi:10.1038/nature05430) when interpreting these velocities is unfortunately not exercised here. This should be addressed because results (i.e. M_{sf}) are "sensitive to the mean velocity" (p2408, l.15). I suggest to discuss that satellite surface velocities are supposedly flawed to a certain degree through undersampled tidal effects and to make clear how this would imprint the inversion for c' (for example, what are the errors in the covariance matrix S_e (eq. 21)?; How does the "observed medial line flow" compare to the GPS observations?). I think this can be rapidly done and would correct what seems at current to be technical imprecise

The reviewer makes a very good point, there are certainly problems with the InSAR derived velocity in regions such as this where there may be issues with under sampling. The degree to which this is a problem is difficult to estimate without knowledge of the exact interval between satellite passes. A plot comparing InSAR velocities along the medial line with in-situ GPS measurements suggests that in general the satellite observations match closely, although a large mismatch of ~20cm/day is noticeable on the GPS site 20km downstream of the grounding line. Since we are only concerned with velocities upstream of the grounding line the data in our area of interest agrees quite closely with GPS measurements. We now make it clear in the text that these velocities are derived from InSAR and cite the appropriate source. We also mention the potential problems with this data and mention that the a-priori error estimate is larger to account for this.

- 2) Figure 3 is disconnected and not referenced in text. What happened here?.

A reference to this figure has been added to the text.

- 3) It is suggested that the M₂ amplitudes are “to small to be sufficiently resolved by the GPS receivers..”. This statement puts a lot of trust into the model and needs to be backed up with details about what type of receivers were used and what kind of processing has been applied. If this information is given in previous publications repeat the principal error estimates and reference them.

The error estimate for the original GPS measurements is between 2-5cm depending on the site, so could explain some of the discrepancy, but problems with the Maxwell rheology probably play a more important role. We feel that adding an entire section on the GPS data is unnecessary since it has been published several times before and this type of processing is very common. However this claim does need backing up so we have added a mention of the errors and details of the original data can be found along with a reference to a paper that described the type of processing in some detail.

- 4) Figure 5 indicates a GPS station at about 10 km upstream of the grounding-line. What is the reason for not showing this one in Figure 1?

We tried plotting this a number of ways but found that since the GPS sites at -20, 0 and 10km all show a very similar signal the figure becomes unclear if they are all plotted together in this way. We will add a comment in the caption explaining that the 10km site was not plotted for the sake of clarity.

- 5) A number of details and references are missing for somebody who would want to reproduce the results presented here. A list is given in the specific comments.

These are addressed individually in the specific comments.

- 6) A question out of interest: This study suggests that a strong modulation of surface velocities with tides hints to a highly efficient drainage system beneath the corresponding tributary glacier. Is the reverse also true, i.e. does the absence of a tidal modulation in ice-stream flow (as for example observed for the Ekström ice shelf by Riedel et al, 1999, *Annals of Glaciology*) indicate a dry (or at least hydrologically disconnected from the GL) bed of the tributary glacier?

Firstly care should be taken when looking at tidal modulation of an ice stream to first consider the local vertical ocean tide. In the case of the Ekstrom ice shelf the tidal range is far smaller than the region near the Rutford ice stream. In addition, the ‘stick slip’ tidal motion observed on Whillans ice stream shows tidal modulation of ice-stream flow without necessarily requiring an explanation based on subglacial hydrology. In general it would be hard to justify inferring anything on the nature of an ice stream drainage system due to the absence of tidal modulation in flow. Having said that, specific modeling studies of a particular ice stream’s response to tidal forcing certainly have the potential to answer these types of questions, and if modeling suggests that stress transmission alone can or cannot explain tidal observations on an ice stream then that may suggest that an ice stream is or is not overlying an efficient drainage system that greatly affects its flow.

Specific Comments:

p. 2399, l. 26: Missing brackets for the five citations.

Corrected citation format

p. 2502, l. 15: I suggest to more clearly specify “a Maxwell rheological model” with “an upper-convected Maxwell model”. A reference about this type of rheology and why it is used here would also be appropriate (maybe Gudmundsson 2011, sec. 3.1?).

Specified that we use an upper-convected maxwell model, and added a reference to Gudmundsson (2011).

p. 2402, l. 15f: Somewhere in this paragraph the exponent in the Glen flow law should be linked to n.

Added a mention of the exponent n in the description following Eq. 4.

p. 2403, l. 8f: “ice-stream” to “ice stream”

Done

p. 2403, l. 9: It is not expanded on how the till deforms in this model. Is it important?

The ‘till’ layer deforms in order to simulate ice stream basal slip as an implementation of the basal sliding law, it is not intended to replicate actual till deformation beneath an ice stream.

p. 2403, l. eq. (8): Provide reference for the assumed functionality of the basal velocity

Several references added in the text

p. 2403, l. 22: Define the coordinate system and sign convention more clearly. In de Fleurian et al. (TC, 2014, doi:10.5194/tc-8-137-2014, eq, 1) the signs are different ($N = -\sigma_{nn} - \rho_w$) which could be confusing for some readers.

The reviewer is correct to point this out, in fact our coordinate system is the same as in de Fleurian et al. and so there should be a negative sign in front of σ_{nn} . This has been corrected and is also now hopefully clearer in the text

p. 2406, l. 21f: what measurements is the thickness distribution based on? It is important for reproducibility, and to judge the following statements in that paragraph (e.g. “..bed undulates considerably..”). How was the lateral extent of the RIS defined?

We have added several sentences clarifying the source of the values (bedmap2) and commenting on how they were derived.

p. 2407, l. 10: Provide textbook (or paper) reference for the two analytical solutions

Reference added

p. 2408, l. 6: “ice-shelf” to “ice shelf”

Done

p. 2408, l. 6: Provide more detail or reference for “spring foundation”.

We have added a comment that more details can be found in: Rosier et al. (2014) doi:10.5194/tc-8-1763-2014.

p. 2409, l. 15: What values were assumed for S_e ?

The S_e covariance matrix is a simple diagonal matrix, with a fixed value of 0.2m/d along the diagonal. Although somewhat arbitrary, we choose this value, larger than the estimated errors given in the InSAR dataset, because the long period modulation due to tides has an amplitude of ~ 0.2 m/d near the grounding line.

p. 2410, l 14: What is the prior value and estimated error for the buttressing strength?

The a-priori estimate for buttressing strength is 500kPa and the error used in the inversion procedure is 1000kPa. We choose a high error because, as we mention in the text, this value is probably somewhat artificial, although a high buttressing may be expected due to flow constriction on the ice shelf downstream of the grounding line. We have added a brief comment that the a-priori error estimate for buttressing is chosen to be high for reasons given above.

p. 2410: A suggestion: Would it be informative to show the inverted, time-averaged basal slipperiness (for a given set of parameters) together with the medial line velocities?

We intentionally avoid going into any more details on the inversion procedure, such as plotting the inverted slipperiness, since this is not the focus of the paper and has been done many times before. The slipperiness that we use would not be useful to another study since we invert for medial line velocities but use the slipperiness in a 3D model, so there is no lateral variation in c' . Overall we feel that a large enough proportion of the paper discusses this aspect of the methodology and more details would detract from the main story.

p. 2412 l. 1: Stating “extensive parameter study” needs backup (i.e. move l. 12 to here for justifying that statement). Also include the stepsize for the respective parameter ranges

This has been re-structured as suggested by the reviewer, and the parameter ranges included.

p. 2412 l. 11: So the “decay length scale” is the constant in something like $\exp(-x/c)$? Because this is a major parameter for comparison later on it is helpful to be more explicit here.

The decay length scale is an e-folding length scale, in other words the distance for which the horizontal tidal signal decreases by factor e. This is now made clear in the text.

p. 2413 l. 1 : What was the a priori estimate of the buttressing strength?

The a-priori estimate of buttressing is 500kPa.

p. 2414 l. 4: Fig.1 does not specifically highlight the M2 amplitude. Make it more clear for the reader which wiggles in that plot you refer to.

We have added a comment in the text clarifying that the M2 signal is the higher frequency signal overlain on the long period Msf modulation.

p. 2414 l. 6: The suggested GPS measurement errors cannot be judged because details about receiver types and processing techniques are missing (see above).

See reply to general comment #3

p. 2415 l. 1: In the first run of this section the best match was found for $q=10$. I would naturally expect that a sensitivity study with respect to q should have $q=10$ at the center. Why was $q=1..10$ chosen?

This set of runs, where both q and m are varied from 1 to 10, is not a sensitivity study but investigates how the model responds to a change in the nonlinearity of both parameters. This is interesting because the observed Msf response is clearly nonlinear but the source of nonlinearity is not clear. Both exponents were changed from 1 to 10 purely because increasing the nonlinearity beyond 10 seems unnecessary since this is outside the range of parameters that we use.

p. 2418, l. 15: “ice-stream” to “ice stream”

Done

p. 2419, l. 5: “Martin” to “Mart\{i}n”

Done

p. 2419, l. 21 “Antasrctic” to “Antarctic”

Done

Figure 1: Include station at 10 km upstream the grounding-line (as done in Figure 5)

As mentioned above, this has not been included to avoid making the plot unreadable, we have added a comment to this effect in the figure caption.

Figure 2: Make that figure larger. Is the x-axis really pointing upstream? Seems counterintuitive to me. I do not understand what the “Clamps” refer to, they are also not further mentioned in text.

Added a note explaining what clamp refers to in the figure caption. Moved a few things around in the figure to maximise the use of space and enable it to be made larger. Corrected the orientation of both the x and y axes.

Figure 3: Is not referenced in Text. Labels should be increased in font size. What role do the tidal currents play? These are not mentioned in text. I suggest to include the hydrological head in this Figure which may be a good way to link it to the text.

The figure is now referenced in the text and it is made clear which processes are not included, both in the text and in the figure caption.

Figure 4: All labels are too tiny; “interpolatin” to “interpolation”; use (a) and (b) instead of “upper left “ and “upper right”; “Young’s modulus” to “Young’s modulus (E)”

Corrected spelling, made all labels larger and referred to letters rather than location in the caption.

Figure 5: The same way the range of M_{sf} amplitudes are shown for the nonhydrologically coupled case, it would be nice to show the spread (and not only the best fit) for the hydrologically-coupled case.

This definitely improves the figure; all hydrologically-coupled sensitivity study results have been added.

Figure 6: Insert “),” in front of “respectively”. Include the +10 km site to make it coherent with Figure 5?

Added missing bracket, model data at +10km not included for the same reason as mentioned previously.

Figure 7: All labels are too tiny for the TC layout.

Made all labels larger

Figure 8: All labels too tiny for the TC layout. Indicate for which location (10, 20 40,..km upstream of GL) that plot is made.

Made all labels larger and added comment in caption that M_{sf} and M_2 amplitudes are taken at 10km upstream of the GL.

We would like to thank the anonymous reviewer for their thorough review and highly appreciate the comments and suggestions that have helped improve the quality of the manuscript.