

Review of TCD 4, 2523, 2010, “Tides and ice-stream flow” by Gudmundsson

General comments:

This is an extremely interesting paper that addresses a phenomena that has been both puzzling to the understanding of ice stream flow and important to the evaluation of future ice-stream flow changes. The feature that distinguishes this paper from others on the same subject, and which motivates the publication of this paper at the highest level, is the fact that it addresses the “rheological participation” of the ice in a manner that was missing from previous study. The results of this study cast previous studies on more “sure footing” and provide an essential statement that the basic cause of the “long period tide response” of ice streams is indeed that described (stemming from properties of the subglacial deformable bed).

I think that any people working in ice stream dynamics and who must confront the fact that ice streams (and outlet glaciers of Greenland) are so sensitive to tide will find this paper very useful.

The two suggestions (to be considered as “optional” by the author) I have for possibly improving the paper are:

1. Can an experiment be done to show how the net overall discharge of the ice stream/till/ocean tide system varies with m and with the amplitude of the “tidal beat amplitude”?
2. While the viscoelastic treatment of the ice represents a very major step forward, it appears to have produced only modest changes in the results presented by previous models where the ice is treated more simply. This is an important reassurance and a very significant result. However, I would still like to know more about what the viscoelastic properties of the ice actually do to change (even if at second order) the response of the system. It would be useful to know what ranges of phenomena (in a general hand wavy sense) or what ranges of parameters (again, no serious parametric study is required) would make viscoelastic properties of the ice sufficient to either enhance or deny the basic phenomena of ice-stream tide (or other wave) interaction.
3. The term “long period tide” has various meanings in oceanography, e.g., strictly it is a tide that is generated by a potential that lacks longitudinal variation (see the citation to Wunsch given below). However, I think that the main issue in this paper is the response of the ice stream to “spring-to-neap” beating between diurnal and semidiurnal constituents that have frequencies close to each other. It might be worth making sure that the various distinctions are clarified so that misinterpretations by oceanographic readers are not made (e.g., an oceanographer could conclude that modeling some very small long-period tide would be a thing that a glaciologist would want... but the results of this study suggest that having M_2 and S_2 modelled well would be enough.).

Specific comments:

5: in the first line, it might be worthwhile emphasizing that it is horizontal flow that is reacting sensitively to tides... often a casual reader will immediately associate tides with vertical motion and might mistakenly think that some sort of vertical flow is being influenced.

5: Also, in the case of the Brunt et al., 2010 reference, the tidally induced variations of flow have been detected ****100's**** of km downstream of the grounding line, and the increase in flow speeds reported are on the order of 100%; thus it may be worth mentioning that variations are detected both upstream and downstream. (In addition or alternatively, it may be worth a rewording of what is currently written: "...causing and increase of up to 10 to 20% in flow speeds..." might be more accurately stated as "...causing a periodic variation in flow speeds of up to 20%..." . The idea is that the tidal influence is not a "increase" only, but is a time-varying influence that increases for ½ the cycle and decreases for the other ½ of the cycle. But, having said this, it may be that the presence of the tide does indeed increase the net time-averaged flow that would exist if the tidal forcing were absent. If this is what is intended to be said, then a new sentence should be added following this one to make it clear that there are two distinct ways in which the tides are influencing: both a time-dependent cyclical variation and a "time mean" additional effect... But I'm not sure that this is what is intended.)

10: This paragraph only offers a rather cursory example of why the variations are interesting. I would revise this paragraph to say that the observations are intriguing for a variety of reasons, all of which challenge our ability to theoretically characterize ice-stream dynamics: First, the variations in stress that tides induce downstream of the grounding line constitute natural stress-variability that gives an interpretation of the general question of how ice streams are sensitive to "backpressures" at the grounding line. Second, (and this would be the place where the Sergienko et al reference could be made) some of the more exotic behavior, e.g., the stick-slip type flows of a small subset of the ice streams that are influenced by tides make specific implications that basal tills are strain softening materials (an additional reference could be made to experiments conducted by Neal Iverson and reported at AGU: C32A-05 TI: Stick-slip Motion of Whillans Ice Stream: Experimental Constraints on Till Frictional Behavior (Invited) AU: *Iverson, N R.) Third, tides are a variable dynamic of the ocean which respond (in amplitude and phase) to changes in ocean geometry such as imposed by the advance or retreat of ice around the continental shelves of Antarctica; thus, a legitimate question will be whether tidal variability induced by ice-sheet changes can have a positive feedback on ice-sheet flow. Fourth, it is essential that glaciologists figure out if the time variability introduced by tidal forcing leads to larger ice-stream cross grounding line discharge than would otherwise occur if the oceans were absolutely tide-free. Fifth, the influence of tides being recognized easily in observations, requires explanation so that the less easy to observe influences of other time variability in the ocean (e.g., longer period variations or short period swell, etc.) can be put into context, e.g., is it important or not?

20: It might be worth adding that the definition of “long period tides” also involves the fact that they stem from tide-generating potential fields that lack sectoral or tesseral variation (i.e., the only variation is in latitude, there is no longitudinal variation of the tide-generating force). I remark that I am not a tidal expert and thus defer to the author on the best conventions for nomenclature... But the idea of a diurnal or semidiurnal tide has much to do with the fact that the earth is spinning through a “lunar fixed” or a “solar fixed” tide-generating potential that has longitudinal variation. (Note a good reference for the long-period tides is this paper: Wunsch, C., 1967. The Long Period Tides. *Reviews of Geophysics*, Vol. 5, No. 4, pp. 447-475 doi:10.1029/RG005i004p0047. This paper begins with a definition of long-period tides that involves the fact that there is no longitudinal dependence of the geoid disturbance by the sun and moon.) Having said this, I wonder if the parenthetical remark in this paragraph about what the long period tides are is really appropriate, as I think that the author means any tidal-driven periodicity that has a period greater than the longest diurnal constituent, and I suspect, but am not sure (and this can be clarified here in the paper) that “tidal beats” (e.g., spring to neap cycles, etc.) are considered long-period modulations that the ice streams are responding to.

So, the above long-winded paragraph asks: is “long period tide” meant in a strict geodetic sense, or is it meant in a “common sense” sense, i.e., simply distinguished by the period of the cycle?

25: “For example a tidal analysis of a 55-day GPS **vertical displacement** record...” At some point, possibly here, it is important to remind the reader that the vertical motion of a GPS deployed downstream of a grounding line (on floating ice) is generally thought to be a faithful representation of what the ocean free-surface height is doing in response to the tide. And additionally that these vertical GPS measured motions are to be distinguished from the horizontal GPS measured motions, which are the “intriguing” signal of the ice-stream’s horizontal flow response to the tide in the ocean (to the ocean’s free surface motion). It may not be immediately clear to some readers that we glaciologists generally use the vertical GPS data as a faithful measure of tide elevation, but the horizontal GPS data is the surprise that this paper is concerned with.

25-5: The following comment is a matter of taste and can be ignored if desired: I perfectly agree with and understand the statement: *A linear system, when forced over a given range of frequencies, will only produce a response at those same frequencies. RIS responds strongly at frequencies absent in the forcing, a clear evidence for some sort of a non-linear system response.* However, one could argue that the RIS is actually responding at frequencies that are indeed in the forcing, but that are simply not measurable with a 55-day GPS vertical displacement record. The implication is that the RIS response is still that of a “linear system”, however the system is **resonant** at the frequency of the otherwise imperceptible MSf and Mf tides in the ocean. A linear system such as a harmonic oscillator with a natural frequency of ω , when forced with a forcing at γ , will respond with an amplitude gain

that is $1/(\omega^2 - \gamma^2)$ or something to the effect. What is strictly non-linear is the relationship between the amplitude of forcing and the amplitude of response, not necessarily implying that the system-dynamics of the oscillator are themselves nonlinear. Of course, the system considered in the study, ice forced by a tide in a shallow sea, is very different from a simple oscillator. Nevertheless, what I think I'm arguing correctly is that a linear system that is a harmonic oscillator can have a non-linear amplitude response to forcing.

Another point to raise in the above comment (but again, I suggest can be fully ignored, as it is a "fine point" that is likely of little interest to many readers). At very low frequencies in the ocean, waves tend to have more kinetic-energy than potential energy because of geostrophy, thus it may be that a very insignificant vertical tidal amplitude at Mf or MSf might be associated with a horizontal current that is unusually large (probably I'm wrong in saying this, but worth checking). This means that possibly horizontal currents in the ocean at low frequencies are forcing the ice stream response inland... the paper cited above by Wunsch seems to talk about this a bit in the Summary section.

Having stated the above, relatively "pedantic" fine points, I note the fact that Figures 1 and 2 show absolutely stunning results: that the long-period motions of the RIS far outweigh the motions associated with periods at M2 and other short-period, strongest amplitude tides.

2527-25: How does the phase shift vary upstream relative to a diffusive system? Are the phase shift migration speeds at different tidal frequencies consistent with a diffusive propagation of the influence upstream? (no need to answer). Is the term "phase velocity" appropriately used in physical systems that are diffusive? I guess I'm very used to seeing "phase velocity" used in seismology or surface-gravity wave dynamics and am surprised to see it used here before I have been told that the ice/till system strictly allows some kind of wave propagation.

Equation 7: It might be helpful (certainly to me, who has never worked with viscoelastic dynamics before) to see how the "upper convected time derivative" is written for τ_{ij} , i.e., a specific ij term of τ . The expression in equation 7 can be worked through to determine this, however, it would be good to see it explicitly, because the upper convected time derivative operates on a τ_{ij} in Equation 4. (OK, now I see that equation 8 restates equation 4, but this might still be helpful).

2533-15: a comma is missing after Leysinger.

2534-5: Does the "spring" cause the floating part of the system to bob up and down with a buoyancy determined frequency (e.g., as described by Swerdtfegger in *Annals of Glaciology* Vol. 1)?

Results and Discussion section: I'd suggest re-organizing into three separate sections (to be more effective in describing what it is this particular reader was most interested in knowing). First, have a section that displays what is necessary to gain the "main effect" of the model, i.e., the high amplitude of the ~14 day cycle (I

take it that $m=3$ is the main aspect of the model that allows this, and that all other aspects are subservient details, which incidentally is a nice affirmation of the previous papers on this subject). Second, I'd have a section on "what is it about $m=3$ that makes this system work?" Possibly this may be a reiteration of what has been said in previous literature, but still useful for this paper. Third, I'd get to the question that I personally have wondered most about: "What is the effect of the viscoelasticity of the ice?" or even "what is the effect of the ice?". The answer to this question currently is scattered in several places in the results and discussion (I take it that the viscoelastic part of the ice simply introduces another time scale that modifies upstream phase migration??); however, I'd like to suggest that it be a major discussion point, as it represents an evaluation of what is unique and pioneering in the model that was created for this study. After reading the Results and Discussion, at this stage, I can't really say whether or not I have a good idea about what the extra effort to treat the ice actually accomplished (and if it did not produce significant differences, it would be interesting, if time permits to know under what other circumstances, parameter values or time forcing periods the viscoelastic properties would have greater bearing on the outcome).